"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . Admirationis generat questionem, quæstio investigationem, investigationis inventionem."—Hugo de S. Victore.

—"Cur sprient venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscera Phæbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina æco,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.
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### ERRATUM.

Page 305, line 6, for object-end read object and.
PLATES.

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II. Illustrative of Dr. Herapath’s Paper on the Discovery of Quinine and Quinidine.

III. Illustrative of Sir David Brewster’s Paper on the Optical Phenomena and Crystallization of Tourmaline, Titanium, and Quartz, within Mica, Amethyst, and Topaz.


V. Illustrative of Dr. Barry’s Papers on a Main Cause of Discordant Views on the Structure of the Muscular Fibril; and Further Remarks on the Muscularity of Cilia.
I. On the Tertiary Beds of Hordwell, Hampshire.

By the Marchioness of Hastings*.

During the six years that I have resided in the immediate vicinity of the Cliffs of Hordwell, I have been enabled, through the extreme kindness and liberality of Dr. Chambers, who has allowed me every facility for pursuing my investigations, and to whom these cliffs belong, to make them and their strata the object of very minute researches. Their nature is such, that any one, to become thoroughly acquainted with each deposit and its contents, must have daily opportunities of examining them.

The top is composed of a bed of gravel, from 15 to 20 feet. This and the whole portion of the upper part of the cliff are perpetually falling, and covering by their descent all the lower strata; or at least coating them so with the various upper sands and clays, that unless the surface be laid bare by digging, it is impossible to ascertain the true nature of the strata beneath.

There have been many opinions as to the natural causes which lead to the rigid decay of these cliffs: the sea has been generally accused of being the originator of all the mischief, and certainly it encroaches considerably every year.

But it is now generally acknowledged that the inland springs do quite as much damage, and that their inroads are more rapid on the upper part of the cliff than those of the sea on the lower.

There are seldom falls in the summer or in the dry weather; and from my own observations, I am inclined to believe that the following is the way in which the springs of fresh water act upon the cliff.

In the winter, during heavy rains, they become much swollen,

* Communicated by the Authoress.
and make for themselves internal channels, thus creating a dis-
jointing of the earth and strata. These channels, during the
summer, or in frosty weather, become dry; but on heavy rain
or thaw occurring, they become again filled and widened, and
the portion thus severed from the surrounding earth, falls when
it is least expected, from external symptoms.

In this month (August 1851) a portion which projected like
a buttress from the face of the cliff as much as 15 feet, and which
was about 50 feet in length, fell, displacing the lower or under-
cliff, and the shingle and sand for some 150 yards. The masses
which fall from above, act as wedges, and force up the under
part of the strata many feet higher than their original position.

Mr. Prestwich, in his most admirable and valuable paper,
hardly notices the freshwater strata of Hordwell. People go to
Alum Bay, remain there for weeks, nay months, examining the
whole of the series of strata so beautifully developed there and
at Headon Hill. These have been repeatedly figured and de-
scribed, but I am not aware of any figured section of Hordwell
having yet appeared.

Having so long resided in these parts, I have devoted a very
large portion of time and attention to the study of these strata
and their contents (of which I have a very large collection); I
have coloured a section of them on the spot, after carefully
uncovering in various parts and measuring, as well as their
varying depth will admit, every stratum. I have hazarded no
supposition; all the information contained in these pages is
derived from my own personal observation, and facts known to
myself; and they are written with the hope that they may be
useful to those who wish to learn the nature and position in our
tertiary series, of these strata, and who may not have had the
facilities which my long residence in their neighbourhood has
given me, for acquiring such knowledge.

As my much-esteemed friend, the late Mr. Dixon, remarks in
his work on the Geology of the Coast of Sussex, local descrip-
tions are those of which we stand the most in need; by com-
paring these, when published, we may in time arrive at some more
accurate knowledge of the relative positions of our tertiary strata.
The strata here are most varied, and very clearly defined when
uncovered.

They have few or no faults, and their contents are so various
and different, that it would be very difficult, when once thoroughly
acquainted with one stratum, to mistake it for another. They
dip gently to the east, and their entire length, from the spot
where they rise to where they disappear at the top of the cliff,
is about a mile. Those composed of sand vary much in colour,
as shown in strata 13 and 17, which makes it difficult to give a
perfect idea of them in a coloured section on a small scale.
They are very level; the only striking exception occurs at Mead End, where the lowest of this series rises, and where there is a sudden dip in the 16th and part of the 15th and 17th strata. The under part of this dip in the 17th stratum is filled up by a very bright green clay, filled with veins of the dark-coloured clay above, and containing the same shells. I was particularly fortunate in finding this portion of the cliff uncovered in a way which had not occurred for four or five years. The sea on this coast, sometimes for a week or ten days together, sets in to one particular spot on shore, that it may not have touched for years, and washes away shingle and cliff for many yards.

It is difficult to account for this, unless it be that the shifting of the enormous bed of shingle in the middle of the channel between this coast and the Isle of Wight alters the direction of the under currents, which are very strong, as well as the tide on this coast.

As it would be difficult for strangers to find the exact spot where these beds crop up and where they disappear, I will merely remark that the whole that are described here, occur within a distance of 2 miles from their commencement, to Mead End about a quarter of a mile further than Hordwell House (Dr. Chambers'). I have not carried my description further, as these constitute properly the Hordwell or freshwater series. I have divided them according to the different character of the deposits and their contents.

Stratum 1 varies from 6 to 8 feet in depth. It is composed of grayish sand in bands with a yellow sand tinged with iron. It does not dip quite so much as the others, and runs perhaps a trifle further.

The upper and principal part of the bed is entirely barren in fossils; but about half a mile from the spot where this stratum crops up, a small band of about 4 inches of dark-coloured clay appears at the top nearly of the bed, which contains many remains, though generally much compressed and in a very brittle state. Small jaws of Rodents, portions of the carapace and plastron of Emys, teeth, and occasionally many broken bones of Crocodiles, vertebrae of Serpents, and rarely the teeth and bones of Mammals, are found here. Remains of Trionyx, so common in the lower beds, are not met with in this. Beneath this band is an imperfectly formed limestone band, about 5 or 10 inches in depth, consisting of crushed Limnea and Paludina. The whole of these narrow bands run for about 300 yards, and then disappear. They are generally so entirely covered by the sands of the upper part, that unless dug for, they cannot be found. They are easily worked, but the larger bones and teeth are always in a very brittle, bad state. Just before the commence-
ment of these small bands was the end of the upper marine, or—

Stratum 2, which is now nearly worked out, as it does not continue in a horizontal direction, so that unless previously known, it would be very difficult to find it in its original position. Its depth was from 10 to 12 inches. It rose with the first stratum and lasted for about 300 yards. I myself have but a small portion of the shells found here, as now they are difficult to be met with; and though I do possess shells from every stratum, my collection has been chiefly confined to the remains of the Vertebrata. I see in a list copied from Mr. Edwards and Wood, Limnea and Planorbis put down as found here. I have never found or even seen one of these shells from this bed, which is a true marine one. It consists of light yellow sand. The band mentioned as occurring at the top of the first stratum comes in after this disappears, and is undoubtedly freshwater. Among the commoner shells in this bed may be mentioned Cerithium cinctum, Neritina concava, Fusus labiatus, Melania fasciata and costata, Melanopsis ancilaroides, Natica depressa, and Cyrena obovata.

Stratum 3 is from 4 to 5 feet in thickness. It is composed of a lilac or gray clay with bands of buff. Very narrow bands of lignite traverse this bed; the thickest occurs about 4 or 5 inches above its base. Its chief characteristic are the layers of Unio Solandri, which begin about 4 inches from the top, and go on increasing in numbers to nearly the end of the bed. They are in beautiful preservation, though from their brittle nature it is very difficult to get them out in a perfect state. Associated with these are Paludina lenta, and principally towards the bottom of the bed Melania polygyra occurs.

Within a foot from the base, about 4 inches apart, are two layers of Melanie in a very perfect state; and resting on the green clay which forms the next stratum, is a bed of seeds of Carpolithes, &c., with but few of the shells which occur in the preceding layers; Paludina lenta being the principal, as well as Potamides cinctus.

No remains of vertebrata are found here.

Stratum 4 varies from 10 to 12 feet, though it is generally very level. It is composed of a green marly clay, traversed by sandy seams. The clay is darkest at top and bottom, and becomes more sandy in the middle. The sandy seams contain Paludina lenta and Potamomya plana very much crushed; remains of fish, and very rarely Serpents’ vertebrae, fish scales, and small teeth of Mammals undescribed, with also very small jaws. I have a large calcancem found here, the only large bone that has ever been met with. The last foot of this stratum contains
in the sandy seams layers of *Uniones*, *Paludina*, and seeds which are very perfect, and flake off in large masses. At the base of the stratum, at about half of its length, is a small layer of *Limnea* and *Cyclas*, which only extends about 2 yards in length, and is rare to find uncovered. Pebbles of pyrites, from which vitriol is procured, are found at intervals in this bed. There are no remains of Trionyx, Emys, or Crocodile found here.

*Stratum 5* averages from 4 to 6 feet, and is a very well marked and interesting bed. It can be traced for a very long distance in the cliff; and in clear weather with a glass, I have seen it when many miles off at sea. It commences with a very narrow band (about an inch) of lignite. After this there is a band of gray sand of from a foot to 18 inches thick, with no fossils. Then there is a band of dark gray clay, containing seams of lignite of 18 inches, resting on these seams; in the clay are found *Paludina*. This stratum ends with 18 inches of gray sand, very variable as to darkness. The first 6 inches under the clay and lignite contain layers of *Uniones*, *Paludina*, *Potamomya* and lignite, *Limnea* and *Melanopsis brevis*, occasionally but rarely the *Potamides cinctus*, with seeds of Charæ and broken portions of the carapace and plastron of *Trionyx*. No other animal remains are found here. The same small layer of *Cyrena cycladiformis* and *Limnea fusiformis* and *arenaria* which occurs at the base of No. 4, occurs here also for the space of about 3 yards. This is the last stratum in which *Unio Solandri* is found.

*Stratum 6* averages from 15 to 20 feet in depth. It consists entirely of blue mottled marl coloured with iron, and a vein of lignite not more than an inch in thickness running through the bed nearly at the middle. It is utterly barren as to fossils, except specimens of *Paludina lenta*, which are very rarely met with.

*Stratum 7* averages from 4 to 6 feet. It commences with a very narrow band of lignite, resting upon about 3 feet of green marl slightly mottled and covered by iron. After this there is a small band of lignite of about 6 inches, which only comes in occasionally, and under it a band of sand of about 8 inches, which also sometimes disappears; after this about 18 inches or 2 feet of green clay mottled with iron. The whole stratum is barren as to fossils.

*Stratum 8* is small, varying from 3 to 10 inches of Limnean limestone, composed apparently chiefly of crushed and broken *Limnea* and *Planorbas rotundatus*; occasionally, but very rarely, a perfect specimen of either of these is found, but no other shells and no animal remains; no fish or seeds are found here. When this stratum is first uncovered, it is in a perfectly hard state, but
on exposure to the air it becomes friable and quickly falls into pieces.

_Stratum 9_ is about 6 feet in thickness. It commences with a very narrow band of lignite; after which is a band of clay, green mottled with iron, through the base of which passes a band of ironstones of about 10 inches in thickness which project from the cliff. Under this band appears occasionally a narrow seam of limestone, succeeded by a narrow band of green clay, containing _Paludinae_ and _Potamomya_ very much crushed. No other remains or shells are found here. There is an idea prevalent that the ironstones which run through this bed are the result of exposure to the atmosphere. This is utterly fallacious; I myself have seen this layer of ironstone uncovered in a horizontal direction for 8 feet beneath the surface, finding them there in the condition precisely as at their first appearance on the face of the cliff. Generally they occur in a regular line, extending horizontally in large masses of many feet, but sometimes they are replaced by the clay.

_Stratum 10_ averages from 8 to 9 feet. It consists of fine white sand traversed with bands of gray marl. The centre of the bed is nearly pure white sand. The gray seams of marl prevail at the top and at the base, which consists of them principally, and which is about 2 or 3 feet in depth.

The whole of this stratum contains seams of _Limnea, Potamides_, and _Melania_; but the lower part contains beds, barely an inch apart, of _Potamomya angulata_, with innumerable seeds of _Chara_ and _Serpula tenuis_ (found only in this stratum), which flake off in large masses after any portion of the stratum has fallen. About 3 feet from the top is where the animal remains are principally to be met with, immediately under the gray bands which traverse the upper part of the bed. Associated with them are _Potamides, Planorbis euomphalus_, _Limnea, Potamomya, Dreissena Brardii_, and _Paludina lenta_. This and strata 5 and 11 are the only beds where the first mention of these shells is found, except very sparingly, and stratum 10 is the only one where they are found in any abundance. They invariably accompany animal remains, and whenever they are found we know bones are near. The sand immediately surrounding the bones becomes of a greenish, dirty, clayey nature, giving evidence of decomposed animal matter. It is rare to find these remains near the commencement or end of the bed. The middle, a little to the west of Hordwell House (Dr. Chambers’s), is the locality which I have found the most productive. Unfortunately, the time of our British Cuvier—Professor Owen—is so much occupied, that many of the species found here are undescribed; I can only give a general idea therefore of its
contents. Of Mammalia, we have Paloplotherium: a fine series of jaws and bones is in my own collection: and a gentleman who resides close to these Cliffs, and who has been indefatigable in his researches, Alexander Pytto Falconer, Esq., who was the first discoverer here of this new species of Mammal, as well as of the Dichobune, has found many other specimens. Hyænodon (one unique lower jaw in my own collection) is likewise met with.

I have two different humeri, coracoids, and various other undescribed bird-bones. Of Reptiles, there are Crocodiles Hastingsæ, of which I have two entire heads, a series of 80 or 100 vertebrae, the same number of humeri, femora, &c.

I believe an alligator has been likewise found in this locality, by Mr. Searles Wood, whose name is well known to the geological world.

Trionyx Henrici, Barbara, marginatus and circumsulcatus were found here, and are in my Cabinet; also Emys crassus, and a new Emys, which I have recently discovered and restored to a nearly perfect state, having plastron and carapace entire, which is undescribed and unfigured.

The remains of Lepidosteus abound in this stratum and are found throughout, but in most abundance with the bones. Generally, where the remains of one animal exist, others are found associated with it, and in a space of six yards, remains of all the above-mentioned animals may be found. The geologist will be deceived, however, if he expects to find the vein of good fortune continue. After finding a fertile spot of this sort, hundreds of yards may be examined, and nothing but a few shells, fish scales, seeds and dermal plates of Crocodile, which are most abundant, be met with. The stratum is singularly uncertain in this respect. It is a very difficult one to work, for many feet or rather yards of strata above, including the layer of ironstone, have to be removed before enough of stratum 10 can be uncovered to have a chance of meeting with anything in it worth finding. The remains are in a most beautiful state of preservation.

Stratum 11 varies from 1 to 2 feet. I have separated it from the preceding, as its contents give evidence of having been deposited under a very different order of things. It consists of very pure white sand, with very rarely a coloured line of ferruginous sand running through it. It contains numbers of seams of crushed Potamomysa lying close to one another, and with them are found tolerably plentifully, rolled bones of Emys, Trionyx, Crocodile, Rodents, Fish, Vertebrae, Birds and Mammals. Occasionally, but very rarely, a perfect one is found. The most perfect specimens are teeth of Mammals, and these
are sometimes met with nearly perfect; all the other bones are much rolled, and hardly recognisable.

*Melanopsis brevis* occurs in this bed.

*Stratum 12* averages from about 1 to 2 feet. It consists of green clay. There is near the top a seam of Potamomyza, and occasionally some of these are found scattered through it, but no other fossil remains are found in this bed.

*Stratum 13* varies from 6 to 8 feet in thickness. It is a bed of white sand, which is very variable in colour. As it rises it is a deep iron-gray, and continues this colour for some hundred yards. As it proceeds westward, it becomes a buff tinged with iron. There is, near the commencement, a small band of green clay running through it, about 2 inches in depth, but this disappears. *Potamomyza, Paludina*, seeds, wood, and leaves (like flags traversing the bed in all directions) are found here; very rarely *Planorbis rotundatus* and *platystoma* and *Limneus longiscatus*; but no animal remains have ever been met with but once. Very far to the westward, at the base of the bed and near its termination, a series of about thirty bones, evidently belonging to the same animal, a Palæotheroid, were found in the space of a few yards. They consisted of the ungual phalanx, two metacarpal, and a carpal bone, an astragalus, the trochanter, and part of pelvis, rib bones, vertebrae, &c.,—all strongly impregnated with iron. A stone containing much vitriol is found here, in masses like scorizae. At the base of the stratum is a small layer of stone about 2 inches thick, from which some pigment or colouring material is prepared.

*Stratum 14* varies from 2 to 4 feet in depth. It consists of an iron-gray clayey sand, which towards the base is coloured with iron. It is full of leaves and fruit, which generally are found lying in a horizontal direction throughout the bed; but the stems, of which there are an immense number, intersect the bed perpendicularly, or at right angles in every direction. There are several kinds of leaves and fruit, but I am not aware of any named list of them having as yet been published. It would appear possible, from the position of the leaves and stems, which is what one observes in stagnant pools of water, that they had originally grown where they are now found in a fossil state. The depth of the bed is quite sufficient for this to have been the case, and had these plants been brought to this bed by a running stream of water, they would not have been found in the same relative position as they are.

*Stratum 15* is the last of the purely freshwater strata. It varies from 20 to 25 feet in depth, and is the most productive in animal fossil remains of the whole series. Its deposits are very varied and well defined. It commences with a band of
green clay of about 4 feet, containing much wood and flags intersecting each other at right angles, and also standing nearly perpendicularly, mottled with iron, barren of shells, but in which are found remains of Mammalia. I have an under jaw, much crushed (as indeed are all the remains found here), of Anthracotherium, and like portions of under jaws of Paloplotherium. This band is followed by a band of white sand, which after about 6 inches becomes mixed with green clay for about 2 feet. No shells are met with in either the sand or clay; but remains of Mammals tolerably well preserved occur in the sand. After these two bands, succeed three, sometimes four layers of ironstone nodules 6 and 8 inches apart and about 2 inches in depth, which extend horizontally, and are about 2 or 3 feet in breadth. The space between these is filled up by white sand, which contains layers of Paludinae and Potamomyæ. Associated with these are found Trionyx, Emys, broken jaws of Mammalia, with teeth of ditto, fish vertebrae and bones, and occasionally bird-bones, and very small jaws, but no Crocodile. Remains are not common in these layers of sand, and they are very difficult to work, owing to the ironstone. The most likely time to find anything, is when a very rough sea has washed out the ironstone, and thus uncovered the beds of sand between. After this, there is a band of sands, which at their top and base are thickly traversed by bands of clay, of a light green colour. There is one rather thicker band of clay, about 18 inches from the top of these sands; and lying upon this and the preceding layers, are found all the same remains which occur in the sand between the layers of ironstone. In the white sand immediately after this, are found layers of Paludinae and seeds (with rarely Planorbis platystoma), of about from 2 to 12 inches thick, which also contain Trionyx remains, but no other. The sandy clay which comes after this is productive of all the best bones which are found in this stratum. I have from here a tooth of Anoplotherium commune, and a scapula, two tibiae, two humeri, metacarps, &c., which are unnamed, but which I believe to be Anoplotherium; a series of jaws of Paloplotherium, remains of Crocodile, Trionyx, Birds, and a very large unnamed fish, and Emys. Just before the end of these clays, comes in occasionally a small band of broken shells tinged with iron (and much resembling in substance, only harder, the limestone found at the base of the small band in stratum 1, which contains Serpents' vertebrae, &c. from 2 to 6 inches in depth, in which are found Serpents' and Lizards' vertebrae, teeth of Mammals, jaws of Rodents, fish scales and vertebrae, Crocodile, Trionyx and Emys remains, and occasionally large and perfect bones, such as astragali and carpal
bones. After these clays, succeeds a layer of ironstone from a foot to eighteen inches in depth, and extending in triangular masses of 4 to 5 feet in a horizontal direction. It is not continuous; sometimes several feet will occur without the stones, which are replaced by the same green clay as immediately above. From these to the base of this stratum are dirty green clays, mottled with iron; immediately under the ironstone is a small layer of white sand, which contains much the same bones as are found in the small band of crushed shells lying immediately above them,—Trionyx, Emys, Crocodile teeth, Serpents' vertebrae, Birds and Fish bones. No large Mammalian bones are found here. Potamomya and Paludina are found with them. A small band of lignite runs through the clay about 8 inches after this band. The clays underneath are full of crushed layers of Potamomya, with which are found in a very rotten state, pieces of Trionyx and Emys: crystals of selenite are met with in this bed only.

Stratum 16 is the first of the fluvio-marine series. It varies from 4 to 5 feet in depth, and commences with a band of lignite so lightly carbonized that it burns like coal. The depth of this is about 18 inches. This is succeeded by a stratum of very dark mottled green and gray clay, about 2 or 3 feet in thickness. Then there is another narrow band of lignite of about 4 inches, and under that, 4 to 6 inches of the same clay as above. These clays contain immense numbers of Potamomya much crushed with other shells. The Neritina is only found here and in stratum 17, as well as in stratum 2, or the upper marine. No vertebrate remains whatever are found here. The whole of this stratum (and the lower part of stratum 15) makes a sudden dip near its commencement. The green clay which fills up the lower part of this dip, is curiously mottled with the gray clay immediately above, and the gray portions contain the same shells as the clay above. As this dip occurs at high-water mark, it is generally covered by shingle, but the sea having set in at this spot for above a week in this month (August 1851), has carried away many yards of shingle and cliff, and laid bare, for the first time for years, this singular variation in the stratum. The first week in September I visited this spot again, and in ten days, the whole, to the height of 6 or 8 feet, had been entirely covered with beach again.

Stratum 17 is about 20 feet in thickness. It consists entirely of sands very variable in colour. It rises about one hundred yards east of Mead End, but owing to being generally covered with beach, is seldom traced there. Its colour on rising is a greenish gray very much like the clays above, but it soon alters
in colour, and becomes lighter and lighter, till, after it has passed Mead End, it becomes a light yellow (for about fifteen or twenty yards, when it will change again to gray, and so on to the end), striped with bands of iron-coloured sands with little patches of gray in it, which are full of shells. The first five feet of the sands are full of shells not in seams, but this depth diminishes till at about three-fourths of its length, the fossiliferous part runs out altogether. Teeth of Lamna, Myliobatis, Ætiobatis, remains of Crocodile, Trionyx, and Chelone are found here, but in so very brittle a state that they are difficult to save. I have, however, one beautiful small Trionyx (rivosus) found here, and my Collector has found bones of Mammals, but too rotten to preserve. The shells found here are clearly fluviomarine, consisting of Potamides, Potamomya, Natica, Ampullaria, and Bulla, &c.

This ends the Hordwell or freshwater strata, the next bed being the first of the Barton or marine series.

II. On some new Methods of forming the Nitroprussides. By Edmund William Davy, A.B., M.B.T.C.D., Lecturer on Chemistry in the Carmichael School of Medicine, &c., Dublin.*

SINCE Dr. Playfair's discovery of the nitroprussides in the year 1849, I am not aware that those interesting compounds have hitherto been formed by any process essentially different from either of those adopted by him, which consisted in acting on the ferrocyanides with nitric acid, or the binoxide of nitrogen, under certain circumstances.

My attention being lately directed to this subject, I have ascertained that compounds possessing all the properties of the nitroprussides may be formed by the action of other chemical agents on the ferrocyanides, besides nitric acid or the binoxide of nitrogen. The following are the new methods by which I have succeeded in forming those compounds:

1st. By the action of hydrochloric acid on certain ferrocyanides in presence of a chlorate. Thus, when a mixture of ferrocyanide of potassium, chlorate of potash, and dilute hydrochloric acid is made, and these substances allowed to react slowly on each other at the ordinary temperature, there will, after some days, be formed, amongst other products, more or less of the nitroprusside of potassium, which may be recognized by the fine purple or violet coloration produced by a few drops of a soluble sulphuret in a little of the solution of the mixture previously neutralized by an alkaline carbonate; this reaction with soluble sulphurets being highly characteristic of the nitroprussides.

* Communicated by the Author.
Mr. E. W. Davy on some New Methods of

The proportions usually employed were about 1 part by weight of the ferrocyanide, from 2 to 3 of the chlorate, and \(3\frac{1}{2}\) of hydrochloric acid diluted with about twice its own bulk of water.

The two salts, being separately reduced to powder, were carefully mixed and then treated with the acid. I generally placed the mixture in a bottle leaving the stopper loose, and shook its contents from time to time to promote the reaction; when, after two or three days, the presence of a nitroprusside was detected in the mixture, and the quantity formed gradually increased with the time it was allowed to remain. By treating the ferrocyanide of lead in the same way, I found that a nitroprusside was very readily formed. It is probable, therefore, that many of the other ferrocyanides, particularly those the bases of which form soluble salts with nitroprussic acid, would give rise to the formation of nitroprussides when treated in like manner.

I may observe, that the application of heat to such mixtures as I have referred to, seems, instead of promoting, to be unfavourable to the formation of the nitroprussides, as the gaseous products arising from the action of hydrochloric acid on the chlorate principally escape without producing the desired effect; so that after gently heating such a mixture, only a mere trace of nitroprusside is ever formed, and that only when the chlorate and the hydrochloric acid have been in large proportions compared with that of the ferrocyanide. But if the mixture be allowed to remain at the ordinary temperature, little gaseous matter is evolved, and after some days a considerable proportion of nitroprusside will be formed.

2nd. By the action of hypochlorous acid on the ferrocyanides. Thus, when an aqueous solution of hypochlorous acid obtained by the usual way is gently warmed with a little of the ferrocyanide of potassium, some nitroprusside is quickly produced, its formation commencing at about 90° F., and being completed at about 110°. It is also produced, though very slowly, when those substances react on each other at ordinary temperatures, so that it cannot be detected till after the lapse of a day or two. I have also succeeded in forming nitroprussic acid compounds by gently heating the hydroferrocyanic acid, the ferrocyanide of lead, and prussian blue, with an aqueous solution of hypochlorous acid. In any of these instances the formation of the nitroprusside may be detected by the addition of a soluble sulphuret, as, for example, the sulphuret of ammonium, previously filtering the solution if necessary.

3rd. When ferrocyanide of potassium is dissolved in water together with chlorate of potash, and the mixture exposed to the action of light and the sun's rays, the two salts will very slowly react on each other, and a series of changes occur, amongst which
are the following:—The solution will become of a deeper yellow colour, which gradually passes to a brown, acquiring a faint alkaline reaction, and evolving a trace of ammonia; some peroxide of iron precipitates; and if the mixture be exposed for a sufficient time, no ferrocyanide of potassium can be detected in the solution, but in its stead is found the ferricyanide of potassium, together with a small quantity of the nitroprusside of the same base.

These changes take place very slowly, and it was not till after several weeks that any nitroprusside could be detected in the solution. The action of light also seems to be necessary to effect this latter change; for in some of the same solution kept excluded from the light for a much longer time, no nitroprusside could be detected, though the other changes noticed had to a slight extent taken place.

I may also remark, that when the ferricyanide of potassium (red prussiate of potash) is substituted for the ferrocyanide of potassium in this process, as also in the two former, nitroprussides will be formed with even greater facility. This is what might have been expected; for in each case using the ferrocyanide, that salt seems first to be converted, more or less completely, into the ferricyanide, and this latter gradually into the nitroprusside.

Dr. Playfair's process of forming the nitroprussides, viz. by the action of nitric acid on the ferrocyanides, particularly that of potassium, affords those compounds in larger quantity and with far greater facility than any of the methods just referred to, which are, however, interesting in a chemical point of view, as they show that those very remarkable compounds may be formed under a variety of different circumstances, and may hereafter tend in some degree to elucidate the theory of their formation, which is not at present very clearly understood.

In the Number of this Journal for last May, vol. v. p. 330, will be found a new test which I proposed for nitric acid and the nitrates, which is founded on the formation of nitroprussides by the action of the nitric acid (if present in the substance under examination) on a little of the ferrocyanide of potassium added to the mixture, and treated in the way there described. The facts which I have just referred to in the present paper, viz. that nitroprussides may be formed by the action of other chemical agents besides nitric acid on the ferrocyanides, would seem at first sight to render that test rather ambiguous; but the circumstances under which the nitroprussides are produced where nitric acid is the active agent, are different from those where they are formed by the combined action of hydrochloric acid and a chlorate on the ferrocyanides. In the former, those salts are imme-
diately produced on the application of a gentle heat, whereas in the latter only very slowly when the mixture is left some days at the ordinary temperature; and though a minute trace of nitroprusside may be formed in this latter case on heating such a mixture, it is only, as I stated before, when the chlorate is in great excess, and therefore enables us to determine the presence or not of that salt by other means before applying the test for nitric acid.

As to the case of hypochlorous acid, that acid also requires to be in considerable excess in order to form a nitroprusside; and as hypochlorous acid is endowed with such marked chemical properties, there is little fear of any mistake occurring from that source.

Finally, in the greater number of cases where it is desirable to determine the existence or not of nitric acid, those compounds of chlorine and oxygen are not at all likely to occur.

III. Note on a remarkable Modification of Sturm's Theorem, and on a New Rule for finding Superior and Inferior Limits to the Roots of an Equation. By J. J. Sylvester, F.R.S.*

In my paper on this subject in the preceding Number of the Magazine, I showed how by means of the quotients

\[ a_1x + b_1; \ a_2x + b_2; \ldots a_nx + b_n, \]

obtained by throwing \( \frac{\int f \, dx}{f \, dx} \) under the form of a continued fraction, the process for finding the signaletic index for any given value of \( x \) in the series for determining the number of real roots of \( f \, dx \) within given limits was reduced to performing two sets of \( n \) multiplications and as many additions or subtractions. But by means of a very simple observation, I can now show that the second and more laborious set of multiplications may be dispensed with and replaced by the simple operation of finding reciprocals, which can be done by mere inspection by means of Barlow's or similar tables, which are familiar to all computers. If we call the quotients

\[ a_1x + b_1; \ a_2x + b_2; \ldots a_nx + b_n, \]

we must, as explained in the preceding article, find the \((n)\) numerical values \( \mu_1, \mu_2, \ldots \mu_n \) which these quotients assume for any assigned value of \( x \). This being done, the signaletic index corresponding to such value of \( x \), i. e. the number of continuations of sign in the signaletic series

\[ 1, \mu_1, \mu_1\mu_2 - 1, \mu_3\mu_2\mu_1 - \mu_3 - \mu_1, &c., \]

* Communicated by the Author.
is evidently the number of positive terms in the series

\[
\frac{1}{\mu_1}, \frac{1}{\mu_2}, \frac{1}{\mu_3}, \ldots \frac{1}{\mu_n}
\]

These terms may be found with the utmost facility in succession from one another; for if \(M_i\) be one of them, the next will be \((\mu_{i+1} - M_i)^{-1}\). Thus, then, the necessity for the more operose set of multiplications is done away with, and the actual labour of computation reduced much more than 50 per cent. below that required by the method indicated in the preceding article on the subject. I need hardly add, that the old method of Sturm would admit of a similar abbreviation; but in using it we should be subjected to the great practical disadvantage of having to begin with the more heavy and complicated quotients \(\mu_n, \mu_{n-1}\), &c. instead of \(\mu_1, \mu_2, \&c.\), which would very greatly enhance the labour of computation. I will conclude by a remark of some interest under an algebraical point of view.

It has been stated that the denominators of the successive convergents to

\[
\frac{1}{q_n} - \frac{1}{q_{n-1}} - \frac{1}{q_{n-2}} \&c.
\]

are equivalent (to a constant factor près) with the Sturmian functions, and the reader may be curious to know something of the nature of the signaletically equivalent series formed by the denominators of the convergents to the direct fraction

\[
\frac{1}{q_1} - \frac{1}{q_2} - \frac{1}{q_3} \&c.
\]

These denominators are (abstracting from a constant factor not affecting the signs) the Sturmian residues resulting from performing the process of common measure between \(f'x\) and \(f_1x\); \(f_1x\) being related in a remarkable manner in point of form to \(f'x\).
Mr. J. J. Sylvester on a remarkable Modification

Call the roots of \( f(x) = a_1, a_2, \ldots, a_n \); we know that \( f'(x) \) is

\[
\sum \{(x-a_1)(x-a_2)\ldots (x-a_n)\};
\]

and I am able to state that \( f'(x) \) is (to a constant factor pres) equal to

\[
\sum \left\{ \xi(a_2, a_3, \ldots, a_n) \left\{ (x-a_2)(x-a_3)\ldots(x-a_n) \right\} \right\},
\]

\( \xi(a_2, a_3, \ldots, a_n) \) denoting the squares of the products of the differences between the \((n-1)\) quantities \( a_2, a_3, \ldots, a_n \). Accordingly it will be seen that whenever \( x \) is indefinitely near, whether on the side of excess or defect, to a real root of \( f(x) \), \( f'(x) \) and \( f'(x) \) will have the same sign; which serves to show, upon an independent and specific algebraical ground, why the two series of residues corresponding to \( \frac{f'(x)}{f(x)} \) and \( \frac{f'(x)}{f(x)} \) are as (by a deduction from a general principle they have been previously shown to be) rhizoristically equivalent.

7 New Square, Lincoln's Inn,
May 31, 1853.

Observation.

In comparing the relative merits of the old and new methods of substitution for the purposes of Sturm's theorem, the effect of the introduction of positive multipliers into the dividends in order to keep all the numerical quantities integral ought not to be disregarded. If we call the quotients corresponding to this modification of the dividends \( Q_1, Q_2, Q_3, Q_4, \ldots \), and the factors thus introduced \( m_1, m_2, m_3, m_4, \ldots \), the true quotients will be

\[
\frac{Q_1}{m_1}, \frac{m_1}{m_2}, \frac{Q_2}{m_1}, \frac{m_2}{m_3}, \frac{Q_3}{m_2}, \frac{m_3}{m_4}, \ldots, Q_n.
\]

and it will be found that we may employ as our rhizoristic index the number of continuations of sign in the series

\( 1, Q_1, Q_2, Q_3, Q_4, Q_5, \ldots \),

(1) or the number of positive signs in the series

\( 1, Q_1, Q_2, Q_3, Q_4, Q_5, \ldots \),

the law of formation of the successive terms \( v_0, v_1, v_2, \ldots \), being

\[ v_{k+1} = Q_{k+1} \cdot v_k - m_{k+1} \cdot v_{k+1}, \]

or \( m_{k+1} \) more multiplications than have been taken account of in the text above.
If integer numbers be used throughout (so that accordingly the \((u)\) series is that made use of), the total number of multiplications will in general be \(n + 2(n - 1)\) or \(3n - 2\); the old method, as previously stated, would require \(n \cdot \frac{n + 1}{2}\) multiplications; for if we call any one of the Sturmian functions
\[ A_0 x^n + A_1 x^{n-1} + A_2 x^{n-2} + \ldots + A_n, \]
we shall, using the most abbreviated method of computation, have to calculate successively
\[ x \cdot A_0 + A_1; \ x(xA_0 + A_1) + A_2, \ &c., \]
giving rise to \(\nu\) operations (but it must be admitted with the practical advantage of the use of a constant multiplier); and as \((\nu)\) may take all values from \(n\) to 1, the total number of such operations will be \(n \cdot \frac{n + 1}{2}\). When \(n = 4\),
\[ n \cdot \frac{n + 1}{2} = 3n - 2. \]
Consequently (if it be thought necessary to adhere to integers throughout), for values of \((n)\) not exceeding 4, the old method would be probably the more expeditious.

Addendum.

On a method of finding Superior and Inferior Limits to the real Roots of any Algebraical Equation.

The theory above considered has incidentally led me to the discovery of a new and very remarkable method for finding superior and inferior limits to the real roots of any algebraical equation. Suppose in general that
\[ \frac{N}{D} = \frac{1}{q_1} + \frac{1}{q_2} + \frac{1}{q_3} + \ldots + \frac{1}{q_n}; \]
then it is easily seen that
\[ D = M_1 \cdot M_2 \cdot M_3 \ldots M_n, \]
where
\[ M_1 = q_1, \ M_2 = q_2 + \frac{1}{q_1}, \ M_3 = q_3 + \frac{1}{M_2} \ldots M_n = q_n + \frac{1}{M_{n-1}}. \]

In general let any numerical quantity within brackets be used to denote its positive numerical value; so that, for instance, whether \(q = \pm 3\), \((q)\) will equally denote +3.

* If all the extraneous factors are units, the number of multiplications (like that of the additions) would be \(2n - 1\), and not \(2n\), as inadvertently stated in the preceding number of the Magazine.

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And now suppose that neither $q_1$ nor $q_n$, the first or last of the quotients, lies between $+1$ and $-1$, and that no one of the intermediate quotients $q_2, q_3, \ldots q_{n-1}$ lies between $+2$ and $-2$; so that, in other words,

$$(q_1) > 1 \quad (q_2) > 2 \quad (q_3) > 2 \quad \ldots \quad (q_{n-1}) > 2 \quad (q_n) > 1;$$

then, I say, that $M_1, M_2, M_3, \ldots M_n$ will have the same signs as $q_1, q_2, q_3, \ldots q_n$ respectively; for

$$M_1 = q_1; \quad \therefore (M_1) > 1;$$

but

$$M_2 = q_2 + \frac{1}{M_1}; \quad \therefore (M_2) = \left(\frac{1}{M_1}\right) > 2 + 1;$$

$\therefore M_3$ has the same sign as $q_3$, and also $(M_3) > 1$;

$\therefore$ in like manner,

$(M_4)$ has the same sign as $q_4$, and also $(M_4) > 1$;

and so on until we come to $M_{(n-1)}$, and we shall find $M_{n-1}$ of the same sign as $q_{n-1}$, and also $(M_{n-1}) > 1$.

Finally,

$$M_n = q_n + \frac{1}{M_{n-1}},$$

where $(q_n) > 1$ and $\left(\frac{1}{M_{n-1}}\right) < 1$.

$\therefore M_n$ has the same sign as $(q_n)$; but we cannot say (nor is there any occasion to say) that $(M_n) > 1$;

$\therefore D = M_1 \cdot M_2 \cdot M_3 \ldots M_n$ has the same sign as $q_1 \cdot q_2 \cdot q_3 \ldots q_n$.

Now let $f(x)$ be any given function of $x$ of the $n$th degree, and $\phi(x)$ any assumed function whatever of $x$ of the $(n-1)$th degree, and let

$$\frac{\phi x}{f x} = \frac{1}{q_1} + \frac{1}{q_2} + \frac{1}{q_3} + \ldots + \frac{1}{q_n},$$

where $q_1, q_2, q_3, \ldots q_n$ are now supposed to be linear functions of $x$, which, except for special relations between $f$ and $\phi$, will always exist, and can be found by the ordinary process of successive division.

Write down the $n$ pairs of equations,

$$u_1 = q_1 + 1 = 0 \quad u_2 = q_2 + 2 = 0 \quad u_3 = q_3 + 2 = 0 \ldots u_n = q_n + 1 = 0,$n$$

$u'_1 = q_1 - 1 = 0 \quad u'_2 = q_2 - 2 = 0 \quad u'_3 = q_3 - 2 = 0 \ldots u'_n = q_n - 1 = 0.$

If the greatest of the values of $x$ determined from these $2n$ equations be called $L$, and the last of these values be called $\Lambda$, 
it may easily be made out that between $+\infty$ and $L$, each of the quantities $q_1, q_2, q_3, \ldots q_n$ will remain unaltered in sign; and between $-\infty$ and $\Lambda$ also the same invariability of sign obtains; and, moreover, between $+\infty$ and $L$, and between $\Lambda$ and $-\infty$, $(q_1), (q_2), \ldots (q_{n-1}), (q_n)$ will be respectively greater than 1, 2, 3. Consequently by virtue of the preceding theorem between $+\infty$ and $L$, and between $\Lambda$ and $-\infty$, $D$ will always retain the same sign as $q_1, q_2, q_3, \ldots q_n$ and therefore no root of $f(x)$ will be contained within either such interval. And hence $f(x)$, which is manifestly identical with $D$ (the denominator of the continued fraction last above written), affected with a certain constant factor, will retain an invariant sign within each such interval respectively. Hence, then, the following rule.

Calling $q_1, q_2, q_3, \ldots q_n$ respectively

$$a_1x - b_1, \quad a_2x - b_2, \quad a_3x - b_3, \ldots a_nx - b_n,$$

if we form the $2n$ quantities

$$\frac{b_1+1}{a_1}, \quad \frac{b_2+2}{a_2}, \quad \frac{b_3+2}{a_3}, \ldots \frac{b_{n-1}+2}{a_{n-1}}, \quad \frac{b_n+1}{a_n},$$

the greatest of these will be a superior limit, and the least of them an inferior limit to the roots of $f(x)$.

The values of these fractions will depend upon the form of the assumed subsidiary function $\phi$. Hence, then, arises a most curious question for future discussion—to wit, to discover whether in any case the subsidiary function can be so assumed as that the superior limit can be brought to coincide with the greatest, or the inferior limit with the least real root, supposing that there are any real roots. I believe that it will be found that this is always impossible to be done. Then, again, if all the roots are imaginary, can inconsistent limits (evincing this Imaginariness) be obtained by giving different forms to the subsidiary function, which would be the case if we could find that the superior limit brought out by one form were less than the inferior limit brought out by another, or the inferior limit brought out by one form greater than the superior brought out by another? If, as I suspect, this also can never be done, then the general question remains to determine for all cases the form to be given to the subsidiary function, which will make the interval between either limit and its nearest root, or between the two limits themselves, a minimum. Thus, it appears to me, a fine field of research is thrown open to those who are interested in the theory of maxima minimorum, and minima maximorum, and one likely to lead to unexpected and important discoveries.

It may be asked how is the above rule to be applied if any of the leading coefficients in $\phi(x)$, or of the successive residues of
fx and \( \phi x \) vanish; in which case, instead of the coefficients being linear, some of them will be, as in fact all might be, polynomial functions of \( x \). The rule, it may be proved, will still subsist.

E quat ing the first and last quotients (each of them to \(+1\) and to \(-1\), and the intermediate ones to \(+2\) and to \(-2\)), the greatest root of all the equations so formed continues to be a superior, and the least root an inferior limit to the roots of \( fx \).

Nor is it ever necessary, even in these special cases, actually to solve any of these equations; for evidently it will be sufficient to find a superior limit and an inferior limit to each of them, and adopt the greatest of the superior and the least of the inferior limits as the superior and inferior limit to the roots of the given equation. Thus, then, we should have to repeat upon the quotients increased and diminished by 1 or 2 (as the case may be), the same process as is supposed to be originally applied to \( fx \), and thus by a continued process of trituration (since every new function so to be operated upon is of a lower degree than the original function) we must finally descend to linear equations exclusively.

It is interesting thus to see that there are no failing cases in the application of the rule, and that a solution of equations of a higher degree than the first is never necessary. But as a matter of fact, the chances are infinitely improbable (if \( \phi(x) \) is chosen at random), of any of the quotients after the first ceasing to be linear; and the first is of course linear, provided that the degree of \( \phi(x) \) is taken only one unit below that of \( fx \).

In working with Sturm's theorem, a system of quotients is supplied ready to hand; and these quotients, by virtue of the rule given above, may be used to assign a superior and inferior limit in the first instance, before setting about to determine the distribution of the roots between these limits by aid either of these same quotients or of the residues. For the change of sign of the residues required by the Sturmian process will only affect the signs, and not the forms of the quotients; but in the application of the above rule for finding the limits, the sign of any quotient is evidently immaterial.

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THE following abstract of observations and notes regarding the aurora, and the connexion which appears to subsist between it and the formation and modification of clouds, parti-

* Communicated by Professor Faraday.
particularly of the cirrus and cirrostratus types, are submitted to the consideration of meteorologists, in the hope that the various points of inquiry involved will receive a more general and systematic attention than they have hitherto obtained. A highly interesting connexion has been recently discovered between the variations of the solar spots and those of the terrestrial magnetic forces. The latter have been long known to be influenced by the aurora, and this phenomenon now appears to have the power of affecting in a very decided manner many of the meteorological processes of our atmosphere. A most interesting field for observation is thus opened up, the investigation of which promises to lead to very important results.

I. Aurora.

1. Table showing the Distribution of Auroral Displays with regard to dates or periods of the year,—1838 to 1847 inclusive.

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2. List of remarkable Displays of the Aurora,—1835 to 1848 inclusive.

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3. Crimson Aurora.

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4. Remarkable Auroral Arches or Bands, isolated and stretching across the Sky from W.S.W. to E.N.E.

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5. Aurora accompanied with Sound.

1835. Nov. 18. Streamers accompanied with a crackling noise, very distinctly audible. Most of the streamers on this occasion were in the southern part of the sky.

1839. Sept. 3. Sound of streamers like that produced by striking the air sharply with a switch. This aurora was seen in North America.

1839. Sept. 4. Aurora more hazy than that of last night, and in the form of sheets flashing wildly up to the coronal point, accompanied with a "whiffing" sound.

N.B. Except in the above instances, I have never heard sounds given forth by the aurora; in these cases the streamers probably descended lower than usual.
6. Height of the Aurora.

Although I have taken very many observations with a view to the determination of this point, yet from the want of comparable notes, a satisfactory result has only in one instance (that of the auroral arch of March 22, 1841) been attained. This arch was observed at Durham by Prof. Chevallier, at York by Prof. Phillips, and at Belfast by Prof. Stavely. From the comparison of the York and Durham observations, Prof. Chevallier deduced a height of 156.6 miles, and from that of Belfast and York 157.3 miles. From a subsequent comparison with Prof. Chevallier of his observations at Durham with mine at Dunse, the height of the arch appeared to be about 160 miles. The close agreement of these results is (when the nature of the subject is considered) very satisfactory.

From observations made by Mr. Brown, at Arbroath, and by myself here, on the aurora of February 11, 1842, I deduced a height of 63 miles above the earth's surface; but from the imperfect definition of the auroral mass observed on this occasion, there is considerable uncertainty as to the correctness of this result, though probably not far from the truth.

I need scarcely add my confirmatory testimony with regard to the relation of auroral phenomena to the magnetic meridian, &c., and their connexion with "magnetic storms,"—points now most satisfactorily established.

II. Cirri.

The following table is a summary of the directions of the motions of cirri, from 1840 to 1847 inclusive:

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<td>7</td>
<td>15</td>
<td>22</td>
<td>26</td>
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<td>128</td>
</tr>
<tr>
<td>1845</td>
<td>5</td>
<td>5</td>
<td>...</td>
<td>...</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>8</td>
<td>20</td>
<td>7</td>
<td>9</td>
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<td>13</td>
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</tr>
<tr>
<td>1846</td>
<td>6</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>3</td>
<td>3</td>
<td>12</td>
<td>32</td>
<td>4</td>
<td>14</td>
<td>3</td>
<td>21</td>
<td>21</td>
<td>4</td>
<td>162</td>
</tr>
<tr>
<td>1847</td>
<td>3</td>
<td>1</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>3</td>
<td>4</td>
<td>27</td>
<td>2</td>
<td>4</td>
<td>7</td>
<td>9</td>
<td>2</td>
<td>62</td>
<td></td>
</tr>
<tr>
<td>Sums</td>
<td>49</td>
<td>25</td>
<td>3</td>
<td>1</td>
<td>3</td>
<td>39</td>
<td>16</td>
<td>37</td>
<td>95</td>
<td>178</td>
<td>60</td>
<td>126</td>
<td>132</td>
<td>162</td>
<td>31</td>
</tr>
</tbody>
</table>

For the sake of comparison with the above, the following table, showing the directions of the winds at the surface for the same years, is subjoined:
Mr. W. Stevenson on the Aurora, and the connexion which

<table>
<thead>
<tr>
<th>Year</th>
<th>N.</th>
<th>N.N.E.</th>
<th>N.E.</th>
<th>E.N.E.</th>
<th>E.</th>
<th>S.E.</th>
<th>S.S.E.</th>
<th>S.W.</th>
<th>W.S.W.</th>
<th>W.N.W.</th>
<th>W.N.</th>
<th>W.N.N.</th>
<th>Sum.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1840</td>
<td>22</td>
<td>20</td>
<td>13</td>
<td>15</td>
<td>17</td>
<td>20</td>
<td>15</td>
<td>7</td>
<td>17</td>
<td>27</td>
<td>54</td>
<td>28</td>
<td>31</td>
</tr>
<tr>
<td>1841</td>
<td>29</td>
<td>20</td>
<td>83</td>
<td>10</td>
<td>20</td>
<td>15</td>
<td>13</td>
<td>8</td>
<td>18</td>
<td>23</td>
<td>33</td>
<td>54</td>
<td>34</td>
</tr>
<tr>
<td>1842</td>
<td>14</td>
<td>23</td>
<td>7</td>
<td>5</td>
<td>4</td>
<td>16</td>
<td>9</td>
<td>7</td>
<td>3</td>
<td>9</td>
<td>18</td>
<td>35</td>
<td>20</td>
</tr>
<tr>
<td>1843</td>
<td>24</td>
<td>31</td>
<td>16</td>
<td>11</td>
<td>28</td>
<td>8</td>
<td>7</td>
<td>3</td>
<td>13</td>
<td>15</td>
<td>72</td>
<td>14</td>
<td>57</td>
</tr>
<tr>
<td>1844</td>
<td>31</td>
<td>31</td>
<td>25</td>
<td>4</td>
<td>36</td>
<td>8</td>
<td>12</td>
<td>9</td>
<td>10</td>
<td>61</td>
<td>37</td>
<td>75</td>
<td>17</td>
</tr>
<tr>
<td>1845</td>
<td>45</td>
<td>32</td>
<td>11</td>
<td>3</td>
<td>18</td>
<td>8</td>
<td>7</td>
<td>2</td>
<td>13</td>
<td>18</td>
<td>80</td>
<td>88</td>
<td>68</td>
</tr>
<tr>
<td>1846</td>
<td>28</td>
<td>19</td>
<td>18</td>
<td>1</td>
<td>38</td>
<td>8</td>
<td>13</td>
<td>4</td>
<td>10</td>
<td>33</td>
<td>80</td>
<td>99</td>
<td>69</td>
</tr>
<tr>
<td>1847</td>
<td>29</td>
<td>38</td>
<td>12</td>
<td>3</td>
<td>28</td>
<td>11</td>
<td>13</td>
<td>1</td>
<td>11</td>
<td>31</td>
<td>72</td>
<td>94</td>
<td>58</td>
</tr>
</tbody>
</table>

Note.—The above observations were taken three times a day. The preponderance of the numbers under the cardinal and secondary points (as W. and S.W.) over those under the intermediate points (as W.S.W.), is due to the circumstance of the observations when the wind was unsteady, veering, or its precise direction not readily ascertainable, being registered rather under the nearest principal point than under a subordinate one. The same remark is also applicable to the table of the motions of cirri.

Comparison of the two preceding Tables.

<table>
<thead>
<tr>
<th>Motions of cirri.</th>
<th>Winds.</th>
</tr>
</thead>
<tbody>
<tr>
<td>N.W. to N.N.E. inclusive</td>
<td>774</td>
</tr>
<tr>
<td>N.E. to E.S.E.</td>
<td>28</td>
</tr>
<tr>
<td>S.E. to S.S.W.</td>
<td>542</td>
</tr>
<tr>
<td>S.W. to W.N.W.</td>
<td>1438</td>
</tr>
</tbody>
</table>

or, proportionally, and neglecting fractions,—

The cases in which the upper currents move from the easterly points are thus remarkably few. Of the eight instances mentioned above, the greater number (if not the whole) were scarcely
true cirri, occupying apparently a lower level in the atmosphere, and being mixed up with and passing into cirrocumuli.

Table showing the directions of the bands of cirri, 1840 to 1847 inclusive.

<table>
<thead>
<tr>
<th>Years</th>
<th>N.E. to S.W.</th>
<th>N.E.E. to S.S.W.</th>
<th>N. to S.</th>
<th>N.N.W. to S.S.E.</th>
<th>N.W. to E.S.E.</th>
<th>W. N.W. to E.</th>
<th>W.S.W. to E.N.E.</th>
<th>Sum. of Broken up or irregular cirri</th>
<th>Gross No. of observations.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1840</td>
<td>13</td>
<td>7</td>
<td>22</td>
<td>34</td>
<td>34</td>
<td>27</td>
<td>25</td>
<td>4</td>
<td>166 (not regd.)</td>
</tr>
<tr>
<td>1841</td>
<td>15</td>
<td>20</td>
<td>15</td>
<td>48</td>
<td>52</td>
<td>33</td>
<td>10</td>
<td>2</td>
<td>190 (do.)</td>
</tr>
<tr>
<td>1842</td>
<td>12</td>
<td>12</td>
<td>22</td>
<td>15</td>
<td>18</td>
<td>18</td>
<td>7</td>
<td>1</td>
<td>98 (do.)</td>
</tr>
<tr>
<td>1843</td>
<td>5</td>
<td>12</td>
<td>9</td>
<td>8</td>
<td>8</td>
<td>10</td>
<td>2</td>
<td>3</td>
<td>57 (do.)</td>
</tr>
<tr>
<td>1844</td>
<td>2</td>
<td>15</td>
<td>17</td>
<td>6</td>
<td>6</td>
<td>6</td>
<td>1</td>
<td>1</td>
<td>54 (do.)</td>
</tr>
<tr>
<td>1845</td>
<td>3</td>
<td>8</td>
<td>8</td>
<td>3</td>
<td>2</td>
<td>2</td>
<td>1</td>
<td>1</td>
<td>32 (do.)</td>
</tr>
<tr>
<td>1846</td>
<td>1</td>
<td>5</td>
<td>4</td>
<td>2</td>
<td>3</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>16 (do.)</td>
</tr>
<tr>
<td>1847</td>
<td>...</td>
<td>3</td>
<td>5</td>
<td>5</td>
<td>6</td>
<td>6</td>
<td>1</td>
<td>1</td>
<td>19 (do.)</td>
</tr>
</tbody>
</table>

Sums: 44 82 102 116 132 99 46 11 632 279 911

From the above table it is evident that the most frequent direction of the cirrus bands nearly coincides with the magnetic meridian; whilst the maximum of the directions of their motions is (as appears from the foregoing tables) nearly at right angles thereto. In the finest displays of cirri, I have almost invariably found the direction of the parallel bands to be at right angles to the line of motion. Cirri are often pennated, the fine cirrus fibres forming angles with the lines of the principal bands. They are also frequently dendriform. This latter arrangement appears due to the same cause as that which produces similar forms on a pane of glass by the freezing of the deposited moisture. True cirri are always composed of ice-crystals (probably four-sided prisms), as appears from their invariably producing halos of 47°, often accompanied with parhelia or paraselene, by the refraction of the rays of the sun or moon, whenever they intervene between us and these luminaries.

The following table exhibits the directions of the cirrus fibres, 1841 to 1847 inclusive, no observations having been made upon them in 1840.

<table>
<thead>
<tr>
<th>Years</th>
<th>N.E. to S.W.</th>
<th>N.E.E. to S.S.W.</th>
<th>N. to S.</th>
<th>N.N.W. to S.S.E.</th>
<th>N.W. to E.S.E.</th>
<th>W. N.W. to E.</th>
<th>W.S.W. to E.N.E.</th>
<th>Sum. of Irregular observations</th>
<th>Gross No. of observations.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1841</td>
<td>3</td>
<td>2</td>
<td>4</td>
<td>6</td>
<td>18</td>
<td>7</td>
<td>2</td>
<td>...</td>
<td>42 (do.)</td>
</tr>
<tr>
<td>1842</td>
<td>1</td>
<td>5</td>
<td>15</td>
<td>10</td>
<td>8</td>
<td>10</td>
<td>7</td>
<td>1</td>
<td>57 (do.)</td>
</tr>
<tr>
<td>1843</td>
<td>1</td>
<td>1</td>
<td>5</td>
<td>3</td>
<td>4</td>
<td>1</td>
<td>6</td>
<td>2</td>
<td>23 (do.)</td>
</tr>
<tr>
<td>1844</td>
<td>2</td>
<td>3</td>
<td>10</td>
<td>9</td>
<td>3</td>
<td>1</td>
<td>2</td>
<td>2</td>
<td>30 (do.)</td>
</tr>
<tr>
<td>1845</td>
<td>3</td>
<td>5</td>
<td>7</td>
<td>3</td>
<td>4</td>
<td>4</td>
<td>1</td>
<td>...</td>
<td>22 (do.)</td>
</tr>
<tr>
<td>1846</td>
<td>1</td>
<td>1</td>
<td>3</td>
<td>...</td>
<td>...</td>
<td>1</td>
<td>1</td>
<td>...</td>
<td>7 (do.)</td>
</tr>
<tr>
<td>1847</td>
<td>...</td>
<td>2</td>
<td>3</td>
<td>2</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>7 (do.)</td>
</tr>
</tbody>
</table>

Sums: 11 30 44 28 32 26 14 3 188 311 409
Comparing proportionally the directions of the cirrous bands and fibres, 1841 to 1847 inclusive, we have the following result:—

<table>
<thead>
<tr>
<th>N.E.</th>
<th>N.N.E.</th>
<th>N.</th>
<th>N.N.W.</th>
<th>N.W.</th>
<th>W.N.W.</th>
<th>W.</th>
<th>W.S.W.</th>
</tr>
</thead>
<tbody>
<tr>
<td>to</td>
<td>to</td>
<td>to</td>
<td></td>
<td>to</td>
<td>to</td>
<td>to</td>
<td>Sum.</td>
</tr>
<tr>
<td>S.W.</td>
<td>S.S.W.</td>
<td>S.</td>
<td>S.S.E.</td>
<td>S.E.</td>
<td>E.S.E.</td>
<td>E.</td>
<td>E.N.E.</td>
</tr>
<tr>
<td>Bands</td>
<td>31</td>
<td>75</td>
<td>80</td>
<td>82</td>
<td>98</td>
<td>72</td>
<td>21</td>
</tr>
<tr>
<td>Fibres</td>
<td>27</td>
<td>74</td>
<td>109</td>
<td>69</td>
<td>79</td>
<td>64</td>
<td>35</td>
</tr>
</tbody>
</table>

The connexion of the direction of the cirrous bands with the weather appears to have been long observed. I have heard the following couplet popularly applied to the phænomena in question:—

"North and south, the sign of drouth (drought),
East and west, the sign of a blast" (storm).

Some years ago, in a communication to the Philosophical Society of St. Andrews, I stated my opinion that the direction of the progressive motion of revolving storms or "cyclones" was the same as that of the upper currents for the time being, as indicated by the motions of cirri. My observations since have tended to confirm me in this opinion. It must, however, be taken only as approximately true, since the upper regions of the atmosphere are also affected by cyclonic movements, but apparently of longer period and of greater extent than those which prevail upon the surface of the earth. The proper ascertainment of this point would be of great interest as well as of much practical utility.

III. Cirrostrati.

These clouds resemble cirri in their tendency to an arrangement in long parallel bands; they differ, however, from them in not being composed of ice-crystals. They appear very frequently as concomitants of the aurora, so much so as to leave no doubt of a connecting influence between them and that phenomenon. They also prevail in dry, windy weather, occupying a stratum in the atmosphere above that in motion. I have frequently observed during a gale from S.W., cirrostratus bands extending in a N.W. to S.E. direction, and moving from S.W. so slowly that it required some time to determine whether they moved at all, whilst immediately under them light cumuli were flying at a tremendous rate. The cirrostratus condition of cloud appears to be induced upon masses of vapour, by the influence of the electricity developed by the friction of currents of dry air. On one occasion, a few years ago, upon a gusty, showery day, I observed that when the wind fell, the cirrostratus clouds overhead immediately became hazy and ill-defined, passing into the nimbus type, and rain followed. This continued until the wind again
rose, when the rain ceased and the clouds resumed the cirro-
stratus character. This interesting phenomenon was repeated
several times. Cirrostrati as well as cirri exhibit very decided
luminosity at night. I have often observed the luminosity
between the dark cirrostratus bands nearly equal to that of a
faint aurora. When the sky is clear during a very high wind, a
faint diffused luminosity has been often observed covering the
whole sky. The light given out on such occasions is often very
considerable. A friend of mine, who was on board a vessel in
the Mersey during the memorable storm of the 6–7 January 1839,
informs me that he was much struck by the distinctness with
which objects were visible during that night.

IV. Connexion of the Aurora with Cirri and Cirrostrati.

The cirrus and cirrostratus modifications of cloud have been
frequently observed as accompaniments of the aurora. The fol-
lowing extracts from my journal, referring to some of the more
striking examples of the effect of the aurora on clouds of these
types, as well as to certain allied phenomena, will perhaps be of
some interest.

"1840. July 15, 11th P.M. A beautiful luminous appearance
in N. and N.E. arising from cirri in that direction. Light very
uniform, and as vivid as that of an average aurora. It had a
sort of phosphorescent appearance.

"Aug. 19. Cirrous bands N. to S., moving briskly from W.
It was very interesting to see the alternating bands keep distinct
without mingling, although impelled by a considerable current.
The bands seemed to act upon each other with a repulsive force.

"Sept. 5, 9th P.M. Clear. Some streaky semi-luminous
masses of vapour resembling cirri scattered over the sky, and
rendered luminous at intervals by rapid pulsations traversing
them instantaneously and showing their full extent and forms.
No aurora visible at the time. Next morning, at 7th, the sky was
covered by cirri extending in lines from N.N.W. to S.S.E., and
moving slowly from W.S.W. A fine 47° halo surrounded the
sun for three or four hours.

"Sept. 21, 9th A.M. Pennated cirri, the principal bands ranging
N. to S., and the fibres N.N.W. to S.S.E.

"1841. Jan. 28, 11th 30m P.M. Wind blowing in gusts from
N.W. From the N.N.W. point, dark continuous cirrus bands
extended across the sky to S.S.E. Between the bands through-
out their whole extent a very decided luminosity prevailed, of a
fine deep yellow tint, like that of the aurora. The luminous
bands did not, however, show any tendency to converge to the
coronal point, which the streamers of the aurora always do, but
Mr. W. Stevenson on the Aurora, and the connexion which
kept distinctly separate to near the S.S.E. point. The light was
strongest between the dark rays in N.N.W., but was very distinct
throughout the whole extent of the bands. The absence of
moonlight rendered the phenomenon much more striking and
unequivocal.

March 14, 10 p.m. A splendid display of luminous cirri
or cirrostrati? From the N.N.W. point proceeded dark lines or
bands, which extended across the sky and appeared to converge
to the S.S.E. point. These were apparently at a much inferior
altitude than that of true cirri. They sometimes appeared to
shoot suddenly over a large arc of the sky. Between them a
very distinct luminosity was apparent, which from the N.N.W.
point, up to nearly 45°, equalled in brightness that of an ordinary
aurora; it was, however, very decided even to the S.S.E. point,
but no alteration was observable at the ordinary point of ap-
parent convergence of auroral beams (or that to which the dipping-
needle points). The air was still, with the exception of a few
light puffs from W.S.W. Occasionally black streaks appeared
to shoot from the N.N.W. point, contrasting very strongly with
the luminous ground on which they seemed projected, and then
as suddenly became so faintly marked as to be almost imperce-
tible. The light emitted by this phenomenon was very con-
iderable, and exactly resembled the pale greenish light of phos-
phorescent substances.

April 20, 7 p.m. Cirri moving from about N. or N.N.W.,
direction of the bands W.S.W. to E.N.E., or at right angles to
that of their motion. The electric state of the atmosphere has
evidently been disturbed for some days.

June 6, 11 a.m. Cirri and cirrocumuli moving rapidly
from N.N.W. The cirrour fibres very distinct, ranging W. to
E., or nearly at right angles to the line of motion. At 11, 30 a.m.,
the wind being brisk from N.W., the cirri presented a
curious appearance, some of the bands ranging N.N.W. to S.S.E.,
crossed by others from W. to E., and passing downwards into
cirrocumuli. The changes of form of the cirri at this time were
very rapid.

July 18, 1 p.m. Cirri exhibiting a most beautiful dendri-
form appearance, resembling the fantastic figures seen on glass
during frost.

July 30, 12 p.m. Cirrostrati affected by luminous pulsas-
tions.

August 6, 10 a.m. A very instructive exhibition of
cirrostrati in connexion with the aurora. A dense mass of
straw-yellow light formed an arch with its apex about 30° above
the N.N.W. point, hazy and ill-defined, and partly hid by some
cirrostrati, showing quite black upon the luminous ground and
banded in a W.N.W. to E.S.E. direction. At 11\textsuperscript{h} 30\textsuperscript{m} the arch was divided into two by a process in the N.N.W., the apex of the larger being N. and of the other N.W. This process at length moved upwards and to E., so that the western arch was in turn the larger. After this the arches united, forming a single arch, which was again divided by a similar process moving from W. At 12\textsuperscript{h} the altitude of the under or inside of the arch was about 15\textdegree{} or 16\textdegree{}. A most remarkable circumstance was, that some cirrostratus kept constantly in the same position, sometimes fading into a light fleecy aspect, and again becoming better defined as the aurora brightened. A small dense cirrostratus cloud in particular remained almost invariably in a position about 16\textdegree{} above the N.N.W. point. At 0\textsuperscript{h} 40\textsuperscript{m} A.M. a scud came over from W., and the aurora immediately became more active, shooting its rays into the clear space within the arch, and also upwards to the coronal point. The auroral rays now began to move, as it were in procession, from W. to E. in alternate light and dark bars, and the cirrostrati, or a portion of them, moved briskly in the same direction. Indeed the whole behaviour of the cirrostrati on this occasion showed in the clearest manner a connexion with the aurora, as was evinced from their remaining for hours in the same position, with but little variation in form,—their extraordinary swinging motion like that of a ship at anchor, as if fascinated by the auroral influence, and their fading and brightening simultaneously with the streamers. The wind during the whole display blew fresh from W.

August 10, 11\textsuperscript{h} 15\textsuperscript{m} P.M. Sky almost quite overcast with fleecy cumulus and cirrostratus clouds, moving very slowly from S.E. A series of rapid pulsations traversed the whole sky, rendering these clouds faintly and fitfully luminous. These sheets of faint lightning moved from W. to E., or at right angles to the major axes of the clouds which lay N. to S. No appearance of an aurora.

August 11, from 6\textsuperscript{h} to 10\textsuperscript{h} P.M. Beautifully distinct luminous cirrostrati extended in unbroken lines from N.W. by N. to S.E. by S.

August 24, 9\textsuperscript{h} P.M. A fine display of cirrostrati ranging N.N.W. to S.S.E. The dark streaks were beautifully regular and well defined, being relieved against a hazy luminous ground, which caused them to present the appearance of alternating light and dark stripes stretching quite across the sky. These experienced several alternations in distinctness, often instantaneously, as if by an electric discharge. The cirrostrati soon after grew broader and assumed a nimbus aspect, and by 10\textsuperscript{h} P.M. the whole sky was overcast with hazy nimbi, from which rain began to fall.
"September 11, 8th A.M. Cirri passing into cirrocumuli extending in broad fleecy bands from S.S.W. to N.N.E. These were crossed at right angles by fine, straight, parallel fibres, and also by parallel undulating bars ranging W.N.W. to E.S.E. The bars were short and arranged in fasciculi, a considerable number being included in each bundle. The motion of the whole was pretty brisk from S.S.W., or at right angles to the direction of the fibres and bars.

"September 25. (During the day some very curious irregularities had been observed with regard to the upper clouds.) 11th 30th p.m. Aurora flashing up to the coronal point with extraordinary rapidity. A bank of cirrostrati remained motionless along the horizon from W. by N. to E., extending upwards a few degrees. The corona was well-defined, and luminous sheets undulated up to it from the horizon with great rapidity. The length of the undulations lay nearly W. to E., convex towards the corona and concave towards the horizon. Two arches then formed, the apex of one bearing W.N.W., and that of the other N.N.E.; the latter was the brighter, and sent a constant undulating stream up to the corona. The cirrostrati next began to move to eastward, and with them the whole apparatus of the aurora, which had a very striking effect, the bright luminosity of the aurora contrasting strangely with the deep black of the cirrostrati, on which it seemed to sail majestically onward. 12th p.m. Sky overcast, a broad dark cirrostratus band stretching from W. to E., with patches of the same form of cloud scattered over the sky. Auroral luminosity with faint sheets and streamers in N.N.E. In W.N.W., a few degrees above the horizon, was a fixed auroral luminosity. The cirrostrati in several parts of the sky, especially in N.W., adorned with luminous fringes or tufts. Light airs from W. Next morning the sky was covered by cirri ranging N. to S.—N.B. A notable 'magnetic storm' accompanied this auroral display.

"November 5, 2th p.m. Fleecy cirri moving briskly from about N.W., under current from S.E., driving before it masses of cumulostrati. 10th p.m. Hazy aurora in N.N.W., cirrostrati moving briskly from S.W. Aurora affected by undulatory pulsations from the horizon to the zenith, which appeared to influence the movements of the cirrostrati, which were fitful and disturbed. These clouds were in some instances streaky, but generally amorphous. There were one or two faint streamers in N.N.W., from which point some luminous streaks extended, becoming fainter and narrower towards the south, without any appearance of coronal convergence.

"November 11, p.m. Cirri irregular, twisted, and passing into cirrocumuli, moving very rapidly from W. 10 p.m. Clear and
frosty, with dark cirri or cirrostrati upon a hazy but decidedly luminous ground, extending from W.S.W. to E.N.E. These covered the sky to an elevation of about 30° above the N.N.W. point, and were affected by very rapid pulsations, propagated apparently in a W. to E. direction. A similar appearance, though not so distinctly marked, was seen in the South, the remainder of the sky being quite clear.

"November 19, 3h p.m. A few cirri, rather irregular, but with a tendency to a N.N.W. to S.S.E. direction. They moved slowly from W.S.W. 7h 50m p.m. A bank of clouds along the S.W. horizon, remainder of sky clear. Auroral column shooting up from the E.N.E. point as far as Auriga. 8h 10m p.m., and for several minutes before and after, sky as before. A splendid luminous arch extended unbroken across the sky from W.S.W. to E.N.E., varying in width and brightness at different parts of its course, and also from instant to instant. Near the zenith its width varied from 2° to 6°. Pulsations passed along it from W. to E. Its centre crossed the magnetic meridian at an altitude of about 78°, passing over Altair, Arided, Capella, &c. No other auroral appearance visible. I was now called away for ten minutes, and on returning to the open air found the whole sky covered by patches of luminous clouds affected by rapid pulsations propagated from W.S.W. The arch, though still distinguishable, was now very faint and hazy. A small but very bright aurora cloud appeared in Cassiopeia, and then suddenly moved 2° or 3° to E.N.E., growing rapidly fainter and then disappearing. The sky was now obscured by clouds approaching to the cirrostratus type, between which the auroral light shone, chiefly, however, to the west, there having as yet been no appearances of the aurora in the usual quarter (N.N.W.). So suddenly was the sky obscured by the clouds in question, that they must certainly have been formed on the spot by the electric or electromagnetic influence, and not brought over by atmospheric currents. The air was nearly calm, except for a few gusty breezes from S.W. A thermometer rose 2° in 20 minutes, and another exposed freely to the sky rose 4° in the same time. At 9h p.m. the former thermometer marked 35°, and the latter 32°, having risen 6° within an hour. After this the clouds cleared off, with the exception of irregular banks of cirrostrati in E. and W., showing a few streaky lines ranging about N.W. to S.E. The thermometers at 10h p.m. indicated 33° and 26° respectively, or nearly the same as at 8h p.m. The auroral light was now concentrated in the usual quarter in an arched mass, the apex of which bore N.N.W. altitude 14°. A few patches of dark clouds moved from W. to E. within the luminous space. 11h 20m. Sky quite clear, with a very faint auroral light just above the N.W. point. Air quite calm, frosty."
"Dec. 14, 9th P.M. Bright aurora in N.N.W. Cirrostrati stretching across the sky from N.N.W. to S.S.E. Auroral light very bright in N.N.W. between the dark radii of the cirrostrati. I noticed a bright luminous streak in S. extending S.S.E. to N.N.W.

"Dec. 17, 9th P.M. During an auroral display the luminosity of the cirri was well-marked. A very curious feature, apparently due to the auroral influence, was, that the cirri alter proceeding in straight lines from the N.N.W. point to an altitude of about 45° in a S.S.E. direction, took a sudden bend and proceeded in straight lines, to the S.S.W. point. At 11th P.M. the auroral light had exhausted itself, and the cirri extended unbroken across the sky from N.N.E. to S.S.W., well defined and very distinctly luminous.

"1842. June 4, 12th P.M. Sky in E. and S.E. overcast by nimbi, from which a few drops of rain were falling. Breaks in the W. and N.W. quarter disclosed some luminous cirrocumuli, with a few streaks shooting rapidly from N.W. by W. The light was nearly as bright as if the clouds had been fully moonlit. June 5, 0th 40m A.M. The whole sky overspread by fleecy clouds emitting a very considerable pale phosphorescent light. These clouds formed an almost continuous stratum of unequal thickness, the denser portions being the more luminous, and arranged in tufty patches connected by rarer spaces, through which the rays of the stars penetrated as through a veil. The whole were agitated in a most singular manner, swinging to and fro, and being traversed by undulating pulsations, which varied their luminosity, and produced an effect resembling that of the waving of a phosphorescent sea. The clouds moved slowly from about W. Occasionally they would suddenly assume a deep nimbus aspect. Soft drops of rain were constantly falling, but in a very irregular and curious manner. I noticed a flash of sheet lightning at this time. 1th 5m A.M. Luminous clouds passing into nimbi. Luminosity greatest in W. and S.W. Air nearly calm. About 11th A.M. a waterspout was observed about ten miles S.W. from Dunse. There were heavy thunder-clouds at the time.

"1843. June 15, 8th P.M. Cirri passing into cirrocumuli, moving briskly from E.N.E. Their elevation was, however, lower than the general region of cirri. The direction of the cirrus bands was N.N.W. to S.S.E., fibres dendriform and fleecy. 12th P.M. Cirri extending from N. to S., shooting out in a fitful manner, and much resembling the rays of an aurora.

"August 24, 6th P.M. Cirrus bands W.S.W. to E.N.E., motion in the same direction, rather brisk. Fibres irregular, twisted and with fleecy or cirrocumulus tufts. At 8th 50m a splendid luminous zone suddenly appeared, spanning the sky from W.S.W.
to E.N.E. At 9h it passed over Arcturus, and on to eastward so as to bisect obliquely a line joining the Polestar and the upper Pointer. From this time to 9h 10m it gradually moved to northward. Its altitude above the N. by W. point was now about 44°. Its breadth was about 3°. At one time it appeared double. Its northern limb was well-defined and regular (with the exception of a slight bend near the middle). The whole sky to south was covered with a sort of cirrus haze so dense as almost to obliterate Jupiter, and a few cirrostrati were moving from about south. The sky to north of the arch was perfectly clear, and without the slightest appearance of an aurora. At about 9h 15m the haze cleared away from behind the arch, and it then was seen to much advantage in the form of a well-defined luminous belt of nearly equal width (about 3°) throughout, along which pulsations (electric?) continually passed so rapidly that I could not be certain whether they moved from W. to E. or from E. to W. The arch lasted about thirty minutes. Its light was white and very splendid, quite free from the yellow tinge which distinguishes the light of ordinary aurora. Its elevation also appeared to be much inferior to that of any of the auroral arches resembling it in form, which I have noted. Indeed I have doubts whether this phenomenon should be classed with the aurora or with cirri. Its direction, it will be remarked, was the same as that of the cirrus clouds noticed three hours before.

"September 2, 9h 15m p.m. Cirrosumuli forming in different parts of the sky, and moving briskly from S.W. A fasciculus of these during the process of formation was a very interesting sight. It seemed as if a machine was at work in the upper regions, turning off a series of flocculous bars of uniform length and width, and strictly parallel to each other. These at length amounted to about twelve in number, ranging in a N.W. or N.N.W. direction, and moving briskly nearly at right angles to this direction. A small insulated cloud of the same kind also changed its form and size very considerably. The sky afterwards became quite overcast, the clouds moving from S.W. with a faint luminosity above them. The barometer which had been high and steady now began to fall quickly, with a brisk S.W. wind.

"1844. Feb. 25, 10h 30m p.m. Cirri extending from W.N.W. and moving from the same point. Within half a minute some hazy cirri (?), formerly so thin as to be scarcely perceptible, suddenly assumed the aspect of dense cirrosumuli moving fitfully from W.N.W. The cirri were affected by luminous pulsations distinctly observable, notwithstanding the moonlight. The cirrostrati became denser and thinner by turns, and moved onwards in an irregular manner, and as it were by fits."

*Phil. Mag. S. 4, Vol. 6, No. 36, July 1853.*
"Aug. 31, 9th p.m. Cirrostrati spread over the whole sky in bands stretching from W. to E. Above these were fleecy cirri, which underwent rapid and frequent changes in density, luminosity and form. Sometimes luminous patches would form in an instant and then as suddenly disappear. Cirrus streaks also appeared and disappeared in the same manner. The clouds moved from W.N.W.

"Sept. 29, 9th 10m p.m. Auroral arch in N.N.W. about 2° or 3° broad, the apex about 25° above the N.N.W. point. Within it were a few faint streamers. In a few minutes the upper part began to assume a cirrous appearance and to move towards the zenith. Cirrus streaks then began to form within the arch, and in a short time (9h 45m) all the space which the aura had occupied was covered with compact, luminous, and fibrous cirri, the aurora having passed by degrees into this congeries of cirrous clouds. The cirri moved briskly from about N.W., their fibres ranging N.N.E. to S.S.W. They formed a 47° halo in passing over the moon. 10th 30m p.m. Clear, slight auroral haze in N.N.W., cirrus moving from N.W. 11th p.m. Sky perfectly clear and no appearance of aurora.

"Nov. 16, 10th 15m p.m. Cirrostrati from N.W. to S.E. Sky obscured by a cirrous haze, which in several places was condensed into patches of cirri or cirroecumuli. Auroral luminosity in N.N.W., from which luminous undulations passed with great rapidity up to the zenith, and were even perceptible over the whole sky, though they all originated in N.N.W. In a few minutes the haze under the influence of these pulsations assumed a more decided cirrostratus character. The auroral light was not very intense, but the pulsations were very distinct, and seemed to traverse the whole sky instantaneously.

"Nov. 22, 8th p.m. Brilliant streamers shooting up towards the zenith in the usual manner. 8th 15m. No auroral appearance visible, fleecy cirri moving from S.W., and at a greater height fibrous cirri moving from S., the fibres ranging W. to E., or at right angles to the direction of the line of motion. These were affected by luminous pulsations similar to those of the aurora; they had not, however, such a yellow tinge. The cirri underwent considerable changes in a very short time.

"Dec. 7, 9 p.m. Sky clear. Cirrostrati suddenly began to form simultaneously in different parts of the sky, and at length formed a broad band extending from N.W. to S.E. A slight haze first appeared which became denser, until it at length assumed a well-defined cirrostratus appearance. During the formation of these clouds the thermometer rose from 27° to 29°.

"Dec. 29, 2nd p.m. Cirri moving briskly from W.N.W., their 'fibres' being distinct N.N.E. to S.S.W. 4th p.m. Cirrostratus
streaks from W.N.W. to E.S.E. 5th 50m. Auroral luminosity in N.W., cirrostrati from W. by N. to E. by S. shooting fitfully across to eastward, and much resembling auroral streamers but for their murky hue. 7th 59m. Aurora over the whole sky except a portion in S.E., the apex of the S. limb being about 35° above the S.S.E. point. Cirrostrati from W. to E. over the whole sky, except a few clear spaces near the S. margin of the aurora. Streamers shooting up to the corona. These proceeded almost exclusively from the W. point, from which a stiff breeze blew, and they seemed to move in accordance with the gusts, their evolutions being often very rapid. [N.B. This auroral display was the finest seen here for a long period, and presented many interesting features. The cirrostrati became less distinctly marked as the auroral light diminished in intensity.]

"1843. Jan. 5, 9th A.M. Cirrostrati extending across the sky from N.W. to S.E., motion imperceptible. High wind with nimbi driving rapidly from W.S.W. 9th p.m. Luminous appearance above the clouds, which were nimbi and cirrostrati; wind still high.

"Jan. 8, 10th p.m. Luminous cirrostratus haze over the sky, with some dark bands extending N.N.E. to S.S.W. The density of the dark and luminous portions seemed to be the same, the brilliance of the stars being nearly equally affected by both.

"Jan. 9. A remarkable display of the aurora connected with cirrostrati. The phenomena deviated in some respects from the regular order of auroral displays, no tendency to the formation of a corona having been exhibited, and the centre of the aurora having at one time been decidedly to W. of the magnetic meridian.

"April 23, 10th p.m. Cirrus bands and fibres ranging N.N.E. to S.S.W., and shifting their form and position, exactly after the manner of auroral streamers, which they much resembled. They were, however, distinguishable from these by a whiter and denser appearance, by being at an inferior elevation, and by showing no tendency to coronal convergence. They moved slowly from E.

"Aug. 29, 8th 30m p.m. Streamers appeared in N.N.E. a few degrees above the horizon, and in a short time formed a flattopped arch. In N.W. and W.N.W. were a number of scattered cirri. At 9th p.m. a faint streak shot up from E.N.E., and in about a minute a splendid arch of intense white was completely formed, spanning the sky from E.N.E. to W.S.W., and crossing the magnetic meridian at an altitude of about 78° above the S.S.E. point. Its breadth was about 1°, regular and well-defined. In the course of five minutes it had moved considerably to S.; it then remained stationary for ten minutes, when it disappeared. Just before its disappearance its apex was 66° above

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the S.S.E. point. 9th 15m. Auroral light in N.N.W. with a few streamers. 9th 20m. A few luminous clouds scattered along the zone formerly occupied by the arch. 9th 25m. These clouds more numerous, small, and ranging with their major axes N.W. to S.E. 9th 30m. The arch re-formed by the union of these, and occupying a position nearly midway between those it formerly held. Its width was now less regular, varying from 1° to 4°. It formed a curious flexure between Lyra and Cygnus, the concave side facing the north. It moved gradually south till its apex bore 65° above S.S.E. The eastern limb now began to break up and disappear, leaving several luminous clouds to mark its course. The western limb remained for several minutes longer. At 9th 55m a streak extended from the W.S.W. point up to α Ophiuchi, being the only auroral appearance visible, the aurora in N.N.W. (which was always most distinctly separated from the arches above described) having disappeared about 9th 30m. The sky was clear during the display. The wind (?) made a singular, continuous, rushing sound, although the trees did not seem to be affected proportionally. Next day the sky was covered by cirri and cirro-cumuli, with compact cirri in N.W.

"Dec. 3, 7th p.m. During a very active auroral display the luminous sheets proceeded chiefly from S.W., from which point a fitful breeze was blowing, the auroral flashes seeming to keep time with the gusts."

"1846. Jan. 21, 8th p.m. A cirrostratus streak suddenly appeared faintly luminous on the west side, and stretching across the sky from about N.W. to S.E.; in a few minutes the sky was completely obscured by hazy cirrostrati. Air calm, barometer very low.

"Sept. 11, 9th a.m. Cumuli moving from S.W., and above them fleecy cumuli moving from about N.W. A mass of compact cirri in N.W. in the form of an irregular arch extending upwards to about 45°. Below this the cumuli assumed more of the cirrostratus and nimbus form. Light gusty breezes from W. 2th p.m. Fleecy cirri passing into cirro-cumuli, moving briskly from N.W. An irregular mass of pale compact cirri in N.W., under which were cumuli as before. 9th p.m. Auroral light in N.N.W., with a few streamers and cirrostrati. Shortly before 10th p.m. a luminous arch was seen by some persons, but it soon disappeared. It was described as extending upwards from W. 11th p.m. A luminous haze (cirrous or auroral?) over most of the sky, through which the stars were dimly visible.

"Oct. 19. During an auroral display a collection of cirrostrati continued for a long time scrupulously within the limits of the auroral luminosity, being arched in the same manner and occupying nearly the same extent of sky."
Oct. 25, 1st 30th A.M. (after a violent storm). The sky presented a singularly wild appearance, heavy masses of cloud driving along from N.N.E., whilst at a greater elevation fleecy cumuli and cirrocumuli strongly luminous overspread the whole sky. Some spaces between the clouds showed a thin veil of cirri which dimmed the stars in the same manner as the auroral light does. It was nearly equal to this in the intensity of its light, which was, however, whiter.

Nov. 20, 9th P.M. A very distinct luminous haze similar to the above over the whole sky. Wind high from S.W.

Dec. 20, 9th P.M. Cirrostrati extending across the sky from W. to E. The spaces between the bands very decidedly luminous over the whole sky, nearly equaling in this respect a faint aurora. Wind light from W.; barometer declining.

1847. Jan. 30, 10th P.M. Faint auroral light in N.W., which soon assumed the appearance of cirrus streaks crossing the magnetic meridian at right angles. At 10th 31st an arch was formed, narrow but well-defined, especially the lower edge, which was about 18° or 19° above the N.N.W. point.

Feb. 6. Fine auroral arch, accompanied by cirrostrati and other appearances similar to those already described.

June 6. A fine display of parhelia occasioned by cirri, &c.


Nov. 2, 9th 15th P.M. Calm, sky almost cloudless, cirrostratus haze over the whole sky in the form of alternately light and dark bands of various widths stretching continuously across the sky from W.S.W. to E.N.E. These bands appeared fainter overhead than to N. and S., but this might be owing to a smaller body of vapour meeting the eye in that direction. The light and dark bands in the southern part of the sky appeared to be nearly equally dense, stars down to the third magnitude being indistinctly visible through both, the obscuration produced not exceeding that caused by a moderately bright aurora. In the north the dark bands were denser, being well-defined cirrostratus streaks, the spaces between which were luminous.

Nov. 9. A similar display of luminous cirrostrati. On this occasion the bands ranged N.N.W. to S.S.E. They were long and slender, and better marked at the N.N.W. point than at the opposite. They did not seem to be formed by spinning or shooting out from a point as they are sometimes seen to do, but looked rather as if formed simultaneously along their whole length, invisibly faint at first, but becoming gradually more definite.

1848. Sept. 18, 10th 30th P.M. Luminous cirri extending
overhead from N.N.W. to S.S.E., occasionally shooting out longitudinally and passing through various gradations of brightness, being affected by the transmission of (electrical?) pulsations. Sometimes the cirri assumed a curdy cirrocumulus appearance.

"Nov. 17. A splendid auroral display. It first appeared about 5h 30m p.m. (Greenwich or railway time) in the form of a luminous haze in N.W. At 6h p.m. a broad hazy auroral arch or zone extended completely across the sky from E.N.E. to W.S.W. Its southern edge was tolerably well defined, and passed exactly through the Pleiades, and the most southerly of the four conspicuous stars in the Swan. In the course of a few minutes it had moved several degrees to S.S.E. Streamers now appeared in great numbers, occupying the whole sky, with the exception of an arched space in S.S.E., which remained clear to an altitude of about 20°. In the N.E. and W. crimson and rose-coloured patches appeared among the white and yellow streamers, presenting a very beautiful appearance. A corona was formed at the usual place (about 18° S.S.E. of the zenith) by the convergence of the auroral beams. These appearances continued with variations till 8 o'clock, when the streamers began to show greater activity in their movements, shooting up from all points to the corona, whilst luminous sheets appeared and disappeared with great rapidity in various parts of the sky. Near the corona the streamers were curiously contorted, forming curves like those assumed by iron filings under the influence of the magnet. At 8h 22m the sky was covered by sheets and streamers of yellow, orange, and crimson hues, excepting the arched space above mentioned, the apex of which was now 14° above the S.S.E. point. The corona was beautifully defined at intervals, position same as before. At 9h p.m. the phenomenon might be described as an Aurora australis, being wholly confined to the southern part of the sky, the northern being quite clear. A greenish-white arch of considerable brightness spanned the southern sky at right angles to the magnetic meridian, enclosing a clear space, which extended to 9° above the S.S.E. point. Above this arch, which was several degrees broad, was a broad irregular zone of crimson streamers, the centre of which ranged from about 32° to 39° above the S.S.E. point. At 9h 7m the height of the clear space in S.S.E. was reduced to 8°, and about 9h 15m to 7°. At 9h 17m a sudden change took place, the white arch breaking up and mixing with the crimson in the form of streamers, whilst brilliant white and yellow rays shot upwards from the east and west. In a minute or two the southern part of the sky, up to an arched line passing from E.N.E. exactly through the corona to the W.S.W. point of the horizon, was occupied by the most splendid streamers which have been observed.
at this place for many years. Crimson, yellow and green, of many different shades, were blended with brilliant white in an indescribably beautiful manner. The light given out by the aurora at this time was considered fully equal to that of the full moon. A dull red haze (perhaps merely the reflection of the crimson streamers) extended along the southern horizon. White streamers next began to shoot up to the corona from the northern points, and a complete cone of rays was formed, of which the corona formed the apex. The crimson streamers were still chiefly confined to the southern parts of the sky. At 10th P.M. the sky (excepting a small space in S.S.E.) was nearly covered by yellowish-white streamers and luminous masses. From this till 12 o’clock the aurora continued with varying brightness, being chiefly confined to the south, and being most vivid about the region of the celestial equator. The rays converged with great regularity and symmetry to the corona. After this the aurora increased in brightness, and at 1h A.M. (18th) the corona was beautifully defined, the streamers in its vicinity being crimson, pink and white, and very vivid. About 1h 20m cirrostratus clouds began to form rapidly, especially in the east, about midway between the horizon and zenith. These arranged themselves in lines ranging nearly N.W. to S.E., and appeared to be influenced in their formation by the aurora, a circumstance which has been repeatedly noticed. These clouds remained for a long time nearly motionless, although a high wind was blowing from S.W. during the whole night in the lower regions of the atmosphere. After this the aurora, besides being obscured by these clouds, became intrinsically fainter.

"The forenoon of the 18th was wet and stormy, wind W. to N.W. When the lower clouds were partially dispersed, the upper strata were seen to consist of compact and fleecy cirri, having a tendency to a linear arrangement from W.N.W. to E.S.E., together with patches of cirrostrati, cirroccumuli, and cumuli. The cirri appeared to be almost motionless. In the evening the aurora again appeared, but faint, and presenting no features of peculiar interest. The barometer fell rapidly during the nights of the 17th and 18th. The morning of the 19th was pleasant, with sharp frost. In the evening a great assemblage of cirrus and cirrostratus clouds appeared, stretching right across the sky from N.W. to S.E., a faint luminosity appearing between the dark streaks at the N.W. point. At 10th P.M. the wind began to rise from about S.W. This day (Nov. 20) has been very stormy throughout, a heavy gale blowing from S.W., accompanied by frequent showers.

"It may be remarked that the period about the 17th of November of almost every year is marked by displays of the aurora.
Out of nineteen displays, selected from upwards of 250 observed here within the last thirteen years, on account of their remarkable splendour, we find the following occurring about the middle of November, viz.:—1835, Nov. 17; 1837, Nov. 12; 1841, Nov. 18; 1847, Nov. 19; and 1848, Nov. 17. On Nov. 17, 1839; Nov. 17, 1840; Nov. 16, 1844; Nov. 17, 1845, and Nov. 17, 1846, aurora also appeared, though not of very remarkable brightness. The two last-mentioned instances were the only cases of the appearance of the aurora which were noticed in the months of Nov. 1845 and 1846 respectively. There would thus appear to be a certain periodicity in aural displays, and to this we would respectfully call the attention of meteorologists.

"Several of the meteors called shooting-stars have been seen in the course of the past week. On the evening of the 12th (a time remarkably characterized by appearances of such meteors), at 6h 57m, one of these, much brighter than the planet Venus, appeared to drop vertically downwards from a point situated about midway between the star Alpha in the Northern Crown and the horizon.

"Nov. 21, 6h 30m p.m. Arched aurora in N.W. under which were many cirrostratus streaks ranging at right angles to the magnetic meridian, and occupying very nearly the same extent of sky as the aurora. Remainder of sky clear, with the exception of large cirrostratus masses in W. 8h p.m. Cirrostrati along the horizon, chiefly in N., but of a hazy and dubious character. Auroral light rather faint and hazy, but occupying fitfully the greater part of the sky both in N. and S. The sky was covered by a thin haze, dimming the stars and affected by luminous pulsations. Irregular patches of various sizes and very vaguely defined would instantaneously become luminous, and then as suddenly relapse into the apparently uniform haze. The aurora could scarcely be said to affect the form of rays or streamers, being rather a congeries of fitfully luminous clouds, somewhat resembling the lightest form of cirroculi, and scattered over the sky in an irregular manner. 8h 30m. Patches of hazy cirrostrati scattered over the whole sky, under a uniform luminous haze, the light emitted by which was very considerable, greatest in the northern part of the sky, but also very distinct in the southern. The cirrostratus patches referred to thickened and extended whilst under observation, and soon arranged themselves in lines extending across the sky from nearly S.W. to N.E. Wind very light from about S.W. 10h p.m. Luminosity above the cirrostrati very considerable over the whole sky. In the north it seemed decidedly auroral, but there was no appearance of streamers. 11h p.m. Sky covered by cirrostrati passing into nimbi, with appearances of rain, luminosity above clouds still
distinct. The whole appearances this evening showed in a striking manner the influence of the aurora on the formation and modification of clouds; the cirrostrati being formed under the eye from a thin, luminous vapour, which if not substantially the aurora, was at least a substratum for its operations.

"Nov. 22, A.M. A complete deluge of rain; wind light from E.S.E. and S.E.; P.M. also very rainy. 8th P.M. Cloudy, fair, wind S.S.W. 10th P.M. A remarkably interesting appearance over the whole sky, of cirrostratus masses under a luminous haze. These had a tendency to range W.N.W. to E.S.E., but were chiefly in the form of small tufts of a dark hue, contrasting curiously with the luminosity above them. The luminosity was strongest in W.N.W., where it seemed to be decidedly (?) auroral, but was also very obvious in every other part of the sky, the light being, as I estimated, sufficient to allow of common hand-bill type to be read by it. The light was indeed remarkable, since there was no moonlight, and the sky was quite clouded. The cirrostrati varied much in form in short intervals of time, but seemed to retain nearly the same position. No luminous pulsations similar to those noticed last night and on other occasions were observed.

"Dec. 16, 6th to 8th P.M. Cirrostrati from S.S.W. to N.N.E. faintly luminous between the dark streaks; hazy aurora in N.N.W., arched, but without streamers. Long dark cirrostrati occasionally shot from S.S.W. across the sky. It would almost seem that the electric or electro-magnetic forces acting in certain lines, separate aqueous vapour from the air which holds it in solution, causing it to become visible and to assume a determinate form. Between the radii in S.S.W. the luminosity was sometimes very considerable. The aurora did not approach this point, but its eastern limb extended to the N.N.E. point of apparent convergence.

"Dec. 17, 7th to 11th P.M. A beautiful auroral display. The greater part of the sky, except a portion in S.E., was at times occupied by sheets and streamers. The corona was frequently very well defined, with a marked contortion of the auroral beams in its vicinity. The streamers were mostly white and yellowish, with occasional patches of crimson and orange. Luminous sheets frequently passed up towards the zenith with great rapidity, these movements being apparently connected with the wind, which blew in gusts most frequently whenever the luminous sheets prevailed. The only clouds seen were cirrostrati, which became more numerous and dense towards midnight.

"1849. Feb. 19, 8th 30m P.M. Aurora in N.N.W. up to about 60°, a few reddish streamers. 9th 15m. A splendid white zone, 2° or 3° broad, extended across the sky from W.S.W. to E.N.E,
its centre passing over Aldebaran and midway between Castor and Pollux. Presently short streamers began to depend from its lower edge, like a row of fringe, sloping as if with a tendency to converge to the coronal point. At 9th 20m waves began to move along it from E. to W., and at 9th 25m it looked like a wavy river of light flowing rapidly from E. to W., exactly between Castor and Pollux. By 9th 40m it had disappeared, having moved considerably to southward. Between 9th 15m and 9th 45m the aurora in the N.N.W. remained nearly unaltered, the upper part hazy, the lower arched and well-defined, enclosing a clear space, the apex of which was immediately under Arided, and in the course of the 30 minutes did not vary its position more than a degree (which it moved upwards). Wind all the time high from S.W. The only clouds were cirrostrati, tending from a W.N.W. to E.S.E. direction.

"Feb. 20, A.M. Cloudy, cirrostrati and nimbi under cirri and cirrocumuli, wind S.W. with a little rain. 8th 30m P.M. Clear, aurora in N.N.W. similar to last night, top of clear space within arch just upon Arided, upper part of aurora hazy, a few orange and crimson streamers. 10th 15m. The same, arch similar to that of last night, stretching across the sky and occupying nearly the same position, passing between Castor and Pollux. It had been seen for some time before this, but soon after disappeared, moving slowly to S. as it grew fainter. Part of the W. limb remained visible for some time after. No appearance of the waves noticed last night. 11th 30m. Diffused aurora in N.N.W., without streamers; sky clear, except some cirrostrati immediately under the aurora.

"1850. Jan. 29, 12th P.M. Cirrocumulus bands moving slowly from about W.N.W. Their length was about ten times their breadth, and at right angles to the direction of their motion. There were thirteen distinct and nearly equal bands, quite parallel and separated by clear spaces of uniform width.

"Aug. 8, 10th to 12th P.M. A very interesting display of electrical clouds. Along the east and west horizon, and extending upwards on each side more than 45°, were bands and patches of dark cirrostrati passing into nimbi and heavy cumuli. Above these clouds was a very distinct luminous haze. The clouds had a decided linear arrangement from N.N.W. to S.S.E. The sky near the zenith was sometimes quite clear, then a thin dark haze would come over it, small dark amorphous patches of cloud would form, expand, unite with other masses, and again fade away all in a few minutes. At one time a very well marked narrow cirrostratus band extended from S.S.E. through the zenith to N.N.W. It was a little twisted, having been formed of amorphous masses united polarly. This also soon disappeared. An
immense number of flashes of sheet lightning were noticed, about
two or three per minute on an average, or probably not less than
300 in the course of two hours. These were all observed in the
east, none having been seen in the west. They flashed along the
cirrostratus bars which extended along the eastern horizon, illu-
minating with a quivering light the upper edge of the east cirro-
stratus bank. A most remarkable feature was the frequent ap-
pearance, at the S.S.E. point, of a very bright flash, from which
the electricity proceeded with greatly diminished brightness along
the east cloud bank. It always appeared suddenly at the same
point, and sometimes looked like a large fire-ball, but was too
rapid in its motion to be defined by the eye. In N.N.W. there
was a strong luminosity, but here of course the remains of twi-
light were mixed up with the electrical light. There was a dead
calm all the time.

"October 30, 10\text{th} p.m. Cirrostrati from N.N.W. to S.S.E.
Auroral (?) luminosity in N.N.W., whence the streaks seemed
to radiate. No streamers. Though dark-looking, the cirrostrati
only dimmed the stars under which they passed. The display
was very striking and beautiful.

"1851. Jan. 3, 10\text{th} p.m. A peculiar luminous appearance in
N.E., from the horizon up to Coma Berenices. It somewhat
resembled in appearance the zodiacal light, but was not so well
defined. Symptoms of the formation of cirrostrati were exhib-
ted in various parts of the sky at the same time.

"Feb. 7, 10\text{th} p.m. A luminous haze over all the sky above
the dense clouds. For a time the intensity of the light was
greater than I ever before noticed from such a cause. There
were also a few flashes of sheet lightning. Soon after a gentle
rain began to fall; wind S.W.

"Sept. 29, 11\text{th} p.m. Hazy luminosity over all the sky, under
which cirrostrati ranging N.N.W. to S.S.E., and passing into
cirrocumuli and luminous curdy-looking cumuli. Faint aurora,
without streamers.

"1852. Feb. 19 to 27. Some remarkable displays of the aurora
borealis and other meteoric appearances. The following are a
few brief notices of the more striking features exhibited, as seen
from this locality.

"On the night of the 19th of February the aurora surpassed
in splendour any display which has been observed here since the
memorable aurora of the 17th of November 1848. The lumi-
nous coruscations covered the whole of the northern half of the
sky, and the southern as far down as an arched line passing
through the constellation Leo, about a degree below Procyon
and through the lower part of Orion. The corona was beauti-
fully formed in the usual position, the streamers exhibiting the magnetic curves with great distinctness in the vicinity of the point of convergence. Streamers and masses of crimson light were mixed with those of the ordinary white and yellowish hues. Two of these masses were very persistent,—one of them situated in the west, a short way above the horizon; the other in N.N.E. at an altitude of about 45°. The motions of the streamers were very rapid, and the whole features of the display passed through an infinity of changes. Next morning a great number of light fleecy cirrus clouds were scattered over the sky. About 3rd p.m. these had become more compact, passing into cirroscumuli, and were arranged in “meridional lines” from N.N.W. to S.S.E. There is every reason to believe that these had some connexion with the auroral display of the preceding evening. Faint aurora also appeared on the evenings of the 20th and 21st.

"On the 22nd, at a few minutes before 8th p.m., a remarkable meteor exploded behind some dark clouds in the N.N.E. part of the sky, giving out for two or three seconds a light almost rivaling that of day. This meteor was also observed at Banff, where it appeared in the S.E.; at Dundee, where it appeared in the E.; at Melrose, where it appeared about N.E., and in many other places. A comparison of the notices of it contained in the newspapers would appear to indicate that it must have exploded over a part of the German Ocean, situated somewhere about 30 or 40 miles east from the Forfarshire coast,—its altitude being then probably about 40 or 50 miles. It would be interesting to ascertain if the phænomenon was observed by the crews of any coasting vessels that happened to be at that time near the locality indicated.

"On the 24th of February, at about 10th 45th p.m., a very singular isolated auroral cloud appeared in the constellation Perseus, extending several degrees towards the Pole Star and the Pleiades, to which respectively its major axis pointed. It disappeared and reappeared several times, always occupying at each successive reappearance nearly the same position and dimensions. No auroral appearance was visible in any other part of the sky.

"On the 27th, between 8th and 9th p.m., an arched aurora extended up to about 25° above the N.N.W. point of the horizon. In the southern part of the heavens an auroral band crossed the magnetic meridian at right angles, passing by Deneb, under Procyon, and through the lower part of Orion. A highly interesting circumstance connected with this was the formation of white fleecy cumulus clouds in the region about Canis Minor, where the auroral band referred to was generally most distinct, although it occasionally disappeared and reappeared like that seen in the constellation Perseus on the night of the 24th. These
clouds after their formation moved gradually to the south, where they soon disappeared and were succeeded in the same manner by others. These phenomena took place in a part of the sky otherwise quite clear.

"April 21, 0\textsuperscript{h} 30\textsuperscript{m} A.M. Luminous cirri scattered over the sky. Faint auroral pulsations flashing up to the coronal point, which was occasionally distinctly marked. During the day the sky was well covered by cirri and cirrocumuli. A dark haze extended to about 30° above the horizon; wind gusty, E.S.E. 8\textsuperscript{h} P.M. Cirrus and cirrostratus streaks extended across the sky from W.N.W. to E.S.E. After dusk the cirri became decidedly luminous. 10\textsuperscript{h} to 11\textsuperscript{h} P.M. Luminous sheets flashing wildly up to the zenith, chiefly from W.N.W., along the line of the cirrus and cirrostratus streaks, the latter showing very dark, and the former acquiring an additional fitful brightness. Pulsations also proceeded from the N.N.W. point. No appearance of a distinct corona. The flashes were fainter than those of the aurora generally are, and no well-defined 'streamers' were seen. The principal seat of the phenomenon was also more westerly than usual. Stars shone faintly through the luminous cirri. 11\textsuperscript{h} 30\textsuperscript{m}. The cirrus and cirrostratus bands approaching nearer the magnetic meridian than before.

"April 24. During the day wind E., gusty; sky clear. 9\textsuperscript{h} 40\textsuperscript{m} P.M. Clear, with the exception of an auroral band about 10° broad, extending from a point a short way to the west of the magnetic meridian (which it crossed between 45° and 55° above the N.N.W. point) to about midway to the E.N.E. point. No other auroral appearance visible. It continued nearly in the same position, though frequently disappearing and reappearing, till 10\textsuperscript{h} P.M. It was affected by rapid pulsations. At 10\textsuperscript{h} P.M., immediately after its final disappearance, a slender luminous streak suddenly appeared, extending from the place where the former phenomenon intersected the magnetic meridian to near the ordinary coronal point. This soon assumed a cirrocumulus or rather compact cirrus appearance, grew broader, and moved slowly from nearly E.S.E. to W.N.W. Its length lay nearly N.N.W. to S.S.E. Some other luminous patches then appeared scattered over the sky. 10\textsuperscript{h} 30\textsuperscript{m}. The cloud formed as above described now much broader, and resembling a compact cirrocumulus. 11\textsuperscript{h} P.M. Broad luminous bands of thin cirri extended from N.N.W. to S.S.E.; wind N.E., moderate. 25th. During the forenoon bands of cirri passing into cirrocumuli (same as last night) extending from about N.W. by N. to S.E. by S., and moving from about E.S.E.; wind E.

"Nov. 11, 9\textsuperscript{h} P.M. Wild and irregular gusts from N.E. Aurora up to at least 30°, but clouded underneath. Cirrostrati
from E.N.E. to W.S.W., and under them towering masses of cumuli, presenting a strange and wild appearance. The auroral light nearly equalled that of the moon when similarly clouded.

"1853. Jan. 7, 7th to 10th P.M. Sky covered by thin hazy streaks of cirri, much resembling faint auroral clouds, and like these only dimming the stars. They extended across the sky from N.by E. to S.by W., and were occasionally traversed by pulsations, apparently from W., but too rapid to admit of this being satisfactorily determined. Wind gusty from S.W. No aurora was seen except a few slender streamers, which appeared for a few minutes about 9th P.M. just above the N.N.W. point. 10th P.M. Cirri as before, but no pulsations now distinguishable. Wind not so gusty as before. No auroral appearance visible.

"March 12, 10th P.M. Clear and frosty, faint auroral light in N.N.W. A faint luminous streak extended from Orion's belt by Pressepe to the head of Leo. This soon disappeared, and shortly afterwards reappeared in the same position. An arched bank of clouds, apparently cirrostrati, in N.N.W. In a few minutes broad, dark, hazy cirrostratus bands appeared in N.W., then overhead, and soon after over the whole sky, crossing the magnetic meridian at right angles."

V. On the Electrical Currents induced in the Metallic Cross.
By Richard Adie, Liverpool*

The following communication has for its object to show that, in a metallic cross made of one metal, when there is a want of homogeneousness, or other source of resistance to conduction, at the crossing of the bars in their centres, a galvanic current passed through one pair of the arms induces in the other pair of arms an electrical current, which has no connexion with unequal heating or thermo-electrical effects. The experiments appear to me to give a very pretty illustration of a series of electrical currents, corresponding in their intensity to those derived from thermal sources, but produced by a want of homogeneousness in the conducting materials, and without the aid of unequal heating, which for thermo-electricity is indispensable.

In the January Number of this Journal, vol. v. p. 46, I gave an explanation of the cross, founded on experiments which I had published some years previously in the Edinburgh Philosophical Journal. As I again wish to return to the subject, I may preface my remarks by stating for the information of those who may not be familiar with the matter, that the metallic cross was designed on the Continent several years ago on purpose to show

* Communicated by the Author.
the reduction of temperatures of metallic joints by electrical currents; and as it readily gave a galvanometer indication corresponding with the effect that would be derived from cooling the central joint by artificial means, the experiments with this cross have been much used to prove the reduction of the temperatures of joints by electrical currents. On this head the experiments I published in the April Number, vol. v. p. 193, to show that the caloric effects in the cell of a battery may be exhibited in the joints of a metallic circuit in connexion with it, explain the reduction of the temperatures of these joints in a manner consistent with recognized laws; while the facts shown in the present experiments give a new explanation to the phenomena presented to us in the metallic cross. The annexed diagram represents the cross, in which AC and BD are two bars of metal soldered or fastened in good metallic contact at E. From the extremities of these bars at A and D wires were used to form a connexion with Smee's batteries; the other extremities of the bars at B and C were by wires connected with a galvanometer. When the electricity from a battery was made to circulate in the direction AED, AC being the bismuth bar, an electrical current was generated in the other pair of arms moving in the direction BEC, or opposite to the primary current; being the same in effect as would be obtained by cooling at E by artificial means,—a result which was uniform for all degrees of intensities of primary currents. On my recent return to these experiments I verified this observation, and found that, change the intensity and quantity of the current how you will, so long as it moved in the direction AED, the action on the galvanometer was the same as if the joint E was cooled. I felt certain that many of the currents I had passed had heated the metals at E; and to try this point, a delicate thermometer was tied down on the central part E of a bismuth and antimony cross. A feeble galvanic current from a single pair of Smee's plates, acted on by a battery a little lower in temperature than that of the atmosphere, reduced the thermometer on the joint 1°; but when the extent of the surface of the battery in action was increased, the thermometer rose above the atmospheric temperature; and with a further increase of surface of battery in action, there was a rise of temperature till the heat of the joint stood 15° higher than that of the surrounding atmosphere. Now while this was the case, the galvanometer indicated that the joint E was cooled, consequently it did not represent the thermo-electrical current generated there. When formerly en-
gaged with the subject, I thought that the heating power of the primary current on entering a bad conductor like bismuth at \( A \) might be the cause of the galvanometer deflection; but on my return to the experiments, in consequence of the objection made to them by Dr. Tyndall, I found I could not verify this view by artificial heating at \( A \). I therefore began to doubt if the effects observed in the metallic cross had anything to do with thermo-electrical currents; and to try this point, I made a cross of copper. The central joint \( E \) was effected by tying the bars firmly together with cord, the surfaces in contact being previously cleaned; the other connections with it were the same as described in the diagram. In this arrangement the current from the battery had a circuit through \( \text{AED} \) of copper only, and yet there was generated in the other arms a current moving in the direction \( \text{BEC} \) which deflected the galvanometer \( 38° \); the action being nearly the same as I had obtained from a bismuth and antimony cross, soldered at their centre, but otherwise under like conditions. An experiment was next made with the copper cross soldered with tin at the centre \( E \); this produced a deflection of \( 6° \). A cross of copper was then prepared with the metal at the centre as nearly homogeneous as possible; from it no permanent effect on the galvanometer could be obtained. These three experiments show clearly that it is the nature of the joint which the primary current has to cross at \( E \) that governs the current induced in the outer pair of arms \( \text{BE} \) and \( \text{CE} \), and that it is not a thermo-electrical effect, is shown by the deflection of \( 38° \), obtained when the joint consisted of two like pieces of copper tied together.

From crosses made of other metals the following indications were obtained:

<table>
<thead>
<tr>
<th>Material</th>
<th>Deflection</th>
</tr>
</thead>
<tbody>
<tr>
<td>Copper cross centre joint</td>
<td>none</td>
</tr>
<tr>
<td>homogeneous metal</td>
<td></td>
</tr>
<tr>
<td>...</td>
<td></td>
</tr>
<tr>
<td>... tin soldered at ( E )</td>
<td>( 6° )</td>
</tr>
<tr>
<td>... tied with cord ( E )</td>
<td>( 38° )</td>
</tr>
<tr>
<td>Antimony-bismuth cross</td>
<td>( 35° )</td>
</tr>
<tr>
<td>soldered at ( E ), primary</td>
<td></td>
</tr>
<tr>
<td>current passed in the</td>
<td></td>
</tr>
<tr>
<td>direction ( \text{DEA} )</td>
<td></td>
</tr>
<tr>
<td>... current passed in the</td>
<td></td>
</tr>
<tr>
<td>direction ( \text{AED} )</td>
<td>( 40° )</td>
</tr>
<tr>
<td>Lead cross tied with cord at</td>
<td>( 64° )</td>
</tr>
<tr>
<td>( E )</td>
<td>( 68° )</td>
</tr>
<tr>
<td>Zinc</td>
<td>( 70° )</td>
</tr>
<tr>
<td>Antimony</td>
<td>( 71° )</td>
</tr>
<tr>
<td>Iron</td>
<td>( 76° )</td>
</tr>
<tr>
<td>Bismuth</td>
<td>( 46° )</td>
</tr>
<tr>
<td>Bismuth cross soldered at (</td>
<td></td>
</tr>
<tr>
<td>( E ) with tin</td>
<td></td>
</tr>
</tbody>
</table>

In these experiments a cross composed of two bars of bismuth tied at \( E \), shows a much more energetic current than the anti-
mony and bismuth cross soldered, and even the soldered bismuth bars are superior to the original form.

The change in the direction of the primary current in the results from the antimony and bismuth cross, where AC represented the bismuth bar, show how much the induced current is influenced by the thermo-electrical current developed at E; for with the primary current passing in the direction DEA, the induced effect at E is opposed to a thermo-electrical current generated by heat there; with the primary moving in the direction AED, the induced and the thermo-electrical current both generated at E are in the same direction. The effect of the thermo-electrical current opposing or exalting the induced current, is to change the deflection of the galvanometer 5°; a fact which is strong evidence of how little in a bismuth and antimony cross thermo-electricity has to do with the results.

Although it is easy to construct a cross of copper which will give no permanent current in the second pair of arms BEC, I fear that this would not be practicable with bismuth, it is so exceedingly liable to variations in density in different parts of the castings.

In conducting these experiments, the immediate effect of the primary current in generating the induced one shows that unequal heating of the bars is not requisite; for this requires time, while the induced current is at its highest the instant the primary circuit is completed.

These feeble induced currents are clearly due to a slight interruption or resistance the main current meets with while passing from one arm of the cross to the other. The three experiments with the copper cross show this; but in a subsequent set of three with a zinc cross, the evidence was still more marked.

\[
\begin{align*}
\text{Deflection.} & \\
\text{Cross of rolled zinc tied with cord at centre, and} & 72 \\
\text{surfaces slightly oxidized} & \\
\text{Cross of rolled zinc tin soldered, superficies sold-} & 25 \\
\text{dered '002 of a superficial inch} & \\
\text{Cross of rolled zinc tin soldered, superficies sol-} & \\
\text{dered '073 of a superficial inch} & \text{no deflec-}
\end{align*}
\]

A friend has suggested that these currents in the other arms of the cross may be derived from the main current without induction, or, in fact, are a portion of it. To try this, I constructed a bismuth cross of long and short arms, where the main current had to traverse two inches of the bad conducting metal, and the other current had fourteen inches of the same to pass. In such an arrangement the main or battery current would experience a

*Phil. Mag. S. 4, Vol. 6, No. 36, July 1853.*
great additional resistance if a portion of it took the outer arms of the cross for its path, and consequently we should look for a diminished action in the galvanometer when the arms are so different; yet in the trial I made, I found the induced current in the long arms apparently exalted above the effect derived from a similar cross of equal arms of 2 inches long in each arm.

VI. Analysis of Hayesine. By Allan Dick, Esq., Metallurgical Laboratory, School of Mines*.

This mineral occurs in the nitre beds of Peru, often in the form of rounded masses about the size of a walnut, composed of interlacing silky crystalline fibres. If kept in a closed vessel a short time, it emits a slight odour like that of iodine, but distinct from it. It showed no action on a piece of starch paper which was enclosed for twenty-four hours in the bottle containing it (it may possibly be due to some compound of iodine or chlorine and oxygen). It has been described by Ulex (Ann. Pharm. lxx. 49) who assigned to it the formula of $\text{NaO}, 2\text{CaO}, 5\text{BO}_3 + 10 \text{aq}$. In Peru it is known by the name of Tiza.

15.60 grains were dissolved in dilute hydrochloric acid, and filtered from a few sandy particles which weighed 0.05.

The lime was precipitated as oxalate, filtered off, and converted into carbonate by ignition, which weighed 3.97.

Hydrochloric acid and chloride of barium were added to the filtrate; the sulphate of baryta obtained weighed 0.50.

10.53 grains were dissolved in dilute nitric acid, filtered, and nitrate of silver added; the chloride of silver weighed 0.69.

10.86 grains were mixed with about 25 of pure fluor spar in a platinum crucible, drenched with sulphuric acid, and heat applied till the excess of acid was expelled; the residue was dissolved in hydrochloric acid, and chloride of barium added; the excess of baryta was precipitated from the filtrate by carbonate of ammonia, and the alkalies determined in the usual manner, weighed as chlorides; the potash was estimated by bichloride of platinum.

Weight of mixed chlorides . . . 2.06
Weight of potas. chlor. of platinum 0.29

5.40 grains were mixed with some dry carbonate of soda and placed in a small tube closed at one end; this was connected to a weighed chloride of calcium tube, and the heat of a spirit-lamp was applied to the tube containing the mixture.

Weight of water 1.47.

Communicated by the Author.
On a new Oxide of Hydrogen and its relation to Ozone.

It also contained nitric acid, apparently under 1 per cent., but this was not accurately determined.

Numerous experiments were tried to obtain some method of determining the boracic acid. Although no good one was found, I think a close approximation to the real quantity might be obtained by adding, drop by drop, a standard solution of chloride of barium or calcium as long as a precipitate is formed; for as soon as the liquid has become saturated with the borate, a very small quantity of the precipitant occasions a distinct precipitate. Circumstances prevented my continuing these experiments as I should have liked.

The hygroscopic water was not determined.

Results tabulated.

<table>
<thead>
<tr>
<th>Substance</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lime</td>
<td>14.32</td>
</tr>
<tr>
<td>Soda</td>
<td>8.22</td>
</tr>
<tr>
<td>Potash</td>
<td>0.51</td>
</tr>
<tr>
<td>Sulphuric acid</td>
<td>1.10</td>
</tr>
<tr>
<td>Chloride of sodium</td>
<td>2.65</td>
</tr>
<tr>
<td>Sand</td>
<td>0.32</td>
</tr>
<tr>
<td>Water</td>
<td>27.22</td>
</tr>
<tr>
<td>Iodine and phosphoric acid</td>
<td>trace</td>
</tr>
<tr>
<td><strong>Boracic and nitric acids by loss</strong></td>
<td><strong>45.46</strong></td>
</tr>
</tbody>
</table>

It is difficult to say whether the sulphuric acid is combined with lime or soda, or both, as both sulphate of lime and soda are found in the same district; and glauberite (NaO SO$_3$ + CaO SO$_3$) occurs crystallized in the very centre of many of the rounded masses of Hayesine. It is most probable that it occurs as the latter compound, a few small crystals of which might have escaped detection in the selection of the sample. It will be seen that this analysis, though imperfect, points to the formula given it by Ulex.

VII. On a new Oxide of Hydrogen and its relation to Ozone.

By Dr. M. Baumert*.

[With a Plate.]

The following investigation is intimately connected with the volatile body which makes its appearance during the electrolysis of water, and which is usually known by the name of ozone. As this, although without any positive proof, is gene-

rally regarded as identical with the substance formed during the passage of electrical sparks through gases containing oxygen, it was necessary to take the latter into consideration at the same time. Nevertheless I abstain from giving any opinion in this memoir upon the ozone produced by other processes than by electricity; and the more so, as the great difficulties presented by investigations of this kind render it above all things desirable only to introduce such facts as admit of a certain and unequivocal interpretation.

As ozone makes its appearance during the electrolysis of water, independently of the substances added to render the fluid capable of conducting the current, we may conclude that this substance can contain nothing but hydrogen and oxygen. The quantity of ozone formed certainly varies according to the substances dissolved in the water, but its formation is never suppressed, whether sulphuric, phosphoric or chromic acids, or other substances, especially such as acquire a higher degree of oxidation with difficulty, are added to the water.

That hydrogen as well as oxygen is contained in the ozone obtained by electrolysis, may be proved in the most incontestable manner by the following experiment. The inside of a long, narrow glass tube is lined with a thin coating of anhydrous phosphoric acid. This is best effected by allowing a stream of dry air to carry the newly-formed products of the combustion of phosphorus through the tube. If perfectly dry ozone be allowed to pass through this tube, the phosphoric acid will remain unchanged; but if the middle of the tube be slightly heated so as to decompose the ozone, the water formed dissolves the anhydrous phosphoric acid beyond the flame in the direction of the stream of gas, whilst the phosphoric acid before the flame undergoes no change.

If thus the separation of water from this substance admits of no doubt, on the other hand it may be asserted with equal certainty that these elements of water are united with oxygen, and not with hydrogen, to form ozone; for this substance possesses such an oxidizing power, that even chloride of potassium is decomposed by it, with the evolution of chlorine and the formation of caustic potash. An excess of hydrogen, on the contrary, could only be accompanied by reducing properties.

This behaviour affords a means of ascertaining the composition of ozone; for if a method of determining the oxygen which is combined with the elements of water to form ozone be given, it would only be necessary to weigh the ozone or its collected products of decomposition, to render its composition certain. Such a means is presented by a process which is essentially the same as that employed by Prof. Bunsen in his investigation of iodide of ni-
The oxygen united with the elements of water separates an equivalent of iodine from a solution of iodide of potassium, like free chlorine or bromine. All that is now necessary is the determination of this separated iodine, which may be effected in the following manner with remarkable exactitude.

A solution of iodine in iodide of potassium is prepared of such a strength, that the liquid in one degree of the burette may contain a small fraction of a milligramme, perhaps, as in the following investigations 0·0002538 grm. of chemically pure iodine = a. Some litres of extremely dilute sulphurous acid are also to be prepared, to contain not more than \( \frac{3}{100000} \) of sulphurous acid. It must now be ascertained how many degrees of the burette \( t \) of the solution of iodine are required to neutralize a measured volume of this sulphurous acid. The quantity of iodine necessary is consequently at. If the quantity \( n \) of this sulphurous acid be added to the solution of iodine obtained by ozone, which contains the unknown quantity of iodine \( x \), this \( x \) neutralizes a portion of the sulphurous acid. By ascertaining now the number of degrees of the burette \( t' \), and by this means also the quantity of iodine \( at' \) which is necessary for the complete neutralization of the measure \( n \) of sulphurous acid, the equation \( x + at' = nat \), or \( x = a(nt - t') \) is obtained. The point where the neutralization of the acid has taken place may be readily determined by means of solutions of starch, according to the process described by Dupasquier. From \( x \), or the quantity of iodine set free by the oxygen of the ozone, we arrive at the equivalent quantity of oxygen, \( w \), contained in the ozone, together with the elements of water, by the readily intelligible equation

\[
\frac{O}{J} a(nt - t') = w.
\]

As the entire weight of the ozone, less this oxygen \( w \), consists of water, it is only necessary now to determine this total weight. This is effected simply by weighing a small globe apparatus, through which strongly dried ozone is passed, and in which its products of decomposition are retained in the solution of iodide of potassium, and the water in a sulphuric acid tube attached.

The following may serve as a proof of the accuracy of this method. In the three following experiments the neutralization of the same measure of sulphurous acid required the following quantities of the diluted test-fluid, which only contained \( a = 0·00002538 \) grm. of iodide in a degree of the burette \( t \):

<table>
<thead>
<tr>
<th>Variation from the average.</th>
</tr>
</thead>
<tbody>
<tr>
<td>t.</td>
</tr>
<tr>
<td>----</td>
</tr>
<tr>
<td>I.</td>
</tr>
<tr>
<td>II.</td>
</tr>
<tr>
<td>III.</td>
</tr>
<tr>
<td>Average</td>
</tr>
</tbody>
</table>

\( t \) = variation from the average.

\( at' \) = average.
The greatest divergence from the mean of these experiments therefore amounts only to 0·00001 of iodine, and represents an error of only \( \frac{6}{10,000,000} \) of oxygen. The chances of error in the determination of the oxygen is thus, according to these experiments, so far reduced, that the quantity of this element may be determined even in less than the tenth of a milligramme of ozone.

The errors most likely to creep into this method must consequently be in the weighing; for this reason the operation was performed with particular care. A new Ertling’s balance was employed, which was never made use of for any other purpose during the whole course of the experiments, by means of which even the tenth of a milligramme might be determined with certainty. The balance was placed in an unheated place, the temperature of which was pretty constant. The preliminary weighing before the experiment, as well as that after it, was two or three times repeated, when the apparatus, protected from the access of moisture, remained for a considerable time in the closed balance exposed to a constant temperature. The difference of temperature observed during the weighings did not exceed 5°·4 F., whilst the weight of the apparatus was 36 to 38 grms., so that the influence of the temperature might be disregarded. On the other hand, the greatest care was taken to displace the oxygen remaining behind from the passage of the electrolytic gases by a stream of dry air before the weighing. In this manner it was possible to accomplish the weighings with accuracy to a few tenths of a milligramme. This represents an immaterial error as regards the composition to be ascertained.

The preparation of ozone presents greater difficulties than its analysis. Water acidulated with sulphuric acid, and decomposed by the electric current, does not furnish sufficient ozone to be weighed with precision. On the other hand, by the electrolysis of chromic acid containing sulphuric acid, such quantities are evolved, that Vulcanized india-rubber tubes of a line in thickness are destroyed by it in a few minutes; and the stream of oxygen bearing the ozone with it forms white clouds with ammonia gas, which on closer examination are found to consist of nitrate of ammonia. It would, however, be a great mistake to suppose that ozone is to be obtained pure in this manner; its quantity, in spite of this powerful action, is so small, that in the whole of the following experiments it was necessary to evolve not less than from 700 to 800 litres of electrolytic gas. In one experiment, in which 76 grms. of water acidulated with \( \frac{1}{10} \)th of sulphuric acid were decomposed, 150 litres of explosive gas were accompanied by 1 milligramme of ozone. Solutions of chromic acid, which still contained some sulphuric acid from its preparation, furnished 1 milligramme of ozone to 10 litres of gas in two
nearly corresponding experiments. A further difficulty also occurred at the commencement of the investigation: it regards the connexion of the various pieces of apparatus. This can neither be effected by cork, india-rubber, or any other organic substance. It is necessary therefore either to fuse together the separate pieces of glass tube forming the apparatus, or where this is not practicable, to unite them by grinding them one into the other. The presence of free hydrogen in the gas containing the ozone must also be avoided, for reasons which I will give hereafter. Lastly, even the proportions of the apparatus are confined within certain limits; the slowly moving stream of gas requires a space of time sufficient for the spontaneous decomposition of a considerable quantity of ozone before arriving in the solution of iodide of potassium.

Plate I. fig. 1 shows the apparatus employed in the experiments. It is divisible into three parts; the first (fig. 2) serves for the evolution of the oxygen containing the ozone, the second (fig. 3) serves to dry the gas, and the third (fig. 1a) is for the reception of the products of the decomposition of the ozone.

A wide tube (fig. 2a), which is intended for the reception of the porous cell b, has smelted to it another tube, such as is used for the ordinary evolution of gas. About an inch above its lower end a platinum wire, to which a platinum plate is attached, is fused in. The latter serves as the positive pole, and reaches down into the porous cell, which is open at both ends. A second tube (fig. 3), which may be conveniently slipped over the preceding, is united by a thinner tube e to the drying apparatus f (comp. fig. 1). In this case also the separate pieces of tube are fused together. This second tube (fig. 3) rests with its lower end upon the funnel-shaped enlargement a of the first tube, fig. 2; its upper end projects about an inch beyond the top of the first tube. The drying-tube represented at f (fig. 3) has the inclination shown in fig. 1; it is 3 feet long, and has the diameter of a wide combustion-tube. It contains pumice-stone saturated with chemically pure concentrated sulphuric acid. To get rid of any sulphurous acid, the smallest quantity of which would frustrate the whole experiment, the pumice-stone is previously moistened with pure sulphuric acid, and repeatedly heated to redness throughout. It fills the tube up to the anterior end, whilst the sulphuric acid collects in the hinder portion, which is bent down, and even runs back in part into the narrow connecting tube. In this manner, the gas, after passing the stratum of sulphuric acid, forms bubbles, which pass gradually through the fragments of pumice-stone. Over the anterior end of the drying tube a closely fitting cap (fig. 4) is now passed. This consists of a wide test-tube, to which a short piece of narrower tube has been attached
by fusion; the latter serves for the reception of the apparatus \( k \) (fig. 1), the office of which is to collect the products of the decomposition of the ozone. This apparatus also consists of two parts united by a glass tube closed with two corks. The first globe apparatus contains a concentrated solution of iodide of potassium; the other, bent backwards, is filled with concentrated sulphuric acid, for the purpose of retaining the water carried along by the oxygen: at one end it is ground into the short tube of the cap. Before the commencement of the operation, the union between it and the glass cap, as well as that between the latter and the drying tube, must be rendered air-tight; for this purpose the parts are enveloped in a piece of platinum foil an inch broad, and this is covered with a thick layer of shell-lac reaching up to the glass. At the opposite end of the globe apparatus, from which the oxygen, after passing through the sulphuric acid, is again expelled, an india-rubber tube, dried by a stream of dry air, and united with a chloride of calcium tube, is attached; this is omitted in the figure. Lastly, the globe apparatus is protected from dust during the whole course of the operation by means of a cloth.

The fluid employed for the electrolysis is placed in a cylinder, \( k \) (fig. 1); the height of this column of fluid is proportioned to the pressure to be overcome in the various parts of the apparatus.

This arrangement of the whole apparatus presents great advantages as regards the putting of it together. When the drying tube with its attached tube is firmly placed, the tube, fig. 2, is passed up into the latter (fig. 3) from below. To prevent it from sinking, the wire of the positive pole may be held tight with a caoutchouc ring, \( l \) (fig. 1). The porous cell is then attached, and the cylinder is also passed up from below; this is placed in a refrigerator, \( m \) (fig. 1). The cylinder is then filled, and finally the negative pole \( n \) (fig. 1) sunk into the fluid. When the evolution of gas has proceeded far enough for the removal of any moisture, the weighed globe apparatus, containing the solution of iodide of potassium, is attached to the cap in the manner already described.

As may be seen from the figure, only the gas produced at the positive pole passes through the apparatus. In order to obtain only tolerably ponderable quantities of ozone, disproportionately large volumes of gas must pass through the apparatus. It was consequently of great importance to ascertain whether the drying tubes before and behind the iodide of potassium were sufficient to retain every trace of moisture from the gases passing through them. For this purpose 150 litres of electrolytic gas, from which the ozone had been removed, were passed through the apparatus. The globe apparatus containing the iodide of potassium gained only 0.0004 grm. in weight during this process.
In this experiment, as well as in the others, the current produced by six carbon and zinc elements renewed twice daily, was regulated by means of an interposed tangent galvanometer. The absolute intensity of the current, reduced to Gauss's standard, varied during a week's experiments between 10.6 and 21.8. With this the gas passing through the apparatus moved sufficiently slowly to become completely dried.

After these remarks, I may turn to the analysis itself.

As has already been mentioned, ozone acts in such a manner upon the solution of iodide of potassium, that the oxygen which is combined with the elements of water to form ozone sets free its equivalent of iodine, whilst caustic potash is formed. In consequence of this, the solution of iodide of potassium is seen to acquire a brown colour after a short time. This colour, however, disappears again soon, in consequence of the separated iodine acting again upon the potash which has been formed, causing the formation of iodate of potash and iodide of potassium. After the electrolytic oxygen has been passed through for some time, a crystalloidal coating of separated iodate of potash, mixed with iodide of potassium, appears on those parts of the apparatus which have become dry. If muriatic acid be added to such a solution, which contains free iodine as well as iodate of potash and iodide of potassium, the iodine of the iodate of potash is completely separated, with formation of water, as shown by the following experiments.

9.2808 grms. of pure dry iodine were weighed between two watch-glasses and dissolved in solution of iodide of potassium. The hundredth part of this solution, which consequently contained 0.092808 of iodine, was mixed with caustic potash until the disappearance of the brown colour, and then with muriatic acid until its reappearance. A certain quantity of sulphurous acid added to this solution required for its perfect destruction $at' = 0.0214$ of iodine. The same quantity of sulphurous acid by itself required $at = 0.1134$ of iodine. The quantity of iodine found consequently amounted to 0.092, which only differs from the quantity employed by about $\frac{3}{10}$ of a milligramme. In a second experiment $at' = 0.0209$ of iodine was obtained, which corresponds with 0.0925 grm. of iodine, and consequently shows a difference of only $\frac{3}{10}$ of a milligramme. As the only intention of these experiments was to prove the complete decomposition of iodate of potash by iodide of potassium in the presence of muriatic acid, I bestowed no particular care upon the accuracy of this quantitative determination. If the fluid be measured more exactly, or still further diluted, the chance of error will be confined within equally narrow limits, as in the preceding experiments. The error may then be reduced to only the hundredth of a milligramme.
It is seen by this, that, whether the separated iodine remains uncombined, or gives rise to the formation of iodate of potash, it may in either case be determined with equal accuracy. The ozone employed for the analysis was produced from solutions of chromic acid, upon the preparation of which the greatest care had been bestowed. It was particularly necessary to get rid of every trace of muriatic acid, which might have given rise to the formation of chlorine. In the first experiment, the water, with the addition of some chromic acid and a little sulphuric acid, was decomposed by the uninterrupted action of the current for six days. In this case the following elements of calculation were obtained:

Total weight of the ozone \( A = 0.0133 \) grm.

\[
\begin{align*}
  a &= 0.00025387 \\
  t &= 55.1 \\
  t' &= 44.4 \\
  n &= 10
\end{align*}
\]

From this, calculation gives \( w = 0.008109 \)
and \( A - w = 0.005191 \).

A second experiment, instituted under similar circumstances, in which, however, the current was in action for eight days, gave—

Total weight of the ozone \( A = 0.0149 \) grm.

\[
\begin{align*}
  a &= 0.00025387 \\
  t &= 246.5 \\
  t' &= 121.8 \\
  n &= 3
\end{align*}
\]

Calculated from this, \( w = 0.009887 \)
and \( A - w = 0.005013 \).

From these numbers the formula \( HO_3 \) is derived, as the most simple expression of the composition of ozone.

If we now express the composition of ozone in per-centage numbers, we obtain—

<table>
<thead>
<tr>
<th></th>
<th>Found.</th>
<th>Calculated.</th>
<th>I.</th>
<th>II.</th>
<th>Average.</th>
</tr>
</thead>
<tbody>
<tr>
<td>H</td>
<td>4.00</td>
<td>4.34</td>
<td>3.76</td>
<td>4.00</td>
<td></td>
</tr>
<tr>
<td>( O_3 )</td>
<td>96.00</td>
<td>95.66</td>
<td>96.24</td>
<td>96.00</td>
<td></td>
</tr>
</tbody>
</table>

In this manner we get the following series of oxides of hydrogen:

- \( HO = \) water.
- \( HO_2 = \) peroxide of hydrogen.
- \( HO_3 = \) ozone.

From the position occupied by ozone in this series, it might be regarded as an acid, which might be not inappropriately named hydrogenous acid. In this view it presents some analogies with nitrous acid. One circumstance, which sometimes
takes place with nitrous acid, appears always to occur with ozone—its decomposition when brought directly in contact with bases. By its chemical properties, ozone is still more strongly characterized as a superoxide of hydrogen. Thus metallic oxides capable of a higher degree of oxidation, and which under these circumstances form either peroxides or acids, become converted into peroxides and acids by contact with ozone. Ammonia becomes converted into nitrate of ammonia, and mercury into its oxide, without entering into any saline combination. It appears to be without any action upon the metallic oxides of the magnesian group. The colour of litmus is destroyed by it without any preliminary reddening. Lastly, in the presence of readily oxidizable substances, ozone gives up its oxygen to the latter, whilst water is separated. This property renders it necessary to exclude the hydrogen formed during the preparation of the ozone by the electrolysis of water. Experiments, in which explosive gas was allowed to pass through the apparatus above described, always furnished an excess of water, and as in these experiments the other conditions were unchanged, this formation of water can only be explained by supposing a direct combination of the free hydrogen with the oxygen existing in excess in the ozone.

From all that has been stated above, no doubt can well remain that hydrogen exists as an essential element in ozone. If we consider the circumstances under which the gas passed through the apparatus, we must arrive at the conclusion, that the hydrogen found could not arise from watery vapour which had been carried over; for before its arrival at the globe apparatus, it had passed through a sulphuric acid tube five times as long as that through which it finally issues. The globe apparatus might therefore weigh less than would correspond with the weight of the oxygen as found by testing with a normal solution, but never more. Another objection which might be urged against these experiments is founded upon the tension of the vapour of sulphuric acid, which might possibly be sufficient to allow free sulphuric acid to be carried over with the gases into the globe apparatus. To refute this objection, I drove 50 litres of atmospheric air through the drying tube in a powerful stream, attaching the globe apparatus filled with distilled water. Not the slightest turbidity was produced by the addition of chloride of barium after the completion of the operation.

In consequence of the uncertainty which exists as to the identity of the substances included under the name ozone, it must always appear doubtful whether the body which has been obtained by the passage of electric sparks through dried oxygen gas is really the compound which has just been investigated. Its formation under these circumstances could only be explained
by the supposition that the dried gas still contained traces of moisture. But if, on the other hand, it be proved that the odorous substance produced by the electric spark results from its action in an atmosphere perfectly free from watery vapour,—that by an excessive, although instantaneous elevation of temperature, such as is produced by the electric spark, oxygen passes into an allotropic state, characterized by an increased capacity of combination, this would establish an extremely simple relation between it and the ozone, which has just been shown to be a superoxide of hydrogen. The latter would then be nothing else than water oxidized at the expense of the allotropic oxygen, and would therefore always be produced wherever this allotropic oxygen comes in contact with water at a low temperature; so that the allotropic oxygen is even indebted to this formation of ozone for its odour. The decision of this question lay therefore in the direction of the proof that oxygen through which the electric spark had passed really possessed a capacity of combination, during the complete exclusion of water, sufficiently great even at ordinary temperatures to overcome the strongest affinities, such as those of chlorine and iodine for potassium. It was consequently above all things necessary to determine how far the vapour of water may be removed from an atmosphere of oxygen. The simplest and most certain means of arriving at this was presented by the above determination of the constitution of ozone, and the reaction produced by a film of phosphoric acid. For this purpose, a long, narrow glass tube was covered with a filmy coating of anhydrous phosphoric acid, in the manner described in the commencement of this paper. When a gas containing only traces of watery vapour passes through this tube, the film disappears in the direction of the stream of gas, in consequence of the white microscopic particles of the phosphoric acid becoming transparent by their combination with water. It may be proved that in this manner even a few ten-thousandths of a milligramme of watery vapour may be rendered visible. From experiments already described, it appears that ozone is split into water and oxygen by a moderate elevation of temperature, and that the water separated by this decomposition from a cubic centimetre of electrolytic oxygen gas, may be distinctly shown by this means, although it scarcely amounts to \( \frac{1}{10,000} \) milligrm.

In order to obtain a considerable quantity of the matter to be examined, it was necessary to employ a very powerful source of electricity for the production of a stream of sparks. For this purpose I employed an excellent induction spiral of Halske and Siemens, which furnished induced currents of such power that sparks of nearly a line in length were formed at the ends of the unconnected secondary wires during the interruption of the
primary current. This interruption was, as usual, effected by the current itself; through the agency of a small hammer and an electro-magnet. The number of the sparks produced may be readily calculated from the tone caused by the vibrations of the small hammer. This tone, which naturally varies in height and depth according to circumstances, was on the average E in the great octave. The number of vibrations corresponding with this sound give eighty-two strokes in a second; this represents more than two million sparks in the course of the experiment, which lasted four hours*. The apparatus in which this stream of sparks was employed is represented in fig. 5.

The porous cell a, which is more exactly represented at fig. 6, contains the positive pole, and stands in a glass vessel filled with diluted sulphuric acid and some sulphate of iron, and kept cool from without. This receives the negative pole outside the cell, in such a manner that the hydrogen evolved is completely separated from the oxygen. The oxygen gas passes through the small globe apparatus b (fig. 5), filled with sulphuric acid, into the tube c, which is surrounded in the middle with fine platinum foil, and kept at a strong red heat by means of a Berzelius' lamp during the whole course of the experiment. By this means the ozone formed, as well as any hydrogen which might possibly have been carried away by diffusion, is converted into water. From this portion of the apparatus the stream passes into the globe d (fig. 7), filled with fragments of iodide of potassium, which is separated from the tube e, filled with powdered chemically-pure phosphoric acid, by a stopper of asbestos which has been heated to redness. The gas, which has been completely freed from ozone and watery vapour in these two pieces of apparatus, passes lastly into the small tube f, which is about half a line in diameter, and into which the platinum wires connected with the induction apparatus for the production of the sparks are fused. This part of the apparatus is united with the anterior portion, blown out of a piece of tube, in the manner previously described, and the whole tube containing the platinum wires coated with a film of anhydrous phosphoric acid. The small globe apparatus h, filled with solution of iodide of potassium, is ground air-tight into the dilated mouth g of this tube.

The following was the system followed during the experiments:—First of all, a stream of oxygen was driven at a measured speed through the apparatus for three hours by means of the polar plate; during this time the piece of tube c was kept at a red heat, and the induction spiral inactive. Neither the iodide of potassium in the globe d, nor the film of phosphoric acid in

* For every stroke corresponds to one closure and one rupture of the circuit; hence to two sparks.—Ed.
the tube $fg$, showed the least change. As the film of phosphoric acid shows even $\frac{1}{10,000}$ milligrm. of moisture, and at least 4000 cub. centims. of oxygen passed through the tube in four hours, it may be regarded as certain that 1 cub. centim. of the gas could not contain even $\frac{3}{10,000,000}$ milligrams. of moisture. The solution of iodide of potassium in the globe apparatus $h$ not only remained perfectly colourless, but showed no trace of blue on the addition of muriatic acid and solution of starch. From this we may come to the conclusion, that electrolytic oxygen gas at a low temperature neither decomposes solution of iodide of potassium, nor bears with it, under the present circumstances, any traces of ozone.

The experiment was now repeated, with the single difference, that a stream of sparks was passed between the wires fused into the tube $f$, after any nitrogen, which might possibly have passed into the anterior tube during the removal of the globe apparatus, had been displaced by the stream of oxygen. Even during the first few minutes the solution of iodide of potassium became distinctly yellow, and in the course of an hour 1 milligrm. of free iodine could be shown to exist in it. The film of phosphoric acid in the spark tube did not exhibit the slightest change during the whole course of the experiment.

The globe apparatus was now for the third time filled with solution of iodide of potassium, and the removal of any nitrogen effected as before. The experiment was repeated exactly in the same manner, the induction spiral kept in action, but the spark tube exposed to the flame of a small spirit-lamp at $i$. The solution of iodide of potassium now did not exhibit the slightest yellow colour; and even after the lapse of an hour, no blue colour could be seen on the addition of muriatic acid and solution of starch. After the removal of the iodide of potassium apparatus, the gas streaming from the tube could be readily smelt. By this means not the least trace of ozone was perceptible, but the moment the heating at $i$ was interrupted, the most penetrating odour of ozone was produced; this immediately disappeared on the interruption of the stream of sparks, but again made its appearance on this being re-established.

The quantity of iodine separated in an hour in the preceding experiment exceeded the quantity of ozone which could possibly result from any contamination with vapour of water more than nine-fold. From this it may be concluded, that there really exists an allotropic oxygen, which at ordinary temperatures is endowed with an affinity which even exceeds that of chlorine.

The conditions under which this allotropy of oxygen occurs are very remarkable. In the case of almost all other bodies, the allotropy induced by an elevation of temperature is characterized
by a remarkable weakening of affinity. In this case, on the contrary, we find an elevation of temperature of the greatest intensity and shortest duration, producing a state directly the reverse, which is marked by an increased affinity. That this state is produced only by the highest temperatures, and yet that when once produced, it disappears again by heating to a temperature even below 392° F., is not less remarkable, although explicable. The conversion of allotropic into common phosphorus does not take place instantaneously, but in a longer period of time. In the same manner, oxygen which has become allotropic by the high temperature of the electric spark may by rapid cooling pass through the interval between that temperature and 392° F. without going back completely to the ordinary state, if the duration of the cooling be shorter than the time necessary for it to return to the usual modification in that interval of temperature. From this it will be understood, how so small a fraction of the oxygen is obtained in this permanent state of allotropy, and how this state so readily disappears at a temperature above 392° F. It is highly probable, that the temperature at which ozone is decomposed into water and oxygen, and that at which allotropic oxygen is converted into the ordinary form of that substance, are not the same.

I hope to return again to this subject. During the preceding investigation, which was carried on in the laboratory of Heidelberg, I enjoyed the benefit of the good advice of Professor Bunsen, for which I have to express to him my warmest thanks.

VIII. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from vol. v. p. 468.]

March 17, 1853. The following papers were read:—

1. "On Animal and Vegetable Fibre as originally composed of Twin Spiral Filaments, in which every other structure has its Origin; a Note showing the confirmation by Agardh, in 1852, of observations recorded in the Philosophical Transactions for 1842." By Martin Barry, M.D., F.R.S., F.R.S.E.

After referring to the drawings to his paper on Fibre, published in the Philosophical Transactions for 1842, and the opinions entertained by physiologists regarding the peculiar views he advanced in that paper with reference to the original composition of organic fibre, the author states that, after the lapse of eleven years, these views have been fully confirmed, and in proof of this refers to a paper—"De cellula vegetabilis fibrillis tenuissimis contexta" (Lundæ, 1852), by Agardh. He further remarks, that his paper of 1842 contains a record of other observations made in a field beyond the region of Agardh's researches; observations which he thinks explain how it is
that fibre forms the membrane of the cell, and, what he deems of
more importance still, the mode of origin of fibre. He refers gene-
really to the drawings in that paper, from which, in connection with
facts previously recorded in the Philosophical Transactions, he states
that it appears—1, that fibre has its origin in the so-called "cyto-
blast," the outer part of which always passes into a ring or coil of
fibre; 2, that when a cell is to arise, its primary membrane is formed
out of this ring or coil of fibre; 3, that then the nucleolus of the
"cytoblast" becomes the nucleus of the cell; 4, that the outer
part of the nucleus of the cell also passes into a ring or coil of fibre,
wherewith to form deposits such as the annular and spiral, or to
weave the secondary membranes; 5, that the term "cytoblast" is
unsuitable, as the body so called does not always become a cell;
6, that fibre is thus more universal as well as more primitive even
than the cell, for fibre not only forms the cell, but it forms other
structures without having first to form a cell; 7, that the prime
mover in both the "cytoblast" and the nucleus is the nucleolus,
which is the organ of absorption, assimilation, and secretion; 8,
that the nucleolus is continually giving off its substance and con-
tinually renewing it, continually passing from the state of nucleolus
into that of "cytoblast" or nucleus,—so that the "cytoblast" and
the nucleus are each of them but the nucleolus enlarged; 9, that it
is therefore the nucleolus enlarged that passes into fibre; 10, that
the nucleolus always passes into fibre, and directly into no other
form than that of fibre; 11, that thus the whole organism arises
out of nucleoli, for fibre is but the nucleolus in another shape, and
every structure arises out of fibre; 12, that the nucleolus is repro-
duced by self-division, and that subsequently, when it has passed
into the form of fibre, the mode in which the nucleolus gives origin
to other structures is such as to imply even here the continued re-
production of its own substance—that mode being self-division.

The author describes particularly the mode of origin of primary
and secondary membranes, and division of the cell. He considers
that the latter is initiated by self-division of the nucleolus into halves
which become "cytoblasts," and it is completed by the formation
out of these of two young cells, the walls of which, where in contact
with one another, form a septum dividing the parent cell into two
compartments. Thus for division of the cell there occurs no folding
inwards of a "primordial utricle," as maintained by Von Mohl,
nor any division of the contents of a parent cell into two parts, around
which contents are formed the walls of two young cells, as supposed
by Nägeli and Hofmeister. On the subject of annular, spiral, and
other deposits in the vessels of plants, the author remarks, that when
the divisions of an annular or spiral fibre are not continued, but par-
tial and irregular, we have the reticular form, as well as an expla-
nation of the supposed tendency in vegetable fibre to anastomosis.

The two spiral filaments composing fibre at first appeared to the
author to run in opposite directions, which he subsequently saw was
not the case,—their direction is the same. This error he corrected
in Müller's Archiv for 1850.
The author remarks, that observers in their endeavours to reach the ultimate structure of the muscular fibril have actually gone too far, and reached a later generation,—mistaking for the fibril a row of quadrilateral particles, the mere elements thereof. These particles, he observes, are known to be alternately light and dark in alternate order; they give origin to the term spirals; and for this purpose the dark particles undergo what observers have entirely overlooked, division and subdivision, which changes he has figured in Müller's Archiv, 1850. The preparation in which he has again met with the subdivision into four is still, the author states, in his possession for demonstration to others.

2. "On the penetration of Spermatozoa into the interior of the Ovum; a Note showing this to have been recorded as an established fact in the Philosophical Transactions for 1843." By Martin Barry, M.D., F.R.S., F.R.S.E.

Referring to a statement by Dr. Nelson, in a paper "On the reproduction of the Ascaris Mystax," that the investigations in that paper "appear to be the first in which the fact of the penetration of spermatozoa into the ovum has been distinctly seen and clearly established in one of the most highly organized of the Entozoa," the author of the present communication remarks, that when Dr. Nelson made this statement he was evidently not aware of what had been published on the subject. In proof of this Dr. Barry refers to his own paper, entitled "Spermatozoa observed within the Mammiferous Ovum" (Phil. Trans. 1843, p. 33), in which he states that he had met with ova of the Rabbit containing a number of spermatozoa in their interior; and to the Edinburgh New Philosophical Journal for October 1843, which contains a drawing in which seven spermatozoa are represented in the interior of an ovum, besides the statement that in one instance he had counted more than twenty spermatozoa in a single ovum. In conclusion he remarks, that Dr. Nelson merely added a further confirmation in ova of an entozoon, to what his own researches on mammiferous ova had enabled him to record as an established fact nine years before.

April 7.—A paper was read, entitled "Observations on the Anatomy of the Antennæ in a small species of Crustacean." By John D. M'Donald, M.D., Assistant Surgeon to H.M.S.V. Torch.

The little crustacean which is the subject of this paper was taken in considerable numbers in the voyage from St. Vincent to Rio Janeiro. There are several anatomical peculiarities mentioned, but the most remarkable is the structure of the right antenna of the male. These organs are in the female perfectly symmetrical, and resemble that of the left side in the male; and although in the very young state of the latter sex the right antenna differs but little in external appearance from the left, yet the peculiar hypertrophied condition of the modified segments in the corresponding organ of the adult male is to be distinctly traced in a rudimentary state.

As the animal lives in the open ocean, none of the limbs are adapted for walking; but when placed in a vessel of sea-water, they

rested upon their antennæ on reaching the bottom, and paddled themselves about by their fore-limbs and tail.

The author remarks that in all their movements the males exhibit a tendency to turn towards the left side, and concludes the rationale of this fact to be, that the brain on the right side being more developed at the part from which the right antenna derives its nerves, a corresponding predominance is given to the power of the locomotive organs on that side.

When fully developed, each antenna in both sexes consists of twenty-five segments. Of these, the first thirteen present nothing remarkable; but all the remaining pieces on the right side enter into the composition of the curious prehensile organ which forms the principal subject of the paper.

This organ is composed in the following manner:—The fourteenth and four following segments are dilated into a large flask-like organ, the neck of which is eked out by the nineteenth and twentieth. The next two segments are fused together, and are articulated with the foregoing by a simple joint, and the whole of the remaining segments form another piece similarly articulated with the intermediate piece; so that the whole results in two simple joints susceptible of flexion in one direction only. On the eighteenth segment is a barbed process having its apex directed backwards, and its anterior border beset with sharp teeth. Two processes of the same nature, but differently placed and more elongated, lie side by side upon the fore-part of the first compound segment. This piece and that which succeeds it act upon each other like a pair of jaws, each furnished with an array of sharp conical teeth, while the last compound member of the series plays over the upper surface of the eighteenth segment.

The author then proceeds to describe the muscles which move this complex apparatus. The extensors are small and feeble, but the flexors are, as might be anticipated, more complex and powerful. They are two in number. The first has its origin in the large flask-like dilatation, and is inserted by a tendon into the second compound piece, from which the second muscle arises, and is inserted, also by tendon, into the third piece.

April 14.—A paper was read, entitled "On certain Functions of the Spinal Chord." By J. Lockhart Clark, Esq.

These investigations were undertaken by the author partly with the view of settling the long-agitated question whether all the roots of the spinal nerves terminate in the spinal chord, or whether any part of them ascend within the white or grey columns to the brain. The preparations employed for this purpose were made according to the new method described in the author’s former communication, Phil. Trans. 1851, Part 2; and the animals selected were the Ox, Calf, Cat, Rat, Mouse and Frog. Of the spinal chord of the Cat, he has succeeded, after much trouble, in rendering transparent longitudinal sections \( \frac{1}{13} \)th of an inch in thickness, and more than two inches in length, including the roots of four or five pairs of nerves.
The principal results at which the author has arrived are as follows:

That the posterior roots of the spinal nerves consist of three kinds; two of these enter the posterior grey substance at right angles, and the third kind with different degrees of obliquity upwards, a small proportion of the latter taking a longitudinal course and becoming lost in the posterior white columns.

That in no instance were any of the fibres of the anterior roots seen to ascend with the anterior white columns, before they entered the grey substance.

That besides the transverse bundles that form the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior grey substance and become lost as they proceed towards the surface of the chord.

That from the preceding facts, it may be inferred that nearly all, if not the whole of the fibres composing the roots of the spinal nerves proceed at once to the grey substance of the chord; and that if any of them ascend directly to the brain, it must be those only of the posterior roots which run longitudinally in the posterior white columns.

That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral.

That many of the fibres which belong respectively to the anterior and posterior roots in different regions of the chord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain, as it is well known that the formation of loops is one mode in which nerve-fibres do terminate there. A portion of the roots however may be connected with the vesicles of the chord, although the evidence of any such connection is very unsatisfactory.

That there are reasons for believing that the grey substance of the chord does not transmit impressions to and from the brain; and that the fine longitudinal fibres described by Stilling have not been found by the author.

That there is a great correspondence in the fibrous arrangement between the grey substance of the chord and the chiasma of the optic nerves. That the fact that the nerve-roots not only diverge both upwards and downwards to a considerable distance beyond their point of entrance, but intermingle also with each other in the most intricate manner, may explain how impressions made at one particular spot are communicated in different directions to distant parts of the chord, so as to excite a simultaneous and sympathetic action in classes of muscles which otherwise would appear unconnected.

April 21.—Extract from a letter from M. Kämtz addressed to Colonel Sabine (Translation).

Dorpat, 15th March (N. Style), 1853.

I have just completed the memoir on terrestrial magnetism which
I mentioned to you in former letters. It would follow from the materials which I have employed, that the horizontal force requires constants which differ from those for the vertical force, and the differences, although for the most part not considerable, are sometimes too large, as it appears to me, to be overlooked: it is for observers to decide in this matter. To avoid the introduction of insecure numbers I have not computed the potential, but have contented myself with deriving the values of X, Y and Z (declination, inclination, and force horizontal and total) being the quantities principally used for comparison. A second calculation of the constants for X and Y would have given rather more exact values, but I think that with the existing data the result would scarcely repay the labour of so extensive a calculation, for the tables would have to be entirely recomputed.

After obtaining the data from the theory, I wished to compare them with observation. I could take for the inclination and force the values which had served as the bases of the calculation; there are indeed several observations of inclination without our knowing the force, and a few of the force without inclination; but on the other hand; there are more extensive districts where I had no declinations, although the other two elements were well determined. Under these circumstances I was obliged to have recourse to older observations, which indeed I had occasion to use also for secular change and reduction for epoch to 1830. Thus the character of the work was gradually modified. For the Atlantic Ocean I availed myself of your excellent Memoir, Contributions No. IX.; I only added the observations of Lütke, d'Urville, and Rumker; of older observations, I took those of Abercrombie and Ekeberg, only using them however in parts where they had observed the inclination, and principally with the view of finding its secular change.

In the Pacific there was more deficiency; with the exception of what has been furnished by Lütke, d'Urville, the ships of the Prussian Merchant Service, and in the neighbourhood of the magnetic equator by Duperrey, modern voyages scarcely gave me anything; for Becquerel, in his detailed table*, omits all determinations made at sea. I therefore took all I could get since the voyages of Byron, Carteret and Wallis, chiefly from Hansteen's great work, and reduced to 1830, taking from each observer the mean of all his determinations within a space of 5° lat. and 10° long. I did not allow myself to exclude any observer, for as all have errors from the ship's iron, it might easily happen that a man but little known may have given values which are much nearer to the truth than those given by a celebrated voyager. A selection is no less objectionable, as it might easily lead to retaining observations which accord with a possibly not altogether correct theory, and omitting others, otherwise good, which might depart from it. Moreover, such a full comparison shows better what may be expected from the observations: incomplete as are the data they afford, yet, when I arrange the remaining errors in latitude and longitude, I rarely find groups in which

* Electricité, tome vii.
the errors show a certain degree of systematic character over extensive districts, so as to alter the declination or inclination a degree. The inclination appears to me to be very incorrect in the meridian of Alexandria. In India the few results of Elliot's given in the Philosophical Magazine from the Proceedings of the Royal Society, accord extremely well with my calculations. I subjoin a few points of comparison:—

North Pole in 70° 7' N., 263° 37' E. Ross's observations give 70° 5' N., 263° 14' E. For the South Pole, 74° 6' S., 152° 47' E. seems to agree less closely with Ross's map, although the inclinations observed in the neighbourhood agree well with the calculated ones.

Maximum of total force in the northern hemisphere 1871.8 in 54° 21' N. 265° 53' E.

(Errors compared with Lefroy's observations ...... +6° 4 in 54° 1 N. 260° 1 E.;

+0° 8 in 55° 6 N. 267° 4 E.; +4° 6 in 50° 2 N. 264° 0 E.)

For the force in the southern hemisphere, 2027.6 in 64° 36' S., 144° 34' E. is perhaps less correct, but there are in the S. hemisphere some striking anomalies in the observations.

The probable error of the force is 19.1; if however some groups which are at any rate not very correct are omitted, it becomes a little less than 15. For the inclination, 31° 1; for the declination, 41° 3.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. v. p. 464.]

April 25, 1853.—Professor Challis gave a lecture on the Adjustments of a Transit Instrument.

A paper was read by Professor De Morgan on the Principle of Mean Values, and an addition to a paper on the Symbols of Logic, &c. in vol. ix. part 1 of the Society's Transactions.

Though the heading of this paper describes one of its main results, yet it might with equal propriety have been styled a discussion of some points of algebra, with reference to the distinction of form and matter. This distinction, it is contended, is more extensively applied in algebra than in logic, though more recognized in logic than in algebra. Looking at the disputed points which exist in the higher parts of mathematics, and feeling satisfied that they will never be settled until the separation of form and matter is both visible and complete, the author makes a first attempt towards the examination of the question how far this yet remains to be done. A number of comparisons are made between algebraical and logical process, in the course of the inquiry, illustrative of the opinion entertained by Mr. De Morgan, that logic, as treated, requires the interposition of the algebraist, and cannot, except by aid of algebraical habits, be rendered a complete exposition of the forms of thought. In digressive notes, he combats the opinion that a generalization of the quantity is, as asserted, a new material introduction. He argues against the too mathematical tendency of some of the logicians who have endeavoured to extend the ancient system, especially the attempt of some to make the logical import of the proposition nothing but a comparison of more and less, and an equation or non-equation of quantities. He
points out that the proposition has been formalised in nothing but its terms, subject and predicate; and gives an instance of the method in which a failure of general maxims is answered by the sole assertion that the mode of expression which brings about the failure is useless. He refers to what were called sophisms, contrasting the neglect of them by the logicians with the use which the algebraists have made of their corresponding difficulties, as in the case of negative and imaginary quantities, the fraction \( \frac{a}{b} \). He argues against the assertion of more than one eminent writer on logic, that the identity of two terms, \( X \) and \( Y \), expressed as in "all \( X \) is all \( Y \)," is not a complex proposition—is not the union of \( \text{Every} \ Y \) is \( X \) with \( \text{Every} \ X \) is \( Y \). In an appendix to a former paper on the symbols of logic, he refers to a complaint of misrepresentation made by Sir W. Hamilton of Edinburgh, to whom certain technical phrases had been attributed. Mr. De Morgan makes the requisite correction, affirms that he had good reason for attributing such phraseology, and points out what that reason was: he then proceeds to answer two new charges of plagiarism against himself, from the same quarter; giving as his reason for addressing such answer to the Society, that Sir W. Hamilton makes the appearance of the asserted plagiarisms in the Transactions his principal ground of notice.

Finally, as to the logical part of the communication, Mr. De Morgan, reverting to his complex syllogism, in which each premise and the conclusion contain two ordinary propositions, generalizes the premises into the numerical form, and, giving terms and quantities algebraical designations, points out the mode of producing all possible inference. The immediate occasion of this introduction is as follows:—Sir W. Hamilton, in a recent publication, one tract of which is directed against Mr. De Morgan's last paper on syllogism, affirms that a proposition, as to its logical force, is merely an equation or non-equation of quantities, from which the declaration of coalescence or non-coalescence of terms into one notion is a consequent. Mr. De Morgan maintains the converse; namely, that the proposition is a declaration of coalescence or non-coalescence, of which the equation or non-equation of quantities is an essential. In treating the complex syllogism, under definitely numerical quantities, he has to search for the properties of the equation of coalescence, as distinguished from the equation of quantity; and, having made the former the means of arriving at inference, he invites those who can to try if the same result can be produced by means of the latter alone.

To pass to the algebraical part of the paper. It is first contended that the states infinity and zero, whether represented by distinctive symbols attached to 0 and \( \infty \), or by negative and positive powers of \( dx \), must be formally distinguished, as being each, not a value, but a status, containing an infinite number of cor rational values, just as happens in finite quantity. In order to lay down the formal laws of connexion of these different states, it is necessary to examine the formal use of the symbol \( \frac{dx}{x} \). After pointing out instances in which the laws of algebra are by many declared invalid, as by those algebraists who admit and interpret \( 2x = x \), but cannot give permission
to divide both sides by \( x \), the following laws are suggested. The symbol \( = \) is to be read with an index, as in \( =_{n} \), which has reference to the order \( \infty_{n} \) or \( 0_{-n} \), or as in \( =_{-n} \) which has reference to \( \infty_{-n} \) or to \( 0_{n} \). The equation \( A =_{a} B \) is normally satisfied when \( A \) and \( B \) are of the order \( n \), and \( A - B \) of a lower order. It is supernormally satisfied if \( A \) and \( B \) be both of any (the same or different) higher order than the \( n \)th, and subnormally if both be of any lower order. Among the most conspicuous rules which follow, are that \( AC =_{m+n} BD \) is normally satisfied, if \( A =_{a} B \) and \( C =_{a} D \) are so; and that when an equation is multiplied or divided by a quantity of the order \( n \), the index of equality must be increased or diminished by \( n \). Various cases are given in which such results as now present anomalies are reduced under formal law, and others which would be absolutely rejected are shown to be capable of consistent interpretation.

The formal law of connexion of the different states, of which finitude (with the index 0) is only one, is that the order \( 0_{m} \) stands to finite quantity in all respects as finite quantity to \( \infty_{m} \). Hence, so far as 1 and \( 1+0 \) are simultaneous as well as equal, so far \( \infty \) and \( \infty+a \) are simultaneous as well as equal. And if \( \phi(1) =_{a} \phi(1+0) \) be a universal law, so must be \( \phi(\infty) =_{a} \phi(\infty+a) \). Further, \( \infty-\infty \) must be, formally speaking, wholly indeterminate, even when it is a case of \( x-x \).

In relation to such indeterminate forms as \( \infty-\infty, \frac{\infty}{0}, \) &c., Mr. De Morgan contends that their formal and \( a priori \) character is that of indeterminateness; and that the choice between determinate and indeterminate character, which so often occurs, is dictated by the matter of the problem, the determinate value being dictated by the laws of algebra. The index of equality, for instance, may be the means of decision: an example is given in which one equation belongs to two different problems, but with different indices of equality; in one \( \frac{\infty}{0} \) is determinate, in the other wholly indeterminate.

In assigning \( \infty \) or 0 as values, it is often necessary to assign relations of order. When a quantity passes from positive to negative, or the converse, through 0 or \( \infty \), it passes through every order of 0 or \( \infty \); and this even when the passage is from one phase of 0 or \( \infty \) to another, of different signs. Thus, the orders being powers, \( x \) cannot pass from \( -a \cdot 0^{m} \) to \( +a \cdot 0^{m} \), without passing through even \( 0^{\infty} \).

Mr. De Morgan insists upon one of two things: either, the abandonment of the separate use of 0 and \( \infty \), except only in the retention of the former symbol to represent \( A - A \); or, the introduction of different orders, and the free use of the comparisons of those orders. For himself, he prefers and adopts the latter alternative.

The principle of limits is considered as a formal law of algebra, but not to the exclusion of every other result. If a constant, for instance, have the value \( A \) up to \( x=a \) exclusive, it has \( A \) for one value when \( x=a \). If the constant be transitive, that is, if it be always \( =B \) after \( x=a \), then \( x=a \) gives both \( A \) and \( B \) for the constant, and, as a fact hitherto observed, its value from calculation is \( \frac{1}{2}(A+B) \). This observed fact Mr. De Morgan believes he connects with the principle of limits, making it a necessary consequence of
the universal truth of that principle; and hence he holds that it may
be stated as a theorem, under the name of the principle of mean
values. Various uses of this principle are given. Further, in assum-
ing the free use of the orders of 0 and ∞, it is shown that it is cor-
correct to say that the constant passes from A to B while h, x being
a + h, passes through the phases of 0. So that, for instance, at an
epoch of transitivity the value of φ(a+0) is dependent upon the
form of 0. The brevity of an abstract prevents the statement of
those cautions under which such use of language is introduced. One
result, however, may be brought forward. When the function.fx is
transitive (or, as commonly said, discontinuous) at x=a, the equa-
tion φ(fa)=(φf)a no longer necessarily exists. But this, as is
pointed out, is what may happen at any value of x which makes a
differential coefficient infinite.

On the question of sin ∞ and cos ∞, Mr. De Morgan deduces
their observed values, sin ∞ = 0 and cos ∞ = 0, both from the prin-
ciple of mean values, and from the formal truth of the equation
φ(∞ + a) = φ∞. From the same principles follows the equation
(−1)∞ = 0. In this case, however, and in all which come under
the principle of mean values, the absolute necessity of the results is
not affirmed. They are the alternatives of indeterminateness. But
in thus representing them, Mr. De Morgan does not concede more
than he conceives must be conceded with respect to ∞ − ∞, ∞, and
the like.

On the question of series, Mr. De Morgan contends that all the
uncertainty and danger of divergent series belongs equally to con-
vergent series, in every case in which the envelopment is unknown.
On this part of the subject he adds to the arguments of a former
paper, and insists upon the superior safety of the alternating series,
in which the terms are alternately positive and negative.

Without going further into details, the purport of this paper may
be stated as follows. Algebra, using the term in the widest sense,
ought to be, and is approaching towards, a science of investiga-
tion, and a symbolic art of expression, of which the laws are strictly and
without exception incapable of failure, suspension, or modification.
The formal laws under which such a result is to be obtained, though
drawn in the first instance by extensive induction, of which many
steps are accompanied by difference of opinion, will at last be received
and admitted as parts of the definition of the science, à priori. The
existing defect of the science is an imperfect formalization, arising
from the want of views of sufficient extent, and leading to material
distinctions, that is, to exceptions dictated, à posteriori, by the re-
results of particular cases. Such exceptions have in many instances
been brought within rule by further consideration; and it is con-
ceived that the same thing will happen at last in all cases. The
paper is an attempt to examine the principal outstanding difficulties
(those connected with the definition of integration excepted) with
reference to the question how far they may arise from imperfect
conception of formal laws. That there is to be a formal science, is
positively assumed, and made the basis of the attempt: how far any
suggestion contained in the paper is a valid step towards it, is treated with doubt and left to opinion.

May 9.—Mr. Hopkins, F.R.S. & c., the President of the Society, gave an account of some experiments for the determination of the temperature of fusion of different substances under great pressure; and on the application of the results to ascertain the state of the interior of the earth.

IX. Intelligence and Miscellaneous Articles.

ON THE APPLICATION OF PHOTOGRAPHY TO THE STUDY OF CERTAIN PHENOMENA OF POLARIZATION. BY MR. W. CROOKES*.

The experiments which I have the honour to submit to the Society this evening were commenced about eighteen months ago, and had their origin in the desire to record and retain in a more tangible form the well-known beautiful figures observed when thin slices of some crystals (such as calc spar and nitre) are seen in the polariscope.

After several preliminary trials with different arrangements of lenses and tourmalines, in the endeavour to produce a well-defined image on the ground-glass of the camera, I finally proceeded in the following manner:—The crystal to be copied was placed between two tourmalines, and adjusted by looking through them at any diffused light; when the proper figure was obtained, the whole was cemented together with marine glue, and fastened in the brass-tube of a double achromatic portrait-lens. Several lenses (non-achromatic) were placed behind the crystal to receive the image and reduce its size before falling upon the collodion plate; these varying in number and focal length, according as the image was required large or small. A lens was placed in front of all, more as a protection against dust, & c. than for any practical purpose; it seems, however, to condense the light a little. Although this arrangement produced a very perfectly defined figure upon the ground-glass, the light was so exceedingly faint, having to pass through so many thicknesses of glass, besides two dark-brown tourmalines not a quarter of an inch square, that no image could be perceived on the focusing-glass unless the camera was held pointed direct to the sun, and every other light carefully excluded from the eye. Collodion, although so exquisitely sensitive under ordinary circumstances, was quite inadequate to copy these figures, as the plates, in their necessarily upright position, would not bear the long exposure requisite to obtain a picture without spontaneously decomposing; besides which, the camera being pointed at the sun, the motion of the latter would produce a very unequal action on the plate, fatal to the success of the experiment. After repeated trials and as many failures, I was obliged to substitute sensitive paper for the collodion plate, and leave the camera pointed for several days to the north. In this experiment, although I obtained no very good result, I had the

* From the Journal of the Photographic Society for June 21, 1853; having been read before the Society June 2.
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satisfaction of knowing that light which is barely intense enough to produce a transient impression upon the retina, will, in time, produce a strong impression upon a sensitive photographic surface. I should however have obtained nothing worth preserving, had not Professor Wheatstone, to whom I mentioned the results of my experiments, kindly allowed me to use his magnificent tourmalines and crystals. On placing these in the position above mentioned, I could perceive a very visible image on the focusing glass, without the sun, and, by a few preliminary trials found that even with the diffused light of the sky, collodion would take a good impression with from half an hour to two hours' exposure, varying with the size and nature of the crystal employed. The camera should be placed on the ground and pointed upwards; in this position the collodion plate will keep for more than three hours, if it be sufficiently level to retain the liquid on its surface. [I find that when kept in this manner its sensitiveness is very little impaired.] The best light is from a rather cloudy sky, bright but without sun; the camera should be turned a quarter round every two or three minutes to prevent the too unequal action of the light.

The figures produced in unannealed glass under the same circumstances presented no difficulty in copying. The piece of glass, set in a black opaque frame, was placed upright in front of a black glass reflector, arranged so that the diffused light from the sky, falling upon it, might be reflected horizontally at the proper angle and pass through the piece of unannealed glass. The photograph was taken in a camera placed opposite, having a tourmaline or Nichol's prism fastened in front of the lens; by rotating which, the proper figure was obtained on the ground-glass.

My first copies were taken on collodion, iodized in the usual manner; they presented a far greater number of rings both in calc spar and nitre, and extending much further than could be seen in the polariscope: while in the latter I was not able to perceive more than eight or nine rings, upwards of fifty could sometimes be counted on the photograph, showing in a remarkable manner the greater extent of the rings formed by the more refrangible rays over those formed by the visible ones.

A few weeks ago on mentioning this circumstance to Professor Wheatstone, he suggested that I might acquire some valuable information by adopting Sir John Herschel's proposition of substituting bromide for iodide of silver, and cutting off the chemical rays with sulphate of quinine; thus obtaining an effect due to the light alone. I immediately made the following experiments on this subject: Having first satisfied myself that several inches in thickness of quartz obstructed none of the chemical rays, I made a small cell of about one ounce capacity, the two sides consisting of flat plates of quartz, and the distance across the cell from one side to the other being one inch and a half: this I filled with a solution of sulphate of quinine in dilute sulphuric acid, containing 25 per cent. of the sulphate, and roughly measured the distance to which the invisible rays penetrated, by concentrating a ray of sunlight on it by means of a lens; the
cone of blue light hardly reached an eighth of an inch beyond the surface. The cell filled with the solution was placed opposite the sky, and a photograph taken of it on iodide of silver with a quartz lens: the sky, on developing, came out black, while the light which had passed through the quinine had hardly affected the plate in the least. I then diluted the solution, until, on trial, the cone of light reached half way across the cell; still the photograph presented the same appearance as before, and it was not until the solution was so diluted that the cone of blue light, formed by concentrating a ray of sunlight upon the solution, would pass completely through, that I could perceive an increase in the effect on the photograph. When the solution was no stronger than one or two grains to the ounce, there were hardly any chemical rays absorbed.

The slight effect which the light produced after passing through the strong solution of quinine, I found to be due solely to the action of the less refrangible rays. A photograph was taken of the solar spectrum, the beam of sun-light being allowed to pass through the above solution before falling upon the prism: there was a slight impression, but only where the visible rays fell. The same effect was produced on bromide of silver in a much shorter time.

I immediately procured some collodion with bromine in the place of iodine, prepared for me by Mr. Hockin; this I found to answer the purpose very well.

On copying one of the figures seen in nitre on this film, interposing a bath of a nearly saturated solution of sulphate of quinine 1\(\frac{1}{2}\) inch thick, I had the gratification of finding it as I had anticipated; the rings were now reduced to the visible number, and at the same time were much broader, owing to the greater extent of surface in each ring capable of affecting bromide of silver. But here a remarkable dislocation presented itself; each quadrant of the interior rings, instead of retaining its usual regular figure, appeared as if broken in half, the halves being alternately raised and depressed towards the neighbouring rings.

Having obtained such unexpected results with nitre—a crystal possessing two axes—it became a matter of great interest to see whether an abnormal figure would be shown by a uniaxial crystal under similar circumstances. Calc spar was the one selected for this purpose; the figure which I had hitherto obtained on iodide of silver was a perfectly regular one, the rings gradually diminishing in breadth and distinctness as they became more distant from the centre, and differing in no respect from those seen in the polariscope but in extending to a much greater distance. When bromide of silver was employed, and the quinine bath used to intercept the chemical rays, the figure no longer presented so regular an appearance as at first; the number of rings was now reduced to about nine, while the fourth and fifth seemed to unite together to form one large ring, entirely obliterating the intervening space.

I have since found that these effects are obtained whether the quinine bath be present or not, whenever bromide of silver is employed as the sensitive surface.
Fig. 1.

Calc Spar.

Normal.

Abnormal.

Fig. 2.

Fig. 3.

Nitre.

Normal.

Abnormal.

Since the rays which produce these abnormal figures do not act upon iodide of silver, but affect bromide whether a quinine bath be interposed or not, they can hardly be present in the more refrangible, invisible portion of the spectrum, or at least not in that part which is absorbed by sulphate of quinine; neither can they be invisible heat rays beyond the extreme red, since these do not affect bromide of silver. I have satisfied myself that the figures are invisible to the eye, by accurately examining each of the crystals in a polariscope, illuminated first by white light, and then by each isolated ray of a very pure solar spectrum; in no case was there any figure but the normal one perceived. It is possible, therefore, that these abnormal figures may be caused by invisible rays which have hitherto escaped notice, existing within the space enclosed by the visible spectrum.
ON THE COMBUSTION OF AMMONIA, ETC., BY THE GREEN OXIDE OF CHROMIUM. BY THE REV. J. E. ASHBY, B.A., F.R.A.S.

1. For the following experiments, it is necessary to provide a small shallow saucer (such, for instance, as is used to hold a small quantity of any water-colour), and a piece of fine wire-gauze to cover it. Set out also chromic acid, pyroxylic spirit, alcohol, and blotting-paper, and turn up the spirit-lamp to a large flame when required.

2. I prepare the green oxide of chromium from chromic acid. With a small test-spoon set the chromic acid in powder or very fine crystal upon the wire-gauze, and placing the gauze upon blotting-paper wet with pyroxylic spirit or alcohol, it will change colour, burst into flame, and emit fumes of the green oxide, which are to be avoided as much as possible. Now heat the residue to incandescence over the spirit-lamp; if necessary, break up the cinders of green oxide into smaller pieces to allow free currents to pass through the gauze, and for each experiment heat the whole to redness before exposing it to the vapours.

3. Nearly fill the saucer with alcohol or pyroxylic spirit, cover it with the gauze on which is the dry and warm Cr₂O₃, and combustion will begin and be kept up for a considerable time.

4. Immediately after incandescence, place the gauze over the saucer nearly filled with the strongest liquid ammonia, and, as in the former instances, combustion will commence and continue with the evolution of much heat from the Cr₂O₃, which presents the appearance of a miniature mass of burning coals. This may be shown even by placing the gauze over the mouth of an unstoppered bottle of liquid ammonia; but to ensure success, the solution of ammonia should be of great strength.

5. I have not had time to examine the products of combustion when ammonia is thus decomposed; but I think that nitrate of ammonia must pass off in combination with watery vapour.

6. The green oxide of chromium thus prepared will probably be found to possess, in a modified degree, all the catalytic properties of spongy platinum.

12 Mornington Road,
Regent’s Park, London.

ADDITIONAL OBSERVATIONS ON CATALYTIC COMBUSTION.

I find that the sesquioxide of chromium, properly prepared, is by far to be preferred to spongy platinum in most cases, and can be made at so trifling a cost that large operations may be cheaply carried on by the use of it. The combustion of ammonia is very singular, and so complete that the strong solution might even be used as fuel for a catalytic ammonia-lamp for distillations, evaporations, &c. Coal-tar naphtha furnishes a very complete and cheap combustible. A friend has tried dry ammoniacal gas fed with oxygen, and the result, as might be expected, is somewhat more brilliant. I now prefer to prepare the sesquioxide by placing chromic acid in fine
Intelligence and Miscellaneous Articles.

powder upon the gauze, and setting it over the strong liquid ammonia, which at once reduces and ignites it, and the ammoniacal gas continues to burn vividly for some time. The Cr₂O₃ has then the appearance of coarse green tea. The wick of a common spirit-lamp should be pulled up rather extravagantly, and spread open to give a very large flame, in which the Cr₂O₃, supported on the gauze (on a ring or otherwise), may be made perfectly dry and warm for successive experiments.

To ignite hydrogen, the oxide must be made very warm; if fresh from the lamp, when all trace of ignition has died away, it will set fire to the gas.

With the prepared sesquioxide on wire-gauze, I have obtained catalytic combustion of the following substances:—

Alcohol.
Æther.
Chloroform.
Pyroxylic spirit.
Acetic Acid.
Tar naphtha.
Turpentine.
Creosote (hot).
AMMONIA.
Oil of orange-peel (warm).
Oil of lemon-peel (warm).
Oil of bergamotte (warm).
Oil of rosemary (warm).
Camphor vapour.
Indigo vapour.
Naphthaline vapour.

Probably all of the essential oils will burn. Caoutchoucine, eupion, and various other substances should be tried. Students of organic chemistry will probably be able to produce many interesting oxidations and transformations, and the results may be collected in considerable quantity.

June 7, 1853,

John Eyre Ashby.

ON SUN COLUMNS OBSERVED AT SANDWICH MANSE, ORKNEY.

BY THE REV. C. COULSTON.

May 18th. About 8 p.m. observed a mock sun having prismatic colours, on the N. side of the sun, with rays on the off side converging to a point. In about 15 minutes another of the same description, but fainter, appeared on the S. side of the sun, and a faint halo appeared over the sun, as if joining there. At sunset there was a faint sun pillar.

21st. At sunset another sun pillar seen.

23rd. A sun pillar seen tonight at 8 p.m., about 45 minutes before sunset, at first being a pale whitish beam, shooting up through the
clear blue sky, fully 15° high, and very slender, scarcely the diameter of the sun.

After the sun had set in a golden sky it became of a rosy hue, brighter and broader at some places than others, as if the brightest strata, or thin laminae of the horizontal clouds, reflected the sunbeams most perfectly. It continued quite perpendicular, and about half-past 9 o'clock, or 45 minutes after sunset, vanished gradually—the last spot of it being 1 or 2 degrees above the horizon. About an hour afterwards, when the moon rose, a pillar of light also appeared over her, but fainter and shorter, and after 30 minutes when she had risen a little above the horizon, a similar pillar appeared below her, but they only extended about 3 or 4 diameters of the moon on each side, having her diameter as a base, and tapering away as they receded from her. This continued till 12 o'clock and probably much longer.

I am told that the sun pillar was again very beautiful before sunrise next morning, about 3 o'clock.

METEOROLOGICAL OBSERVATIONS FOR MAY 1853.


Mean temperature of the month ........................................... 51°41
Mean temperature of May 1852 ........................................... 51°45
Mean temperature of May for the last twenty-seven years .... 53°95
Average amount of rain in May ........................................ 1’77 inch.


Sandwick Manse, Orkney.—May 1. Bright a.m.: damp p.m. 2. Hazy a.m.: fog p.m. 3. Clear a.m.: clear, aurora p.m. 4. Bright a.m.: fog p.m. 5. Damp a.m. and p.m. 6. Cloudy, showers a.m.: sleet-showers, aurora p.m. 7. Snow-showers a.m.: sleet-showers, aurora p.m. 8. snow-showers a.m.: sleet-showers p.m. 9. Sleet-showers a.m.: snow-showers p.m. 10. Snow-showers a.m.: clear p.m. 11. Bright a.m.: clear p.m. 12, 13. Clear a.m.: clear, fine p.m. 14—17. Clear a.m. and p.m. 18, 19. Clear, fine a.m. and p.m. 20. Clear a.m. and p.m. 21. Bright, fine a.m.: clear, fine p.m. 22. Bright a.m.: clear, fine p.m. 23. Bright a.m.: sun pillar, clear, fine p.m. 24, 25. Bright, fine a.m.: clear, fine p.m. 26. Cloudy a.m. and p.m. 27. Clear a.m.: clear, fine p.m. 28, 29. Cloudy a.m. and p.m. 30. Bright a.m.: clear, fine p.m. 31. Bright, fine a.m.: clear, fine p.m.

Mean temperature of May for twenty-six previous years ... 47°94
Mean temperature of May 1852 ........................................... 50°49
Mean temperature of this month ....................................... 49°07
Average quantity of rain in May for seven previous years ... 1’68 inch.
Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; and by the Rev. C. Clouston, at Sandwich Manse, Orkney.

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<td>30°021</td>
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X. On the Reflexion of Polarized Light from the surface of Transparent Bodies. By the Rev. Samuel Haughton*.

In the year 1837, Mr. George Green of Cambridge published in the Transactions of the Cambridge Philosophical Society†, a paper on the "Laws of Reflexion and Refraction of Light at the common surface of two Non-crystallized Media," in which he obtained formulæ for the intensity of reflected polarized light which appeared to explain the peculiarities of polarized light reflected from the surface of transparent bodies endowed with a high refractive index, as observed by Brewster, Airy, and other observers.

Formulæ intended to answer the same purpose were deduced from theory, and published by M. Cauchy in 1839: these formulæ are not the same as those obtained by Mr. Green.

The principle from which Mr. Green deduced his formulæ is the following:—that in general there must be in an elastic medium both normal and transverse vibrations; and that although we are obliged to assume in physical optics that the velocity of propagation of the former is infinitely greater than that of the latter, yet the evanescent normal waves which are produced at the bounding surface of two transparent bodies will produce an effect upon the transverse waves by changing their phase.

In order to apply this principle, and calculate the change of phase, Mr. Green uses the method of the Mécanique Analytique of Lagrange; and in applying it to the problem of reflexion, adds an hypothesis obtained from the analogy of gases, which hypothesis reduces the conditions at the limits obtained by following Lagrange's method to other conditions, which are identical with those used by M. Cauchy, and obtained by him from what he calls the 'principle of continuity of movement.'

* Communicated by the Author.
† Vol. vii. part 1.

In a memoir read before the Academy of Paris, Jan. 15, 1849, M. Cauchy, after stating that Mr. Green had perceived the influence which the normal evanescent ray must have in changing the phase of the real ray, adds, "et si, après avoir fait cette remarque, dans son mémoire du 11 Décembre 1837, M. Green n'a pas obtenu définitivement les véritables lois de ces phénomènes, cela nous parait tenir principalement à ce qu'il a cru pouvoir appliquer à l'éther considéré isolément la formule générale du mouvement donné par Lagrange." (Comptes Rendus, vol. xxviii.)

M. Cauchy proposes elsewhere to substitute the 'Principle of continuity of Movement' for the 'Principle of equality of Pressures,' which latter is the principle obtained directly from the method of Lagrange.

M. Cauchy cannot have honoured Mr. Green's paper with an attentive perusal, or he would have perceived that Mr. Green's hypothesis as to the equality of coefficients in the two bodies in contact reduces the principle of equality of pressures to that of the continuity of movement. In fact, Mr. Green's conditions at the limits (p. 17) are the same as those given by M. Cauchy's Principle of Continuity of Movement. But although the principles from which M. Cauchy and Mr. Green start are identical, their results differ in two important particulars:—1st, M. Cauchy has introduced a constant distinct from the index of refraction, and depending on the nature of the reflecting body; 2nd, the particular manner in which the condition of evanescent normal waves is introduced is different, so that the results differ in form as well as in the number of constants.

The publication* of M. Jamin's beautiful experiments on the reflexion of light at the surface of transparent bodies, affords an opportunity of comparing Mr. Green's formulæ with observation. In this paper I propose to compare the form of Mr. Green's results with M. Jamin's experiments on sulphuret of arsenic, as there can be no doubt but that M. Cauchy is right in introducing a second constant proper to the reflecting surface: this will appear in the course of the comparison.

Let $I$ and $J$ denote the amplitudes of the reflected vibration of the unit of light polarized in and perpendicular to the plane of incidence respectively.

Let $e$ and $e_1$ denote the difference of phase between the incident and refracted, and reflected and refracted rays respectively.

Let $i$ and $r$ denote the angles of incidence and refraction.

And let $Q$ be defined by the following equation,

$$Q = \frac{\mu^2 + 1}{(\mu^2 - 1)^2}$$

(1)

where $\mu$ denotes the refractive index.

It is not difficult to deduce from Mr. Green’s equations the following:

\[
\begin{align*}
\cot e &= Q(\mu^2 \cot i + \cot r) \\
\cot e_t &= -Q(\mu^2 \cot i - \cot r)
\end{align*}
\]

Equations (2) belong to the case of light polarized perpendicular to the plane of incidence; and since light polarized in the plane of incidence is proved by Mr. Green to undergo no change of phase by reflexion, it is easy to see that \(e_t - e\) is the difference of phase produced by reflexion in the two components of light originally polarized in any azimuth.

The quantity which M. Jamin has selected for comparison with theory is

\[
\frac{e_t - e}{180}\]

A few trials convinced me that the coefficient \(Q\), as defined by equation (1), would never represent the observations; I therefore proposed to myself to examine whether it were not possible to represent M. Jamin’s observations by equations (2), considering \(Q\) as an unknown constant to be determined by experiment.

The formulae from which M. Cauchy deduces the difference of phase cannot be reduced to the form of equations (2). In the following table I have given both M. Cauchy’s results and those deduced from the modification of Mr. Green’s formulæ, which I have proposed.

Table I.

Sulphuret of Arsenic (transparent).

Difference of phase.

\[\mu = 2.454, \quad Q = 2.55.\]

<table>
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<th>(i)</th>
<th>(\frac{e_t - e}{180})</th>
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<td>1.974</td>
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<td>81</td>
<td>1.961</td>
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<td>77</td>
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<td>73</td>
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<td>71</td>
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<tr>
<td>69</td>
<td>1.611</td>
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<td>62</td>
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<td>60</td>
<td>1.154</td>
<td>1.161</td>
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<tr>
<td>59</td>
<td>1.075</td>
<td>1.086</td>
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<tr>
<td>30</td>
<td>1.018</td>
<td>1.030</td>
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The Rev. S. Haughton on the Reflexion of Polarized Light

A comparison of the columns of differences between the observed results and those calculated from the two theories, shows that they equally well represent the phenomena.

The value of \( Q \), however, deduced from (1), is 0.278. If this value had been used in calculating Mr. Green’s formulæ instead of 2.55, the results would have differed widely from the observations.

The following value for the ratio of \( J \) to \( I \) may be deduced from Mr. Green’s equations,

\[
\frac{J^2}{I^2} = \frac{Q^2 \cos^2 (i+r) + \sin^4 r \cosec^2 (i-r)}{Q^2 \cos^2 (i-r) + \sin^4 r \cosec^2 (i+r)}.
\]

This equation differs from that found by M. Cauchy, and cannot be reduced to the same form.

In the following table I have expressed, as before, the results of both calculations, and compared them with observation, using for Mr. Green’s formula the value of \( Q \) found from experiment.

Table II.
Transparent Sulphuret of Arsenic.
Ratio of amplitudes.

\( \mu = 2.454 \).

\( Q = 2.55 \).

<table>
<thead>
<tr>
<th>( \tan^{-1} \left( \frac{J}{I} \right) )</th>
<th>Green (modified)</th>
<th>Cauchy</th>
<th>Green</th>
<th>Cauchy</th>
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<td>Observed.</td>
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<td>32.10</td>
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<td>( 80 )</td>
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<tr>
<td>( 76 )</td>
<td>16.20</td>
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<td>16.19</td>
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<td>( 72 )</td>
<td>9.22</td>
<td>9.53</td>
<td>9.43</td>
<td>-0.31</td>
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<td>( 68 )</td>
<td>6.56</td>
<td>6.23</td>
<td>6.3</td>
<td>+0.33</td>
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<td>( 66 )</td>
<td>6.46</td>
<td>6.51</td>
<td>6.27</td>
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<td>( 62 )</td>
<td>10.33</td>
<td>10.41</td>
<td>10.24</td>
<td>-0.8</td>
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<td>( 58 )</td>
<td>15.40</td>
<td>15.24</td>
<td>15.10</td>
<td>+0.16</td>
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<td>19.46</td>
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<td>( 50 )</td>
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<td>23.53</td>
<td>-0.40</td>
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<td>( 40 )</td>
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<td>32.18</td>
<td>32.12</td>
<td>+0.22</td>
</tr>
</tbody>
</table>

From this table it is plain that the formula (3) (considering \( Q \) to be an experimental constant, depending on the refractive index and on the reflecting surface) represents the observations at least equally as well as that given by M. Cauchy.

I have compared the observations of M. Jamin for other substances than sulphuret of arsenic with Mr. Green’s formulæ, modified as I have proposed, and have found the agreement perfect. The case of sulphuret of arsenic is the severest test of theory given by M. Jamin, as the observations in this case differ
from the surface of Transparent Bodies.

It remains for me now to explain the theoretical signification of the alteration which I have proposed to make in Mr. Green's formulæ. In order to do so, it is necessary to give the equations from which he sets out, and to show where the restriction occurs which prevents Mr. Green's formulæ from accurately representing the phenomena in their present state.

In order to facilitate reference to Mr. Green's paper, I shall retain his notation.

Let \( x \) be perpendicular to the reflecting plane; \( z \) and \( y \) lying in this plane, and \( y \) in the plane of incidence. Let \( u, v \) denote the displacements parallel to \( x, y \), the light being polarized perpendicular to the plane of incidence.

The conditions at the limits are (p. 17)—

\[
\begin{align*}
\left. u \right|_0 &= u_i, \\
\left. v \right|_0 &= v_i; \\
\frac{du}{dx} &= \frac{du_i}{dx}, \\
\frac{dv}{dx} &= \frac{dv_i}{dx}
\end{align*}
\]

(4)

combined with the understood condition, \( x = 0 \).

Let us assume

\[
\begin{align*}
u &= \frac{d\phi}{dx} + \frac{d\psi}{dy}, \\
v &= \frac{d\phi}{dy} - \frac{d\psi}{dx},
\end{align*}
\]

(5)

and transform equations (4).

Mr. Green thus finds, referring to his equations (14) and (16),

\[
\begin{align*}
\frac{d\phi}{dx} + \frac{d\psi}{dy} &= \frac{d\phi_i}{dx} + \frac{d\psi_i}{dy}, \\
\frac{d\phi}{dy} - \frac{d\psi}{dx} &= \frac{d\phi_i}{dy} - \frac{d\psi_i}{dx}, \\
\frac{1}{g^2} \frac{d^2\phi}{dt^2} &= \frac{1}{g_i^2} \frac{d^2\phi_i}{dt^2}, \\
\frac{1}{\gamma^2} \frac{d^2\psi}{dt^2} &= \frac{1}{\gamma_i^2} \frac{d^2\psi_i}{dt^2}
\end{align*}
\]

(6)

\( g, g_i \) denoting the velocities of the normal wave in the two bodies in contact; \( \gamma, \gamma_i \) the velocities of the transverse wave.

Mr. Green substitutes in these equations, and in the general equations of motion, the following expressions:

\[
\begin{align*}
\psi &= \alpha \sin (ax + by + ct + e) + \beta \sin (-ax + by + ct + e), \\
\phi &= e^{ax} \{ A \sin (by + ct) + B \cos (by + ct) \}, \\
\psi_i &= \alpha_i \sin (ax + by + ct) \\
\phi_i &= e^{-a\gamma x} \{ A_i \sin (by + ct) + B_i \cos (by + ct) \}
\end{align*}
\]

(7)
Substituting these expressions in the general equations of motion, which are

\[
\begin{align*}
\frac{d^2\phi}{dt^2} &= g^2 \left( \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} \right) \\
\frac{d^2\psi}{dt^2} &= \gamma^2 \left( \frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} \right)
\end{align*}
\]

(8)

with corresponding equations for the other body; he finds

\[ c^2 = \gamma^2(a^2 + b^3) = \gamma_1^2(a_i^2 + b^3) = g^2(-a_i^2 + b^3) = g_1^2(-a_i^2 + b^3); \]

from these equations, since \( g \) and \( g_1 \) are infinite, we find

\[ b = a' = a_i'. \]

Mr. Green then substitutes (7) in equations (6), and thus finds results from which may be deduced the formulæ (2) and (3).

If, instead of supposing \( g \) and \( g_1 \) to be infinite, we merely suppose them to be very great compared with \( \gamma \) and \( \gamma_1 \), we shall obtain expressions containing an additional constant, and which therefore can be reconciled in the most satisfactory manner with observation.

Let us suppose, in fact,

\[
\begin{align*}
\frac{c^2}{g^2} &= -a_i^2 + b^3 = \beta^2 \\
\frac{c^2}{g_1^2} &= -a_i^2 + b^3 = \mu^2 \beta^2
\end{align*}
\]

(9)

where \( \beta \) is not zero, but small; and substituting equations (7) in the equations of condition (6), we find from the last two equations (6),

\[
\begin{align*}
A &= \mu^2 A_i \\
B &= \mu^2 B_i \\
\alpha \cos e + \beta \cos e_i &= \mu^2 \alpha_i \\
\alpha \sin e + \beta \sin e_i &= 0
\end{align*}
\]

(10)

which are the same as Mr. Green's equations (23); and from the first two equations, remembering the relations (10), and writing \( \frac{\alpha}{\beta} = \epsilon \), we obtain

\[
\begin{align*}
A - A_i + \frac{a_i}{b} \alpha_i + \frac{a_i}{b} (\beta \cos e_i - \alpha \cos e) &= 0 \\
B - B_i + \frac{a_i}{b} (\beta \sin e_i - \alpha \sin e) &= 0 \\
A \sqrt{1-\epsilon^2 + A_i} \sqrt{1-\mu^2 \epsilon^2} &= 0 \\
B \sqrt{1-\epsilon^2 + B_i} \sqrt{1-\mu^2 \epsilon^2 + (\mu^2 - 1) a_i} &= 0
\end{align*}
\]

(11)

The supposition \( \epsilon = 0 \) would reduce the last two equations to
those used by Mr. Green (24); the first two are the same as
two of Mr. Green's equations. This supposition is equivalent
to supposing the normal wave velocity to be infinite.

From (10) and (11) it is easy to deduce the following:

\[
\begin{align*}
\alpha \cos \epsilon &= \frac{1}{2} \left( \mu^2 + \frac{a_1}{a} \right) \alpha_i \\
\beta \cos \epsilon_i &= \frac{1}{2} \left( \mu^2 - \frac{a_1}{a} \right) \alpha_i \\
\alpha \sin \epsilon &= -\frac{1}{2} \frac{b}{a} \frac{\left(\mu^2 - 1\right)^2}{\sqrt{1 - \mu^2 \epsilon^2 + \mu^2 \sqrt{1 - \epsilon^2}}} \alpha_i \\
\beta \sin \epsilon_i &= -\frac{1}{2} \frac{b}{a} \frac{\left(\mu^2 - 1\right)^2}{\sqrt{1 - \mu^2 \epsilon^2 + \mu^2 \sqrt{1 - \epsilon^2}}} \alpha_i 
\end{align*}
\]

(12)

Dividing the first of these equations by the third, and the
second by the fourth, and writing

\[
Q = \frac{\sqrt{1 - \mu^2 \epsilon^2 + \mu^2 \sqrt{1 - \epsilon^2}}}{\left(\mu^2 - 1\right)^2} 
\]

(13)
we find (since \(\mu = \frac{\sin i}{\sin \gamma}, \frac{a_1}{a = \tan i, \text{ and } \frac{b}{a = - \tan i)},
\]

\[
\cot \epsilon = Q(\mu^2 \cot i + \cot \gamma) \\
\cot \epsilon_i = -Q(\mu^2 \cot i - \cot \gamma)
\]

(14)
which agree with equations (2).

Again, dividing the sum of the squares of the second and
fourth by the sum of the squares of the first and third, we find,
writing for shortness

\[
A = \mu^2 \cot i + \cot \gamma \\
B = \mu^2 \cot i - \cot \gamma \\
J^2 = \frac{\beta^2}{\alpha^2} = \frac{Q^2 B^2 + 1}{Q^2 A^2 + 1} 
\]

(15)

Hence we obtain

\[
\frac{J^2}{\alpha^2} = \frac{\sin^2 (i + \gamma)}{\sin^2 (i - \gamma)} \times \frac{Q^2 B^2 + 1}{Q^2 A^2 + 1} 
\]

(16)
which is reducible to the form (3).

As it has been shown that equations (2) and (3) will represent
accurately the laws of reflected polarized light, provided the value
of Q be suitably determined, it is plain that the equations (14)
and (16) will represent the observations, as they contain two
constants, one of which only is known independently of the laws
of reflexion, and the other is determined by the observations
themselves.

Having thus shown that equations agreeing in form with those
found by Mr. Green may be obtained without the restriction
which determines the value of Q, and having shown that these
equations represent the observations of M. Jamin equally well as the equations given by M. Cauchy, it becomes interesting to inquire into the reason of this agreement.

The principles from which Mr. Green and M. Cauchy set out are the same; viz. 1st, the equivalence of vibrations; 2nd, the continuity of movement, or equality of coefficients of Lagrange's function for the two bodies; 3rd, the evanescence of the normal waves. It is plain that the third of these conditions may be introduced into the equations which express the first two in many different ways, by selecting different particular integrals for the equations of motion. Mr. Green has selected one method and M. Cauchy another; they have arrived at formulæ differing in form, but equally well representing the observations. This agreement gives a high degree of probability to the common principles from which both geometers have started; while at the same time the difference in the form of the results must render us cautious in pronouncing positively in favour of either.

I would venture to propose equations (14) and (16) as empirical formulæ to represent the laws of reflexion of plane polarized light at the surface of uncrystalline transparent bodies; they admit of easy calculation and comparison with experiment, and may be used provisionally until better can be deduced from theory. Of this, however, I confess I have little hope, so long as the molecular conditions to be fulfilled at the bounding surface of two bodies remain to be supplied by our conjectures, and cannot be obtained by direct experiment.

Trinity College, Dublin,
June 20, 1853.

XI. On the Atomic Weight and Constitution of the Alcohols.
By Francis Wrightson, Ph.D.*

That class of bodies which in organic chemistry we understand under the term alcohols, and especially that termed æthyle-alcohol, which may serve as the type of the whole class, has ever been an object of particular attention to the chemist. Although, however, the latter is among the easiest of all organic compounds to prepare and to analyse, and notwithstanding both its formation and its numerous metamorphoses under various reagents have been very carefully studied, nevertheless the views of chemists differ widely both as to its rational constitution and its atomic weight.

Since Berzelius exchanged his earlier view of alcohol, considering it namely as \( \text{C}_2 \text{H}_5 \text{O} \), that is, the oxide of the compound

* Communicated by the Author.
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radical \( \text{C}_2^2 \text{H}_3 \), for the more recent and generally adopted one in which it is considered as the hydrated oxide of æthyle \( = \text{C}_4^4 \text{H}_5^5 \text{O} \). No other hypothesis worthy of note has been propounded until the very plausible one recently proposed by Williamson*; one undoubtedly deserving of attention, explaining as it does, in so simple and unconstrained a manner, a number of phenomena observed by Williamson and others.

Williamson’s researches led him to the conclusion that water, considered as a compound of one atom of oxygen with two atoms of hydrogen \( \text{H}_2^1 \text{O} \), one or both of which are replaceable by atoms of other elements, or by compound bodies such as \( \text{C}_4^4 \text{H}_5^5 \), might form the type of a large number of compounds such as \( \text{H}_2^1 \text{O} \) potass-hydrate, \( \text{C}_2^2 \text{H}_5^5 \) alcohol, \( \text{C}_2^2 \text{H}_5^5 \) O, æther, &c.

This hypothesis is based principally upon the fact, that kaliæthylat (I retain the name for simplicity sake) and iodide of æthyle afford, by mutual reaction, iodide of potassium and æther,

\[
\begin{align*}
\text{C}_2^2 \text{H}_5^5 & \text{O} + \text{C}_2^2 \text{H}_5^5 \text{. i} = \text{k} \cdot \text{i} + \text{C}_2^2 \text{H}_5^5 \text{O}.
\end{align*}
\]

And further, that by treating the æthyle-kalium compound with iodide of methyle, or the methyle-kalium compound with iodide of æthyle, the body \( \text{C}_2^2 \text{H}_5^5 \text{O} \), and similarly, from the amyle-kalium compound and iodide of æthyle the body \( \text{C}_2^2 \text{H}_5^5 \text{O} \) is formed.

Chancel’s researches‡, in which he obtained several bodies identical with the above by a different process, viz. by the dry distillation of the sulphovinates, methylates, &c., with kaliæthylat, methylat, &c., led him to adopt Williamson’s interpretation, which applies here with equal seeming simplicity and conclusiveness,

\[
\begin{align*}
\text{C}_2^2 \text{H}_5^5 \text{SO}^4 & + \text{C}_2^2 \text{H}_3^3 \text{O} = \text{k} \cdot \text{k} \cdot \text{SO}^4 + \text{C}_2^2 \text{H}_3^3 \text{O}.
\end{align*}
\]

* 
† Williamson assumes with Gerhardt the equivalent of hydrogen, nitrogen, chlorine, potassium, sodium, &c. to be one-half the generally accepted equivalent. As symbols for their equivalents, where they are half the usual equivalent, I have used the small letters, a, b, &c. \( \text{H} = 0 \cdot 5 \), \( \text{N} = 14 \cdot 5 \) &c. = 17.7, \( \text{Cl} = 35 \cdot 5 \), k = 19.5(K = 39.0); - C = 6; O = 8.
‡ Comptes Rendus des Travaux de Chimie, par Gerhardt et Laurent, 1850, pp. 369, 403, 405.
the latter body being identical with that previously obtained by Williamson from kali-æthylat and iodide of methyle. I need scarcely advert to the double carbonates, oxalates, &c. obtained by Chancel; because while their formation could be described by similar equations, their rational constitution and atomic weight were not more determinate à priori than those of Williamson’s new bodies. It was the absence of this essential element in the new theory (namely, the establishment of the equivalent weights of the new bodies from which so much was deduced, in accordance with the long accepted equivalent weights of other bodies) which led me to doubt the deductions already detailed of Williamson and Chancel.

But Williamson, in extending his theory of the alcohols over their respective acids, appeared to me to furnish a hint for more rigid experimental proof. Hitherto the ‘othyle’ theory has remained uncriticised: some chemists, evidently dazzled by the apparently very simple interpretation which it affords for a number of phenomena, have taken it for granted without further question; others, more cautious, appear to wait the result of more extended experiment. It was under these circumstances, that, with the advice of Prof. Kolbe, I undertook the present investigation, which appeared to promise in its results conclusive evidence upon the question at issue, viz. whether, of two atoms of hydrogen when combined with another element, generally, as for example, when combined with one equiv. of O in water or with one equiv. of Cl in hydrochloric acid, either or both of these atoms can be replaced by another simple element or by a compound organic radical. A direct proof of this would appear to me a powerful argument in favour of the new theory of the alcohols and ethers. Before describing my experiments, it will be necessary for me to refer more particularly to Williamson’s theory of the alcohol acids, and to show from certain known facts that it is untenable. Before the proof which Williamson derives for his view of acetic acid from the formation of valeracetone (by distillation of a mixture of acetate and valerianate of potash) can be considered at all conclusive, we must possess more certain knowledge as to the rational constitution of acetone itself than at present, while its equivalent weight is so doubtful. Neither must we forget that the formation and constitution of valeracetone can be represented in several ways.

Williamson, in assuming the rational formula of hydrated acetic acid to be \( \text{C}_2\text{H}_3\text{O}_\text{h}^\text{h} \) with half the usual atomic weight, appears to have entirely overlooked the experiments of Kolbe* and Frankland upon the decomposition of acetate of potash by

the galvanic current, and upon the transformation of cyanide of methyle into acetic acid, as well as those so beautifully verifying the former, made by Dumas, Malaguti and Leblanc*, upon the reconversion of acetate of ammonia into cyanide of methyle, facts certainly meriting a careful consideration in the establishment of a new hypothesis. I have at least seen no reference to them in the foregoing papers, or any indication as to how they are to be interpreted without halving the equivalent of potash, and of course all other equivalents when needful,—so sweeping a change that few probably would be prepared to subscribe to it without much more conclusive reasons than we have at present. Williamson must undoubtedly admit that 'othyle' is a purely hypothetical body, for whose existence in acetic acid no decisive fact can be adduced; neither will he deny that there are many of a very conclusive nature to support the view that it contains

the radical methyle $C_2H_3$.

To show, however, the relation of the othyle theory to the before-named important facts, and convey the conviction that it is no longer fairly tenable, it will perhaps be sufficient to compare the equations by which the transformations are represented according to each view. It will doubtless be objected, that the simplicity of a formula representing a chemical change is by no means the test of its truth; where, however, it is in harmony with the majority of important facts, it will have the preference.

\[
\begin{align*}
\text{Acetate of potash.} & \quad \text{Methyle.} \\
KO\cdot(C_2H_3)C_2O_3 + O = C_2H_3 + KO\cdot CO_2 + CO_2 \quad (\text{Kolbe}). \\
\text{Acetate of potash.} & \quad \text{Methyle.} \\
2\left(C_2H_3 OH\right) + O = 2(\text{Ch}_3) + k^2 \cdot CO_3 + CO_2 \quad (\text{Williamson}). \\
\text{Cyanide of methyle.} & \quad \text{Acetate of potash.} \\
(C_2H_3)\cdot C_2N + KO\cdot HO + 2HO = KO\cdot(C_2H_3)C_2 , O_3 + H_3N \quad (\text{Kolbe and Frankland}). \\
\text{Cyanide of methyle.} & \quad \text{Acetate of potash.} \\
2((C_2H_3)\cdot C_2N) + k^2 \cdot O.h_2O + 2h_2O = 2\left(C_2H_3O\right) + 2(h_3n) \quad (\text{Williamson}). \\
\text{Acetate of ammonium-oxide.} & \quad \text{Cyanide of methyle.} \\
H_4N.O\cdot(C_2H_3)C_2 , O_3 + xPO_5 = (C_2H_3)\cdot C_2N + 4HO + xPO_5 \quad (\text{Kolbe}).
\end{align*}
\]

Acetate of ammonium-oxide, Cyanide of methyle.

\[ 2 \left( \frac{C^2 H^3 O}{n} \right) + xP0^5 = 2 \left( \frac{(C H^3).C n}{2} \right) + 4H^2 O + xP0^5 \]  
(Williamson).

No further argument will be needful, I think, to show that Williamson's 'othyle' theory is not based upon scientific principles. Notwithstanding this, however, his theory of the alcohols and aethers may be true. If the assumptions, however, upon which it is founded are correct, we shall have so far to modify our views of the constitution of acetic acid expressed in the formula \( H(O . (C^2 H^3)C^2 O^3) \), that instead of one equivalent of methyle, we must assume two atoms \( 2Ch^3 \) to exist in one equivalent of acetic acid, and its constitution would be expressed by the rational formula \( h^2 O . (C^2 h^3)^2 C^2, O^3 \), or rather by

\[
\left. \frac{h^2}{h} \right\} O \left( \frac{C^2 h^3}{C h^1} \right)^2 C^2, O^3;
\]

that of acetic aether by the formula

\[
\left. \frac{C^2 h^5}{C^2 h^5} \right\} O \left( \frac{C h^3}{C h^1} \right)^2 C^2, O^3;
\]

and in these bodies, conformable to Williamson's theory, one atom of the methyle of the radical acetylene may be replaced by other carburetted hydrogens of analogous constitution. We should have, in fact, acetic acids of composition like the following, \( h^2 O . (C^2 h^5)C^2, O^3 \), and \( h^2 O . (C^2 h^1)C^2, O^3 \); among others, we should have a propionic acid with one atom of amyle in the place of one atom of æthyle, namely,

\[
h^2 O . \left( \frac{C^2 h^5}{C^5 h^1} \right)^2 C^2, O^3 = C^9 h^{18} O^4.
\]

The method of obtaining such an acid, supposing it to exist, would be a modification of the process already referred to, discovered by Kolbe and Frankland, in which propionic and capric acids are obtained from the cyanides of æthyle and amyle by the action of potash; when, for instance, a mixture of cyanides of æthyle and amyle in equivalent proportions dissolved in alcohol is digested continuously with an alcoholic solution of potash, a transformation should ensue such as indicated by the following equations, wherein I assume Williamson's view of the cyanides of æthyle and amyle containing equal atoms of cyanogen and æthyle (or amyle) to be correct:
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Cyanide of æthyle. Propionate of potash.

\[ 2(C^2h^5, \text{C n}) + k^2 O + 3h^2 O = k^2 O \cdot (C^2h^5)^2 C^2, O^3 + 2h^3 n. \]

Cyanide of amyle. Caproate of potash.

\[ 2(C^5h^{11}, \text{C n}) + k^2 O + 3h^2 O = k^2 O \cdot (C^5h^{11})^2 C^2, O^3 + 2h^3 n. \]

Contrary to my expectation, the experiments I have made, variously modified and conducted with the most scrupulous care, show that the acid in question does not exist. The products of decomposition of a mixture of cyanides of æthyle and amyle were propionic and caproic acids alone; not the slightest trace of any other acid could be discovered, although a considerable quantity of the cyanides was used in the experiment. I think myself justified in inferring from this negative result, that the radical contained in propionic acid is not composed of two atoms of æthyle, one of which may be substituted by other elementary or compound atoms, but that it consists of one indivisible equivalent; and that therefore Williamson's theory of the alcohols and æthers, if not disproved, certainly loses very much in probability, so much the more as it is very deficient in decisive facts for its support.

The formation of æther by distilling together iodide of æthyle with kali-æthylat appears to me simply and quite satisfactorily explained by assuming that two equivalents of æther are formed,

\[ C^4 H^5 \cdot KO + C^4 H^5 \cdot I = 2C^4 H^5 \cdot O + KI. \]

There is certainly no more difficulty here than in the following:

\[ KO \cdot Cl \cdot O^5 + 6H \cdot Cl = K \cdot Cl + 6HO + 6Cl. \]

We have no more reason to suppose that six separate equivalents of chlorine are set free in the latter case, than that two equivalents of æther are in the former.

It may perhaps be objected, that a combination of two bodies so similar in nature and constitution as methyle-oxide and æthyle-oxide, by the mutual action of kali-æthylat and iodide of methyle, is improbable:

\[ (C^4 H^5)O \cdot KO + (C^2 H^3) \cdot I = (C^4 H^5)O \cdot (C^2 H^3)I + KI. \]

But have we not perfectly analogous cases presented by inorganic chemistry? Äthyle-oxide and methyle-oxide are certainly not more similar in their chemical character than chlorine and iodine, which combine together, not merely in one, but in several proportions. And does not the theory of double salts (as, for
example, that of sulphates of nickel and potassium oxides) in inorganic chemistry, perhaps our safest guide in investigation, justify us in viewing the bodies obtained by Chancel, for example, the oxalo-vino-methylat \[ \frac{C_1H_3}{C_2H_5} O C^2 O^3 \] according to Williamson, as a double oxalate with organic bases?

There appears, therefore, to me very insufficient ground entirely to discard the generally accepted theory of the alcohols and æthers; and although the view here adopted, in which alcohol is considered not as a hydrate of æthyle-oxide, but as a binary compound of the oxides of æthyle and hydrogen, may not be quite free from objections, yet they are comparatively slight, whilst no change in our system of equivalents is required. Perhaps before chemists become agreed upon the subject, new facts will be brought to light still more decisive in their character. My own attention at least will be directed with that aim as early as opportunity permits.

I shall now describe my experiments upon the deportment of the cyanides of æthyle and amyle, when mixed in equivalent proportions and digested with potash in solution.

For the preparation of cyanide of æthyle I first procured 5 lbs. of sulphovinate of potash by the usual process, mixing proper proportions of alcohol and strong sulphuric acid, afterwards neutralizing with milk of lime, and precipitation of the fluid (separated by filtration from sulphate of lime) with carbonate of potash. The slightly alkaline solution, after separating the precipitated carbonate of lime by filtration, was evaporated to syrup consistence, and before it became quite cold was decanted from a portion of sulphate of potash which had crystallized out. The solution when quite cold solidified to a crystalline mass, which was drained and dried over a water-bath. Supho-amylate of potash was prepared in a similar manner from fusel oil, purified by distillation at a temperature of 192°; about 6 lbs. of impure cyanide of æthyle were obtained, which after being shaken with water and purified by fractional distillation, afforded about 9 oz. of pure cyanide of æthyle.
In a similar manner a corresponding quantity of pure cyanide of amyle was prepared from sulpho-amylate of potash.

The cyanides of æthyle and amyle mixed in equivalent proportions and dissolved in alcohol, were boiled with an alcoholic solution of potash in excess, until the smell of ammonia, at first disengaged in large quantities, had quite disappeared. The flask in which the fluids were boiled was connected with a Liebig’s condensing tube, so that the distilled fluids flowed continually back. It required about thirty hours to finish the operation.

The solutions of the salt thus formed, containing potash in excess, were evaporated over a water-bath, adding water from time to time to the mixture until all the alcohol was got rid of, finally evaporated to dryness, and then dissolved in a small quantity of water. Sulphuric acid somewhat diluted and in excess was now added gradually, and the mixture kept cool by surrounding the vessel with snow. After standing a short time, an oily layer separated and rose to the surface; this was separated by a pipette, well washed with water, and dried over chloride of calcium.

*The Acid soluble in Water.*

The wash-water was added to the fluid from which the oily layer was separated, the sulphate of potash which had been formed being first removed, and the whole then distilled until no more volatile acids came over. The distilled fluids were saturated with carbonate of soda, the saline solution evaporated to dryness. The dried salts were placed in a retort, and strong sulphuric acid added by means of a tubulure, very gradually and then carefully distilled. In order to remove a little sulphurous acid from the fluid thus obtained, which was quite soluble in or rather miscible with water, it was agitated for some time with a little dry superoxide of lead. 40 parts of dry carbonate of soda required 88 parts of the volatile acid thus purified for neutralization. Of pure propionic acid 84 parts would have been sufficient. It contained evidently but a small quantity of another acid with a higher atomic weight; the smell indicated it to be caproic acid.

To ascertain if any other acid between the propionic and the caproic acids, namely the one sought for, \( \text{C}_9 \text{H}_9 \text{O}_4 \), were present, I submitted the greater portion of the fluid to fractional distillation. It began to boil at 105° C., at which temperature a few grammes of acid containing a little water distilled over. The temperature then rose quickly to 140° C., afterwards very slowly to 152°. That distilled between 140° and 150° weighed between
2 and 3 oz., and formed three-fourths of the entire quantity. It was nearly pure propionic acid, as shown by the following baryta determination in the baryta salt.

0·1225 grm. of the salt, after once recrystallizing and then dried at 100° C., afforded 0·0994 grm. of sulphate of baryta corresponding to 53·3 per cent. BaO; theory requires 53·8 per cent.

That portion of the fluid which distilled over between 152° C. and 180° C., i.e. the entire residue, was so small that no product having a constant boiling-point could be separated by fractional distillation; the greater part was still tolerably pure propionic acid. Analysis gave 50·0 per cent. carbon and 8 per cent. hydrogen. The hydrate of propionic acid requires 48·6 carbon, and 8·1 hydrogen.

The Oily Acid insoluble in Water.

The oily fluid smelling like caproic acid, which was dried over chloride of calcium, was rectified by once distilling, and then subjected like the first portion to fractional distillation. It began to boil at 130°, a small quantity of propionic acid and water going over; the boiling-point then rose gradually and uniformly to 196°–198° C., at which temperature it remained constant until about half the fluid distilled over as pure caproic acid. Towards the end of the distillation the temperature rose about 7° C., and the residue in the retort began to blacken. The portion distilled over between 130° and 196° was again rectified, and that part received between 155° and 175° C. put aside. This operation was repeated several times, the thermometer each time approaching nearer to a medium temperature between the boundaries indicated. The product from the fourth distillation between 165° and 172° C., about 7 or 8 grms. by weight, still showed no constant boiling-point, nor could any such be observed in any part of the acid distilled between 130° and 198° C.

If in the metamorphosis of the cyanides of æthyle and amyle by the action of potash the new acid $\text{C}_9\text{H}_9\text{O}_4$ had been formed, then the boiling-point should have been between that of butyric and valerianic acids, i.e. about 166° C.

For this reason I examined the fluid obtained between 165° and 172° very carefully, although the uniform rising in its boiling-point gave sufficient proof that I had not a simple acid in my hands, but most probably a mixture of two. 0·1568 grm. of the fluid by combustion with oxide of copper gave 0·3166 grm. of carbonic acid = 55·06 per cent. carbon. The acid $\text{C}_9\text{H}_9\text{O}_4$ would contain the same quantity per cent. of carbon as a mixture of equivalent weights of propionic and caproic acids, i.e. 56·8. To
show that the substance analysed was really a mixture of propionic and caproic acids, another portion of the same product was shaken with a little water; as was to be expected, a portion was dissolved, the rest swam upon the surface of the water, and was separated by a pipette; the aqueous portion was neutralized with carbonate of baryta, and the salt crystallized. The crystals viewed under the microscope were prisms mingled with long fine needles of another salt (caproate of baryta). 0·1189 grm. of the salt dried at 100° C, gave 0·0925 of sulphate of baryta = 51·1 per cent. baryta. The composition of propionate of baryta requires 53·9 baryta; it contained therefore some caproate of baryta. The baryta salt of the acid $\text{C}^9\text{H}^9\text{O}^4$ would require 47·7 per cent. of baryta. The oily portion, which was separated by a pipette, was treated with caustic solution of baryta, and the salt crystallized; it consisted of needle-shaped crystals, fatty to the touch, and proved to be caproate of baryta. 0·1444 grm. of the salt dried over sulphuric acid (as it undergoes slight decomposition at 100° C.), afforded 0·902 of sulphate of baryta, corresponding to 41·1 per cent. of baryta in the salt. The formula

$$\text{BaO} \cdot \text{C}^{12}\text{H}^{11}\text{O}^8 + \text{HO}$$

requires 39·7. From these experiments it is evident that, from the decomposition of the mingled cyanides of æthyle and amyle by the action of potash, no new acid of the composition $\text{C}^9\text{H}^9\text{O}^4$ is formed, but merely a mixture of propionic and caproic acids.

It occurred to me that the desired acid might possibly be formed by a modification of the foregoing process. It was not improbable, that if the cyanides of æthyle and amyle were brought in contact in statu nascentis, they might unite in some what the same manner as Cyanæthine, $\text{C}^{12}\text{H}^{15} \cdot 3\text{C}^2\text{N}$, is formed by acting upon cyanide of æthyle with potassium. If such a compound of cyanide of æthyle and amyle were exposed to the action of potash, it was highly probable that the desired acid $\text{C}^9\text{H}^9\text{O}^4$ would be formed, much easier than from a mere mixture of cyanides of æthyle and amyle,

$$(\text{C}^9\text{h}^5 \cdot \text{C}n + \text{C}^9\text{h}^{11} \cdot \text{C}n) + k^2\text{O} + 3h^2\text{O}$$

$$= (k^2\text{O}, \text{C}^9\text{h}^{11}) \cdot (\text{C}^2, \text{O}^3) + 2h^3 n.$$  

It was possible that such a compound of cyanide of æthyle and amyle might be formed by heating an intimate mixture of the sulphoamylate and the sulphovinate of potash in equivalent proportions, together with two equivalents of cyanide of potassium, and distilling.

I have made the experiment with considerable quantities of the salts, but obtained only a mixture of the cyanides of æthyle and amyle, and from this mixture of course only propionic and caproic acids.

I add the following observations upon some salts of the propionic and caproic acids not hitherto prepared.

**Propionate of Lime**, CaO, C₆H₅O₃+HO, obtained by neutralizing an aqueous solution of propionic acid with fresh precipitated carbonate of lime, crystallizes by spontaneous evaporation of the solution in long and beautiful prisms which group together. It does not alter by exposure to the air, and dried over sulphuric acid it retains one equivalent of water, which it only loses by heating to 100° C. 0·4394 grm. of the salt dried over sulphuric acid lost by heating in water-bath to 100° C. 0·039 grm. = 8·8 per cent. of water, exactly what the formula

$$\text{CaO} \cdot \text{C}_6\text{H}_5\text{O}_3 + \text{HO}$$

requires. 0·3854 grm. of the salt dried at 100° C., on being ignited with the usual precautions, left 0·2098 grm. of carbonate of lime = 30·4 per cent. of lime. The formula CaO · C₆H₅O₃ requires 30·4 lime.

**Propionate of Copper**, CuO, C₆H₅O₂ + HO, obtained in a similar manner, forms very regular octahedral crystals, sometimes with cube facets, of a dark green colour; it is easily soluble in water, and crystallizes best by spontaneous evaporation. These crystals, dried over sulphuric acid, retain one equivalent of water of crystallization, which they lose on heating to 100° C. 0·3951 grm. of salt lost in water-bath 0·0229 grm. of water =7·52 per cent. The formula CuO · C₆H₅O₃ + HO requires 7·9 HO. 0·2218 grm. of the salt dried at 100°, left, after ignition, moistening with NO₅, &c., 0·0798 grm. of oxide of copper =37·68 per cent (calculated 37·9 oxide of copper).

**Propionate of Baryta**, BaO · C₆H₅O₃ + HO, obtained in very large and regular tabular prisms by spontaneous evaporation, contains after drying over sulphuric acid one equivalent of water, which it loses at 100° C. 0·2432 grm. of the dried salt lost at a temperature of 100° C. 0·0146 = 5·97 water, exactly what the formula BaO · C₆H₅O₃ + HO requires.

**Propionate of Amyloxide**, C₁₀H₁₁O · C₆H₅O₃.—I obtained this compound by distilling a mixture of propionate and sulpho-amylate of potash in equivalent proportions. It is a clear colourless fluid, with a very agreeable odour like pine-apple, little soluble in water, but soluble in æther and alcohol in all proportions; it boils at about 155° C. 0·1809 grm. dried over chloride of calcium and rectified, afforded by combustion with oxide of copper 0·4344 grm. CO₂, and 0·1848 grm. HO.
Note on the Porism of the in-and-circumscribed Polygon.

<table>
<thead>
<tr>
<th></th>
<th>Calculated</th>
<th>Found</th>
</tr>
</thead>
<tbody>
<tr>
<td>C⁰₁⁶</td>
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<td>66.6</td>
</tr>
<tr>
<td>H¹⁶</td>
<td>16</td>
<td>11.1</td>
</tr>
<tr>
<td>O⁴</td>
<td>32</td>
<td>22.3</td>
</tr>
<tr>
<td></td>
<td>100.0</td>
<td>100.0</td>
</tr>
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</table>

Caproate of Magnesia crystallizes in small starry groups of fine needles, and appears like the propionates to contain one equivalent of HO after drying over sulphuric acid. 0.1808 grm. of the dried salt gave, on treating with sulphuric acid, 0.083 grm. of sulphate of magnesia =15.3 per cent. of MgO. The formula MgO. C¹⁰ H¹¹ O⁸ + HO requires 15.1 magnesia.

XII. Note on the Porism of the in-and-circumscribed Polygon.

By A. Cayley, Esq.*

The equation of a conic passing through the points of intersection of the conics

\[ U = 0, \quad V = 0 \]

is of the form

\[ wU + V = 0, \]

where \( w \) is an arbitrary parameter. Suppose that the conic touches a given line, we have for the determination of \( w \) a quadratic equation, the roots of which may be considered as parameters for determining the line in question. Let one of the values of \( w \) be considered as equal to a given constant \( k \), the line is always a tangent to the conic

\[ kU + V = 0; \]

and taking \( w = p \) for the other value of \( w \), \( p \) is a parameter determining the particular tangent, or, what is the same thing, the point of contact of this tangent.

Suppose the tangent meets the conic \( U = 0 \) (which is of course the conic corresponding to \( w = \infty \)) in the points \( P, P' \), and let \( \theta, \infty \) be the parameters of the point \( P \), and \( \theta', \infty \) the parameters of the point \( P' \). It follows from my "Note on the Geometrical representation of the Integral \( \int \text{dx} + \sqrt{(x+a)(x+b)(x+c)} \)†,"

and from the theory of invariants, that if \( \Delta \xi \) represent the "Discriminant" of \( \xi U + V \) (I now use the term discriminant in the same sense in which determinant is sometimes used, viz. the

* Communicated by the Author.
† I take the opportunity of correcting an obvious error in the note in question, viz. \( a^2 + b^2 + c^2 - 2bc - 2ca - 2ab \) is throughout written instead of (what the expression should be) \( b^2 c^2 + c^2 a^2 + a^2 b^2 - 2ab - 2bc - 2ca - 2ab \).
Mr. A. Cayley on the Porism of discriminant of a quadratic function

\[ ax^2 + by^2 + cz^2 + 2fyz + 2gzx + 2hxy \]
or \((a, b, c, f, g, h)(x, y, z)^2\) is the determinant

\[ k = abc - af^2 - bg^2 - ch^2 + 2fgh, \]
and if

\[ \Pi \xi = \int_{-\infty}^{\infty} \frac{d\xi}{\sqrt{\Delta_\xi}}, \]
then the following theorem is true, viz.

"If \((\theta, \infty), (\theta', \infty)\) are the parameters of the points \(P, P'\) in which the conic \(U=0\) is intersected by the tangent, the parameter of which is \(\rho\) of the conic \(kU + V = 0\), then the equations

\[ \Pi \theta = \Pi \rho - \Pi k \]
\[ \Pi \theta' = \Pi \rho + \Pi k \]
determine the parameters \(\theta, \theta'\) of the points in question." And again,—

"If the variable parameters \(\theta, \theta'\) are connected by the equation

\[ \Pi \theta' - \Pi \theta = 2\Pi k, \]
then the line \(PP'\) will be a tangent to the conic \(kU + V = 0\)." Whence, also,—

"If the sides of a triangle inscribed in the conic \(U=0\) touch the conics

\[ kU + V = 0 \]
\[ k'U + V = 0 \]
\[ k''U + V = 0, \]
then the equation

\[ \Pi k + \Pi k' + \Pi k'' = 0 \]

must hold good between the parameters \(k, k', k''\).

And, conversely, when this equation holds good, there are an infinite number of triangles inscribed in the conic \(U=0\), and the sides of which touch the three conics; and similarly for a polygon of any number of sides.

The algebraical equivalent of the transcendental equation last written down is

\[
\begin{vmatrix}
1 & k & \sqrt{\Delta k} \\
1 & k' & \sqrt{\Delta k'} \\
1 & k'' & \sqrt{\Delta k''}
\end{vmatrix} = 0
\]

let it be required to find what this becomes when \(k = k' = k'' = 0\), we have

\[ \sqrt{\Delta k} = \Lambda + Bk + Ck^2 + \ldots, \]
and substituting these values, the determinant divides by
the quotient being composed of the constant term $C$, and terms multiplied by $k, k', k''$; writing, therefore, $k = k' = k'' = 0$, we have $C = 0$ for the condition that there may be inscribed in the conic $U = 0$ an infinity of triangles circumscribed about the conic $V = 0$; $C$ is of course the coefficient of $\xi^2$ in $\sqrt{\Delta}$, $i.e.$ in the square root of the discriminant of $\xi^U + V$; and since precisely the same reasoning applies to a polygon of any number of sides,—

Theorem. The condition that there may be inscribed in the conic $U = 0$ an infinity of $n$-gons circumscribed about the conic $V = 0$, is that the coefficient of $\xi^{n-1}$ in the development in ascending powers of $\xi$ of the square root of the discriminant of $\xi^U + V$ vanishes.

It is perhaps worth noticing that $n = 2$, $i.e.$ the case where the polygon degenerates into two coincident chords, is a case of exception. This is easily explained.

In particular, the condition that there may be in the conic* $\begin{align*}
a x^2 + b y^2 + c z^2 &= 0 \end{align*}$
an infinity of $n$-gons circumscribed about the conic $\begin{align*}x^2 + y^2 + z^2 &= 0,\end{align*}$ is that the coefficient of $\xi^{n-1}$ in the development in ascending powers of $\xi$ of $\sqrt{(1 + a \xi)(1 + b \xi)(1 + c \xi)}(1 + \frac{1}{2}a \xi - \frac{1}{8}a^2 \xi^2 + \frac{1}{16}a^3 \xi^3 - \frac{5}{64}a^4 \xi^4 + \&c.) (1 + \frac{1}{2}b \xi - \&c.)$ vanishes; or, developing each factor, the coefficient of $\xi^{n-1}$ in $(1 + \frac{1}{2}a \xi - \frac{1}{8}a^2 \xi^2 + \frac{1}{16}a^3 \xi^3 - \frac{5}{64}a^4 \xi^4 + \&c.) (1 + \frac{1}{2}b \xi - \&c.)$

vanishes.

Thus, for a triangle this condition is $a^2 + b^2 + c^2 - 2bc - 2ca - 2ab = 0$;

for a quadrangle it is $a^3 + b^3 + c^3 - bc^2 - b^2 c - ca^2 - c^2 a - ab^2 - a^2 b + 2abc = 0$;

which may also be written $(b + c - a)(c + a - b)(a + b - c) = 0$;

and similarly for a pentagon, &c.

* I have, in order to present this result in the simplest form, purposely used a notation different from that of the note above referred to, the quantities $ax^2 + by^2 + cz^2$ and $x^2 + y^2 + z^2$ being, in fact, interchanged.
Note on the Porism of the In-and-Circumscribed Polygon.

Suppose the conics reduce themselves to circles, or write
\[ U = x^2 + y^2 - R^2 = 0 \]
\[ V = (x - a)^2 + y^2 - r^2 = 0; \]
R is of course the radius of the circumscribed circle, r the radius of the inscribed circle, and \( a \) the distance between the centres. Then
\[ \xi U + V = (\xi + 1, \xi + 1, -\xi R^2 - r^2 + a^2, 0, -a, 0)(x, y, 1)^2, \]
and the discriminant is therefore
\[ -(\xi + 1)^2(\xi R^2 + r^2 - a^2) - (\xi + 1)a^2 \]
\[ = -(1 + \xi)(r^2 + \xi(r^2 + R^2 - a^2) + \xi^2 R^2). \]
Hence, theorem—

"The condition that there may be inscribed in the circle \( x^2 + y^2 - R^2 = 0 \) an infinity of \( n \)-gons circumscribed about the circle \( (x - a)^2 + y^2 - r^2 = 0 \), is that the coefficient of \( \xi^{n-1} \) in the development in ascending powers of \( \xi \) of
\[ \sqrt{(1 + \xi)(r^2 + \xi(r^2 + R^2 - a^2) + \xi^2 R^2)} \]
may vanish."

Now
\[ (A + B\xi + C\xi^2) = \sqrt{A}\left\{1 + \frac{1}{2}B\frac{\xi}{A} + \left(\frac{1}{2}AC - \frac{1}{8}R^2\right)\xi^2 + \ldots\right\}, \]
or the quantity to be considered is the coefficient of \( \xi^{n-1} \) in
\[ (1 + \frac{1}{2}\xi - \frac{1}{8}\xi^2 + \ldots)\left\{1 + \frac{1}{2}B\frac{\xi}{A} + \left(\frac{1}{2}AC - \frac{1}{8}B^2\right)\xi^2 + \ldots\right\}, \]
where, of course,
\[ A = r^2, \quad B = r^2 + R^2 - a^2, \quad C = R^2. \]

In particular, in the case of a triangle we have, equating to zero the coefficient of \( \xi^2 \),
\[ (A - B)^2 - 4AC = 0; \]
or substituting
\[ (a^2 - R^2)^2 - 4r^2R^2 = 0, \]
that is,
\[ (a^2 - R^2 + 2Rr)(a^2 - R^2 - 2Rr) = 0, \]
the factor which corresponds to the proper geometrical solution of the question being
\[ a^2 - R^2 + 2Rr = 0, \]
Euler’s well known relation between the radii of the circles inscribed and circumscribed in and about a triangle, and the distance between the centres. I shall not now discuss the meaning of the other factor, or attempt to verify the formulae which have been given by Fuss, Steiner and Richelot, for the case of a polygon of 4, 5, 6, 7, 8, 9, 12, and 16 sides. See Steiner, Crelle, vol. ii. p. 289; Jacobi, vol. iii. p. 376; Richelot, vol. v. p. 250; and vol. xxxviii. p. 353.

2 Stone Buildings, July 9, 1853.
XIII. Note on the Integral \( \int dx \div \sqrt{(m-x)(x+a)(x+b)(x+c)} \).

By A. Cayley, Esq.*

In the formulae of my “Note on the Porism of the in-and-circumscribed Polygon,” it is assumed that

\[
U = x^2 + y^2 + z^2 + \frac{1}{m} (ax^2 + by^2 + cz^2)
\]

\[
V = ax^2 + by^2 + cz^2,
\]

and if a new parameter \( \omega \) connected with the parameter \( w \) by the equation

\[
w = \frac{om}{m-\omega}
\]

be made use of instead of \( w \), then

\[
wU + V = \frac{m}{m-\omega} \{ \omega (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 \};
\]

or the equation \( wU + V = 0 \), viz. the equation

\[
\omega (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0,
\]

is precisely of the same form as that considered in my Note on the Geometrical Representation of the Integral \( \int dx \div \sqrt{(x+a)(x+b)(x+c)} \). Moreover, introducing instead of \( \xi \) a quantity \( \eta \), such that

\[
\xi = \frac{m\eta}{m-\eta},
\]

then

\[
\frac{d\xi}{\sqrt{m} \cdot d\eta} = \frac{\sqrt{m} \cdot d\eta}{\sqrt{(m-\eta)(a+\eta)(b+\eta)(c+\eta)}}.
\]

Also \( \xi = \infty \) gives \( \eta = m \), the integral to be considered is therefore

\[
\Pi \eta = \int_{m}^{\infty} \frac{\sqrt{m} \cdot d\eta}{\sqrt{(m-\eta)(a+\eta)(b+\eta)(c+\eta)}};
\]

i.e. if in the paper last referred to the parameter \( \infty \) had been throughout replaced by the parameter \( m \), the integral

\[
\Pi \eta = \int_{0}^{\infty} \frac{d\eta}{\sqrt{(a+\eta)(b+\eta)(c+\eta)}}
\]

would have had to be replaced by the integral \( \Pi \eta \). It is, I think, worth while to reproduce for this more general case a portion of the investigations of the paper in question, for the sake of exhibiting the rational and integral form of the alge-

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* Communicated by the Author.
Note on the Integral $\int dx = (m-x)(x+a)(x+b)(x+c)$.

braical equation corresponding to the transcendental equation
$+ \Pi k + \Pi p + \Pi \theta = 0$. Consider the point $\xi, \eta, \zeta$ on the conic
$m(x^2+y^2+z^2) + ax^2 + by^2 + cz^2 = 0$, the equation of the tangent
at this point is

$$(m+a)\xi x + (m+b)\eta y + (m+c)\zeta z = 0.$$ 

And if $\theta$ be the other parameter of this line, then the line touches

$$\theta(x^2+y^2+z^2) + ax^2 + by^2 + cz^2 = 0;$$
or we have

$$\frac{(m+a)^2\xi^2}{\theta+a} + \frac{(m+b)^2\eta^2}{\theta+b} + \frac{(m+c)^2\zeta^2}{\theta+c} = 0;$$
and combining this with

$$(m+a)\xi^2 + (m+b)\eta^2 + (m+c)\zeta^2 = 0,$$
we have

$$\xi : \eta : \zeta = \sqrt{\frac{b-c}{a+\theta}} \sqrt{\frac{a+\theta}{b+m}} \sqrt{\frac{b+m}{c+m}}$$

$$+ \sqrt{\frac{c-a}{b+\theta}} \sqrt{\frac{b+\theta}{c+m}} \sqrt{\frac{c+m}{a+m}}$$

$$+ \sqrt{\frac{a-b}{c+\theta}} \sqrt{\frac{c+\theta}{a+m}} \sqrt{\frac{a+m}{b+m}}$$

for the coordinates of the point P. Substituting these for $x, y, z$
in the equation of the line $PP'$ (the parameters of which are $p, k$),
viz. in

$$x \sqrt{b-c} \sqrt{(a+k)(a+p)} + y \sqrt{c-a} \sqrt{(b+k)(b+p)}$$
$$+ z \sqrt{a-b} \sqrt{c+k} \sqrt{c+p} = 0,$$
we have

$$\frac{(b-c)}{\sqrt{a+m}} \sqrt{(a+p)(a+k)(a+\theta)} + \sqrt{(c-a)} \sqrt{(b+p)(b+k)(b+\theta)}$$
$$+ \sqrt{(a-b)} \sqrt{(c+p)(c+k)(c+\theta)} = 0,$$

which is to be replaced by

$$\frac{(a+p)(a+k)(a+\theta)}{a+m} = (\lambda + \mu a)^2$$
$$\frac{(b+p)(b+k)(b+\theta)}{b+m} = (\lambda + \mu b)^2$$
$$\frac{(c+p)(c+k)(c+\theta)}{c+m} = (\lambda + \mu c)^2.$$ 

These equations give, omitting the common factor
$$(a+m)(b+m)(c+m),$$
On the Harmonic Relation of two Lines or two Points.

By A. Cayley, Esq.*

THE 'harmonic relation of a point and line with respect to a triangle' is well known and understood†; but the analo-

* Communicated by the Author.
† The relation to which I refer is contained in the theorem, "If on each side of a triangle there be taken two points harmonically related with
The harmonic relation of two lines with respect to a quadrilateral, or between two points with respect to a quadrangle, is not, I think, sufficiently singled out from the mass of geometrical theorems so as to be recognized when implicitly occurring in the course of an investigation. The relation in question, or some particular case of it, is of frequent occurrence in the _Traté des Propriétés Projectives_, and is, in fact, there substantially demonstrated (see No. 163); and an explicit statement of the theorem is given by M. Steiner, _Lehrsätze_ 24 and 25, _Crelle_, vol. xiii. p. 212 (a demonstration is given, vol. xix. p. 227). The theorem containing the relation in question may be thus stated.

**Theorem.** The harmonic relation of two lines with respect to a quadrilateral. "If on each of the three diagonals of a quadrilateral there be taken two points harmonically related with respect to the angles upon this diagonal, then if three of the points lie in a line, the other three points will also lie in a line,"—the two lines are said to be harmonically related with respect to the quadrilateral.

It may be as well to exhibit this relation in a somewhat different form. The three diagonals of the quadrilateral form a triangle, the sides of which contain the six angles of the quadrilateral; and considering three only of these six angles (one angle on each side), these three angles are points which either lie in a line, or else are such that the lines joining them with the opposite angles of the triangle meet in a point. Each of these points is, with respect to the involution formed by the two angles of the triangle, and the two points harmonically related thereto, a double point; and we have thus the following theorem of the harmonic relation of two lines to a triangle and line, or else to a triangle and point.

**Theorem.** "If on the sides of a triangle there be taken three points, which either lie in a line, or else are such that the lines joining them with the opposite angles of a triangle meet in a point; and if on each side of the triangle there be taken two points, forming with the two angles on the same side an involution having the first-mentioned point on the same side for a double point; then if three of the six points lie in a line, the other three of the six points will also lie in a line,"—the two lines are said to be harmonically related to the triangle and line, or (as the case may be) to the triangle and point.

The theorems with respect to the harmonic relation of two lines with respect to the angles on this side, then if three of these points lie in a line, the lines joining the other three points with the opposite angles of the triangle meet in a point,"—the line and point are said to be harmonically related with respect to the triangle.
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points are of course the reciprocals of those with respect to the harmonic relation of two lines, and do not need to be separately stated.

The preceding theorems are useful in (among other geometrical investigations) the porism of the in-and-circumscribed polygon.

2 Stone Buildings,
July 9, 1853.

XV. On the Cause of the Occurrence of Abnormal Figures in Photographic Impressions of Polarized Rings. By G. G. Stokes, M.A., F.R.S., Lucasian Professor of Mathematics in the University of Cambridge*.

The object of the following paper is to consider the theory of some remarkable results obtained by Mr. Crookes in applying photography to the study of certain phenomena of polarization. An account of these results, taken from the Journal of the Photographic Society, is published in the last Number of the Philosophical Magazine†.

In the ordinary applications of photography, certain objects and parts of objects are to be represented which differ from one another in colour, or in brightness, or in both, according to the nature of the substances, and the way in which the lights and shadows fall. In the photograph the objects are represented as simply light or dark. Inasmuch as the photographic power, in relation to a given sensitive substance, of a heterogeneous pencil of rays is not proportional to its illuminating power, the darkness of the objects in a negative photograph is not proportional to, nor even always in the same order of sequence as, their brightness as they appear to the eye. Still, the outlines of the objects and of their parts are faithfully preserved. For although it is conceivable that two adjacent parts of an object, which the eye instantly distinguishes by their colour, should reflect rays of almost exactly equal photographic power in relation to the particular sensitive substance employed, so as to be absolutely indistinguishable on the photograph, or on the other hand, that two parts of an object between which the eye can see absolutely no distinction should yet come out distinct on the photograph, the conditions which would have to be satisfied in order that the forms of a set of objects, suppose coloured patterns, or a painting of the rings of crystals, should be changed in this way by the substitution of one set of outlines for another are so very

* Communicated by the Author.
† Vol. vi. p. 73.
peculiar, that the chances may be regarded as infinity to one that no such changes of form will be produced to any material extent.

But when photography is applied to the representation of phenomena of interference, such as the rings of crystals seen by means of polarized light, the case is in some respects materially different. To take a particular instance, let us suppose the rings of calcareous spar to be viewed by white light, the planes of polarization of the polarizer and analyzer being crossed, so as to give the black cross; and consider the alternations which take place in going outwards from the centre of the field, suppose in a direction inclined at angles of $45^\circ$ to the arms of the cross. At first there are evident alternations of intensity; but very soon the eye, which under such circumstances is but a bad judge of differences of intensity even when the lights to be compared have the same colour, can no longer perceive the differences of illumination, but judges entirely by the difference of tint. The same takes place with nitre, sugar, and other colourless biaxal crystals. Except in the immediate neighbourhood of the optic axis or axes, the rings, which owe their existence and their forms in the first instance to the laws of double refraction and of the interference of polarized light, are in other respects created and their forms determined by the condition of maximum contrast of tint.

Now consider what takes place when an image of such a system is thrown on a sensitive plate, prepared suppose by means of bromide of silver. The rays of any one refrangibility would together form a regular system of rings, which, if these rays were alone present, and if the refrangibility were comprised within the limits between which the substance is acted on, would impress on the plate a system of rings exactly like those seen by means of the same homogeneous rays, provided they belong to the visible spectrum. The same would take place for rays of each refrangibility in particular, and the several elementary systems of rings thus formed are actually superposed when heterogeneous light is used. When the photograph is finished, it exhibits certain alternations of light and shade corresponding to alternations in the total photographic intensity of the rays which had acted on the plate, without any distinction being preserved between the action of rays of one refrangibility and that of rays of another; whereas, when the rings are viewed directly, the eye catches the differences of tint without noticing the difference of intensity, except in the neighbourhood of the optic axis or axes. Of course I am now speaking only of the alternations perceived in following a line drawn across the rings, not of the dark brushes, or of the variation of intensity perceived in passing
along a given ring. Hence, when heterogeneous light is used, the circumstances which determine the rings are so different in the two cases that it is no wonder that the character of the rings seen on a photograph should differ in some respects from that of the rings seen directly.

But not only is a difference of character indicated as likely to take place; a more detailed consideration of the actual mode of superposition will serve to explain some of the leading features of the abnormal rings as observed by Mr. Crookes. Let us take for example calcareous spar, and suppose the transmitted rays to be all of the same refrangibility. In this case the intensity along a given radius vector, drawn from the centre of the cross, varies as the square of the sine of half the retardation of phase of the ordinary relatively to the extraordinary pencil (see Airy’s Tract). If \( i \) be the angle of incidence, the retardation varies nearly as \( \sin^2 i \); and if \( \sin^2 i = r \), we may take, as representing the variations of intensity \( I \),

\[
I = \sin^2(mr^2) = \frac{1}{2}(1 - \cos 2mr^2). \quad \quad (1)
\]

In this expression \( m \) is a constant depending upon the refrangibility of the rays. In the case of calcareous spar the tints of the rings follow Newton’s scale, and \( m \) is very nearly proportional to the reciprocal of the wave-length.

Suppose now that rays of two different degrees of refrangibility pass through the crystal together, and that the photographic intensities of the two kinds are equal. Suppose also that the aggregate effect which the two systems produce together on the plate is the sum of the effects which they are capable of producing separately. The latter supposition, if not strictly true, will no doubt be approximately true if the plate be not too long exposed. Then, if \( m' \) be what the parameter \( m \) becomes for the second system, we may represent the variation of intensity along a given radius vector by

\[
I = \sin^2(mr^2) + \sin^2(m'r^2) = 1 - \cos (m - m' r^2) \cos (m + m' r^2). \quad (2)
\]

Suppose the refrangibilities of the two systems to be moderately different; then the difference between the two parameters \( m, m' \) will be small, but not extremely small, compared with either of them. Hence of the two factors in the expression for \( I \) the second will fluctuate a good deal more rapidly than the first, and will be that which mainly determines the radii, &c. of the rings. If the first factor were constant and equal to 1, its value when \( r = 0 \), the expression (2) would be of precisely the same form as (1), the parameter being the mean of the two, \( m, m' \). However, the first factor is not constant, but decreases as \( r \) increases, and presently vanishes, and then changes sign. Hence
the rings become less distinct than with homogeneous rays, and
presently there takes place a sort of dislocation amounting to
half an order, that is, the bright rings beyond a certain point,
or in other words, outside the circle determined by a certain value
of \( r \), correspond, in regard to the series formed by their radii,
with the dark rings inside this circle, and vice versa. At some
distance beyond that at which the dislocation takes place the
rings become very distinct again; but it is useless to trace further
the variations of the expression for \( I \), because the circumstances
supposed to exist in forming that expression are too remote from
those of actual experiment to allow the interpretation of the
formula to be carried far.

According to the numerical values of \( m \) and \( m' \), a dark ring might
be converted, by the change of sign of the factor \( \cos (m-m')r^2 \),
to a bright ring, or a bright ring into a dark ring, or a ring of
either kind might be rendered broader or narrower than it would
regularly have been. The coalescence of the fourth and fifth
bright rings in Mr. Crookes's photographs when bromide of
silver was used seems to be merely an effect of this nature.

But in order that a dislocation of the kind above explained
should take place, it is not essential that two kinds only of rays
should act, nor even that the curve of photographic intensity
should admit of two distinct maxima within the spectrum.
Suppose that rays of all refrangibilities lying within certain
limits pass through the crystal and fall on the plate. For the
sake of obtaining an expression which admits of being worked
out numerically without too much trouble, and yet results from
a hypothesis not very remote from the circumstances of actual
experiment, I will suppose the total photographic power of the
rays whose parameters lie between \( m \) and \( m + dm \) to be propor-
tional to \( \sin m dm \) between the limits \( m = 2\pi \) and \( m = 3\pi \), and to
vanish beyond those limits. Since \( m \) is very nearly proportional
to the reciprocal of the wave-length, and the ratio \( 3\pi \) to \( 2\pi \) or
3 to 2 is nearly that of the wave-lengths of the fixed lines D, H,
this assumption corresponds to the supposition that the rays less
refrangible than D are inefficient, that the action there com-
ences, then increases according to a certain law, attains a maxi-
mum, decreases, and finally vanishes at H. The action would
really terminate at H if a bath of a solution of sulphate of qui-
nine of a certain strength were used. On this assumption, and
supposing, as before, that the rays of different refrangibilities act
independently of each other, we have

\[
I = \int_{2\pi}^{3\pi} \sin^2 (m r^2) \sin m dm.
\]
On working out this expression, and writing \( x \) for \( 2r^2 \), we find

\[
I = 1 + \frac{\cos \left( \frac{1}{2} \pi x \right) \cos \left( \frac{5}{2} \pi x \right)}{x^2 - 1} \quad \ldots \quad (3)
\]

As the full discussion of this formula presents no difficulty it may be left to the reader. The last factor in the numerator of the fraction is that where fluctuations correspond to the rings. Whenever \( x \) passes through an odd integer greater than 1 the first factor changes sign, and there is a dislocation or displacement of half an order, but when \( x \) passes through the value 1 the denominator changes sign along with both factors of the numerator, and there is no dislocation. When \( x \) becomes considerable the denominator \( x^2 - 1 \) becomes very large, and the fluctuations of intensity become insensible.

The following table contains the values of \( I \) calculated from the formula (3) for 16 values of \( x \) in each of the first 7 orders of rings. In passing from one ring to its consecutive the angle \( \frac{5}{2} \pi x \) increases by \( 2\pi \), and therefore \( x \) by 0.8. The sixteenth part of this, or 0.05, is the increment of \( x \) in the table. Each vertical column corresponds to one order. The value of \( x \) corresponding to any number in the table will be found by adding together the numbers in the top and left-hand columns.

<table>
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<tr>
<th>( x )</th>
<th>0.00</th>
<th>0.80</th>
<th>1.60</th>
<th>2.40</th>
<th>3.20</th>
<th>4.00</th>
<th>4.80</th>
</tr>
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<tbody>
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<td>0.000</td>
<td>0.142</td>
<td>0.481</td>
<td>0.830</td>
<td>1.033</td>
<td>1.067</td>
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<td>0.030</td>
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<td>0.860</td>
<td>1.037</td>
<td>1.000</td>
<td>1.010</td>
</tr>
<tr>
<td>0.10</td>
<td>0.035</td>
<td>0.418</td>
<td>0.667</td>
<td>0.905</td>
<td>1.032</td>
<td>1.044</td>
<td>1.005</td>
</tr>
<tr>
<td>0.15</td>
<td>0.061</td>
<td>0.692</td>
<td>0.829</td>
<td>0.955</td>
<td>1.020</td>
<td>1.021</td>
<td>1.001</td>
</tr>
<tr>
<td>0.20</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>0.25</td>
<td>1.377</td>
<td>1.293</td>
<td>1.154</td>
<td>1.033</td>
<td>0.977</td>
<td>0.979</td>
<td>1.001</td>
</tr>
<tr>
<td>0.30</td>
<td>1.692</td>
<td>1.527</td>
<td>1.268</td>
<td>1.051</td>
<td>0.956</td>
<td>0.964</td>
<td>1.004</td>
</tr>
<tr>
<td>0.35</td>
<td>1.898</td>
<td>1.669</td>
<td>1.327</td>
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<td>0.939</td>
<td>0.956</td>
<td>1.008</td>
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<tr>
<td>0.40</td>
<td>1.963</td>
<td>1.702</td>
<td>1.333</td>
<td>1.045</td>
<td>0.932</td>
<td>0.956</td>
<td>1.012</td>
</tr>
<tr>
<td>0.45</td>
<td>1.881</td>
<td>1.791</td>
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<td>1.030</td>
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<td>0.963</td>
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<td>0.950</td>
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</tr>
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<td>1.103</td>
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<td>0.987</td>
<td>1.007</td>
</tr>
<tr>
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<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
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<td>0.775</td>
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<td>0.600</td>
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<td>1.049</td>
<td>1.015</td>
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<td>0.192</td>
<td>0.499</td>
<td>0.826</td>
<td>1.024</td>
<td>1.063</td>
<td>1.016</td>
<td>0.977</td>
</tr>
</tbody>
</table>

A curve of intensity might easily be constructed from this table by taking ordinates proportional to the numbers in the table, and abscissæ proportional to the values of \( r \), and therefore to the square roots of the numbers 0, 1, 2, 3, 4, &c. But the form of the curve will be understood well enough either from
the formula (3), or from an inspection of the numbers in the table.

It will be seen that in the first three columns the numbers lying between the horizontal lines beginning \(0.20\) and \(0.60\) correspond to bright rings, and the remainder of each column, together with the beginning of the next, corresponds to a dark ring. But the dark ring, which would regularly follow the fourth bright ring, is converted, by the change of sign of the factor \(\cos \left( \frac{1}{2} \pi x \right)\), into a bright ring, forming with the former one broad bright ring having a minimum corresponding to \(x = 3\), where however the intensity falls only down to its mean value unity. A similar displacement occurs in the seventh column, but here the whole variation of intensity is comparatively small.

In the case of calcareous spar the character of the rings is the same all the way round, but in the photographs of the rings of nitre a new feature presents itself. Mr. Crookes's figure of the abnormal rings of nitre is rather too small to be clear, but with the assistance of his description there is no difficulty in imagining what takes place. With reference to these photographs he observes, "But here a remarkable dislocation presented itself; each quadrant of the interior rings, instead of retaining its usual regular figure, appeared as if broken in half, the halves being alternately raised and depressed towards the neighbouring rings."

This effect admits of easy explanation as a result of the superposition of systems of rings which separately are perfectly regular, when we consider that the poles of the lemniscates of the several elementary systems do not coincide, since in nitre the angle between the optic axes increases from the red to the blue. Now the change of character which may be described as a displacement of half an order is due to the circumstance that the smaller rings corresponding to the more refrangible rays are, as it were, overtaken by the larger rings corresponding to the less refrangible. It is plain that the variation of position of the poles of the lemniscates would tend to retard this effect in directions lying outside the optic axes, and to accelerate it in directions lying between those axes. Hence what was a bright ring in one part of its course would become a dark ring in another part, so that each quadrant would exhibit a dislocation of half an order in the rings. In order to show this dislocation to the greatest advantage, a crystal of a certain thickness should be used. With a very thin crystal there would be no dislocation of this nature, but only a displacement like that which takes place with calcareous spar. With a very thick crystal the effect of the chromatic variation of position of the optic axes would be too much exaggerated.
Photographic Impressions of Polarized Rings.

It appears then that all the leading features of the abnormal rings are perfectly explicable as a result of the superposition of separately regular systems. But if known causes suffice for the explanation of phenomena, we must by no means resort to agents whose existence is purely hypothetical, such for example as invisible rays accompanying, but distinct from, visible rays of the same refrangibility. Some of the minor details of the abnormal rings may require further explanation or more precise calculation; but such calculations are of no particular interest unless the phenomena offered grounds for suspecting the agency of hitherto unrecognized causes.

The difference between the photographs taken with iodide and bromide of silver is easily explained, when we consider the manner in which those substances are respectively affected by the rays of the spectrum. With iodide of silver there is such a concentration of photographic power extending from about the fixed line G of Fraunhofer to a little beyond H, that even when white light is employed we may approximately consider that we are dealing with homogeneous rays. On this account, and not because the rays of high refrangibility are capable of producing a more extended system of rings than those of low refrangibility, the rings visible on the photograph are much more numerous than those seen directly by the eye with the same white light. Moreover, the rings do not exhibit the same abnormal character as with bromide of silver, in relation to which substance the photographic power of the rays is more diffused over the spectrum.

It is not possible to place the eye and a sensitive plate prepared with bromide of silver under the same circumstances with regard to the formation of abnormal rings. It would be easy, theoretically at least, to place the eye and the plate in the same circumstances as regards rings, by using homogeneous light; but then, I feel no doubt, the rings visible on the plate would be as regular as those seen by the eye. On the other hand, if differences of colour exist in the figure viewed by the eye, they inevitably arrest the attention, and it is impossible to get rid of them without at the same time rendering the light so nearly homogeneous that on that account nothing abnormal would be shown. Hence Mr. Crookes's abnormal rings afford a very curious example of the creation, so to speak, by photography of forms which do not exist in the object as viewed by the eye.

Pembroke College, Cambridge,
July 8, 1853.
XVI. On the Mutual Attraction or Repulsion between two Electrified Spherical Conductors. By Prof. W. Thomson.*

[Continued from vol. v. p. 297.]

The following numerical results have been calculated (by means of the formulæ established above) for application to the theory of a new electrometer which I have recently had constructed to determine electrical potentials in absolute measure, from the repulsions of uninsulated balls in the interior of a hollow insulated and electrified conductor, by means of a bifilar or torsion-balance bearing a vertical shaft which passes through a small aperture to the outside of the conductor.

Table I.—Showing the Quantities of Electricity on two equal spherical conductors, of radius $r$, and the mutual force between them, when charged to potentials $u$ and $v$ respectively.

<table>
<thead>
<tr>
<th>Col. 1</th>
<th>Cols. 2 and 3.</th>
<th>Cols. 4 and 5.</th>
<th>Col. 6.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from centre to centre $= er$.</td>
<td>For determining the quantities of electricity, $D = \frac{(1u - Jo)r}{r}$, $E = \frac{(1v - Ju)r}{r}$.</td>
<td>For determining the mutual force, $F = 2Bu - A(u^2 + v^2)$; being repulsion when positive, and attraction when negative.</td>
<td>Ratio of the potentials when there is neither attraction nor repulsion, $\rho = \frac{B}{A} \left( \frac{E^2}{A^2 - 1} \right)$.</td>
</tr>
<tr>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
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<tr>
<td>2.0</td>
<td>$\infty$</td>
<td>$-693147$</td>
<td>$\infty$</td>
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<td>23159</td>
</tr>
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<td>26924</td>
<td>20750</td>
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</table>

* Communicated by the Author.
Table II.—Showing the potentials in two equal spherical conductors, and the mutual force between them, when charged with quantities D and E of electricity respectively.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from centre to centre = cr.</td>
<td>For determining the potentials, ( u = \left( \frac{1}{1^2 r^2} \right) D + \frac{J}{1^2 r^2} E ) ( 1^2 r^2 )</td>
<td>For determining the mutual force, ( F = \left{ \frac{2 DE - \alpha (D^2 + E^2)}{r^2} \right} \frac{1}{r^2} ) where ( \alpha = \frac{A (P^2 + J^2) - 2BJJ}{(1^2 r^2)^2} )</td>
<td>Ratio of the quantities when there is neither attraction nor repulsion, ( \theta = \frac{\rho - J}{J - \rho} )</td>
</tr>
<tr>
<td>c.</td>
<td>( \frac{1}{1^2 r^2} )</td>
<td>( \frac{1}{J^2 r^2} )</td>
<td>( \alpha )</td>
</tr>
<tr>
<td>2.0</td>
<td>0.073858</td>
<td>0.073858</td>
<td>( \infty )</td>
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XVII. Mineralogical Notes:—Second Series. By E. J. Chapman, Professor of Mineralogy and Geology in University College, Toronto, Canada West; late Professor of Mineralogy in University College, London.*

1. Staurolilte.—The angle of the rhombic prism in staurolite is generally stated to be equal to 129° 20'. In a small crystal of the common configuration, from St. Gothard, I have found the angle D : L (\( \infty P : \infty \tilde{P}, \infty \), Naumann), fig. 1 = 115° 44', which gives for D : D 128° 32'. Three of the

* Communicated by the Author.
planes of the rhombic prism in the crystal measured were more or less dull, so that \( \mathbf{D}:\mathbf{D} \) could not be conveniently obtained by direct measurement; but, in other respects, the crystal was very symmetrically formed, and its edges were sharp and well-defined. The above value closely approaches the angle \( \mathbf{D}:\mathbf{D} \) obtained by Kengott. As deduced from Kengott's measurements, this equals \( 128^\circ\ 44' \); we may therefore consider the angle to oscillate between \( 128^\circ\ 30' \) and \( 129^\circ\ 30' \). A range of this kind, or one of even greater extent, might be naturally anticipated from the variable constitution of the mineral.

I have never met with a crystal of staurolite in which the side planes \( \mathbf{L} \) were absent, and, in general, these are largely developed; but in the curious compound crystal shown in fig. 2, the planes in question are reduced to the breadth of mere lines. They have been, unavoidably, enlarged in the figure. This crystal comes from the neighbourhood of Quimper. The compound structure is exhibited on only two of the planes of the form \( \mathbf{D} \); so that, on one side, the crystal appears simple, as shown in the drawing.

2. Calcite.—The acute rhombohedron \( 4\mathbf{R} \) in calcite, in the simple or uncombined state, is comparatively a rare form. I have noticed it, however, not unfrequently in the interior of fossil shells or casts. To cite a few particular examples: in the interior of a Spirifer glaber from the carboniferous limestone of Ireland; in a Productus punctatus from the same locality; and in a fractured ammonite of the Upper Jurassic period (\( A.\ Rupe-\) lensis, D'Orbigny; \( A.\ Bakeria, \) Quenstedt), from the vicinity of La Rochelle. The angle over a polar edge = \( 65^\circ\ 50' \) (Contrast-\( \text{ante, Haüy; } e^3,\) Dufrenoy; \( 3\bar{1}\bar{1},\) Miller). I have also a specimen (fig. 3) of a calcareous nodule from the latter locality, the surface of which is crystallized in the form \( 4\mathbf{R} \). Numerous and very distinct lines of stratification or growth pass through the mass of the nodule, as shown in the figure. This specimen was found in a marly clay of the same age as the English coral rag (\( \text{étage corallien, D'Orb.} \)). In all of these cases, the form has evidently resulted from the gradual infiltration of calcareous matter into a more or less resisting medium.
The following simplified formulae for the determination of the relative length of the principal axis in rhombohedrons, from the angle over a polar edge, will be found, I believe, more expeditious in the working than any that have been hitherto proposed. They are adapted to the ordinary logarithmic tables, and are so arranged as to be conveniently employed by persons but little accustomed to crystallographic calculations.

Let \( a \) = half the given angle over a polar edge; \( b \) the inclination of a face of the rhombohedron on the principal axis; and \( p \) the principal or vertical axis required, the other axes being unity. Then

\[
\begin{align*}
\log \cos b &= \log \cos a + 0.0624694; \\
\log p &= \log \cot b - 10.0624694.
\end{align*}
\]

Or, let \( a \) and \( p \) stand as above, and let \( c \) = the inclination of a polar edge on the principal axis. Then

\[
\begin{align*}
\log \cos c &= \log \cot a - 0.2385606; \\
\log p &= \log \cot c - 9.7614394.
\end{align*}
\]

If the rhombohedron under investigation be not assumed as the protaxial form, the value of \( p \), thus found, must be compared with the protaxial value. Thus, in calcite, \( p \) in the form \( \text{R} = 0.854 \), and in the form \( 4\text{R}, 3.416 \).

The accompanying figures may serve to illustrate the construction of these equations; but it should be remarked, that the letters in the figures have no reference to those given above.

I. Fig. 4.

[Fig. 4.]
\( A = 60^\circ; \quad B = \frac{R}{2}; \quad C = 90^\circ. \)
Here \( A \) and \( B \) are given to find \( b \).

\[
\cos b = \frac{\cos B}{\sin A}.
\]

[Fig. 5.]
\( A = 30^\circ; \quad C = 90^\circ; \quad b = b \) found by the first equation.
Here \( A \) and \( b \) are given to find \( c \).

\[
\cot c = \frac{\cos A}{\tan b}.
\]
Prof. Chapman’s Mineralogical Notes.

Fig. 6.

A = the value $c$ found by the last equation; $a = 1$. $p$ is required.

$$p = \cot A.$$  

II.

[Fig. 4.]

$$\Lambda = 60^\circ; \quad B = \frac{R}{2}; \quad C = 90^\circ.$$  

$A$ and $B$ are given to find $c$, the angle between a polar edge and the principal axis.

$$\cos c = \cot \Lambda \cot B.$$  

(See fig. 4.)  

[Fig. 7.]

$$B = \frac{R}{2}; \quad C = 90^\circ (60^\circ + 30^\circ);$$  

$a$ = the angle between the principal axis and a polar edge, as found by the preceding operation. Here $B$ and $a$ are given to find $b$.

$$\tan b = \frac{\sin a}{\cot B}.$$  

[Fig. 6.]  

$A$ = the value $b$, found in the last operation; $a = 1$. $p$ is required.

$$p = \cot A.$$  

(See fig. 6.)  

3. Absorption of Water by Chalk.—The following results (reduced to one uniform term of comparison) of a series of experiments on the absorbent power of chalk, were performed for the London (Watford) Spring Water Company in the early part of last year. They confirm in all essential respects the prior investigations of Professor Ansted.

(1.) Chalk from Watford, Herts: spec. grav. = 2.503. Water absorbed by 100 parts = 18.76.

(2.) Chalk from Ware: spec. grav. = 2.511. Water absorbed by 100 parts = 18.68.

(3.) Chalk from the hills above Worthing: spec. grav. 2.506. Water absorbed by 100 parts = 17.91.
(4.) Chalk from the neighbourhood of Bletchingly in Surrey: spec. grav. = 2.49. Water absorbed by 100 parts = 15.60.

(5.) Chalk from Tring: spec. grav. = 2.482. Water absorbed by 100 parts = 15.12.

(6.) Chalk from Luton in Bedfordshire: spec. grav. = 2.477. Water absorbed by 100 parts = 14.94.

The mean of these results gives 16.83 for the amount of water absorbed by 100 parts of chalk, corresponding to rather more than 2.4 gallons per cubic foot, or to very nearly 215 millions of gallons per square mile of one yard in thickness. The absorbent power, on the whole, may be greater in the upper chalk beds than in those of the middle chalk; but it should be observed, that hard layers of comparatively low absorbent power occur indifferently both in the upper and in the middle chalk, and, indeed, in the lower division also; for here, as in all other cases, the mineralogical quality of the rock is entirely a local phænomenon.

In addition to the above, specimens of chalk and chalk marl from several other localities were likewise tested, but they fell to pieces during saturation. One of the latter kind from the vicinity of Eastbourne in Sussex was of so absorbent a nature, that a specimen weighing 486.3 grs., and consequently of the bulk of rather more than three-fourths of a cubic inch, crumbled into powder, with a loud hissing noise, in less than ten minutes. Under the microscope, the powder exhibited a multitude of Foraminifera belonging to the genera Lituola, Textularia and Cuneolina, unless the forms which I attributed to the latter genus were those of a very cuneiform species of Textularia. I could not clearly make out the openings.

Large masses and even layers of considerable extent of a hard clunch-like substance called "pan" by the well-borers, from the fact that it usually lines the water-channels in the chalk, occur in many localities. A specimen from Hertfordshire which I examined, and which was taken from a depth of more than 100 feet from the surface, consisted of a subcrystalline and more or less siliceous limestone. H. = 3.0 to 3.5; spec. grav. = 2.54. Amount of water absorbed by 100 parts = 3.45. Insoluble silicate of alumina was present to the amount of 8.28 per cent., the remaining portion consisted of carbonate of lime with traces of the carbonates of manganese, iron and magnesia.

In some borings near Brighton these layers of "pan" were not met with; but the curious fact was brought to light of the existence of a bed of brown clay, 16 inches thick, at a depth, of 150 feet from the surface. A second clay bed of a blue colour 8 inches thick, was passed through at a further depth of 34 feet (or 184 feet from the surface), the boring still continuing in the chalk.
4. Tungstate of Lead.—Scheeleite.—Amongst some tabular crystals of molybdate of lead, on a specimen from Coquimbo (?) in Chile, I observed several elongated and slightly curved forms, which at first sight I took to be tungstate of lime. On exposing a particle, however, to the action of the blowpipe, the flame assumed the peculiar blue colour indicative of the presence of lead, but without the accompanying green tint at the point and sides, which is always produced by the molybdate. On charcoal, with soda, globules of lead were obtained; and a more extended examination showed the presence of tungstic acid, oxide of lead, and lime. The general configuration of the crystals is exhibited in the annexed drawing. \[A : A \text{ over a middle edge } = 114^\circ 32' ; O : O = 131^\circ \text{ to } 131^\circ 30'.\]

The latter angle could only be approximately measured, owing to the unfavourable condition of the faces. Calculation shows that it should equal \(131^\circ 6'.\) The cleavage seemed parallel to the position of the basal planes \(P\) (OP of Naumann), and the points of many of the crystals were thus broken off. Colour pale yellowish-gray; \(H. 3-0; \text{ spec. grav. } 7-87.\)

17.26 grs. yielded on analysis:—tungstic acid, 10.27; oxide of lead, 5.74; lime, 1.10; corresponding to the per-cent-age composition.

\[
\begin{align*}
\text{Tungstic acid} & \quad 59.50 \\
\text{Oxide of lead} & \quad 33.26 \\
\text{Lime} & \quad 6.37 \\
\hline
\text{Total} & \quad 99.13
\end{align*}
\]

These values lead to the common formula \((\text{PbO, CaO}), \text{WO}_3\), in which a portion of the oxide of lead is replaced by lime.

In another trial, 12.85 grs. of the mineral gave 7.69 grs. of tungstic acid, or 59.84 per cent.

The mineral in a very fine state of subdivision was decomposed by fusion with five parts of carbonate of soda and one part of borax. The alkaline tungstate thus formed was dissolved in water, and the residue well washed with water containing a little ammonia. In several preliminary trials it was found that not a trace of tungstic acid was left in the residue after this treatment. Owing probably to the presence of tungstate of lime in the mineral, the decomposition was also found to be much facilitated by the addition of the borax. The tungstic acid was precipitated by nitric acid, redissolved in ammonia, which left a trace of silica, and again thrown down by nitric acid. The residue of the first treatment was dissolved in diluted nitric acid, the lead thrown down as sulphide, and the precipitate converted into sulphate. The lime was also weighed as sulphate.
5. *Tinstone Pseudomorphs.*—The well-known pseudomorphs of tinstone from Cornwall—after feldspar twins, flattened parallel to the side planes \( L \), or \( \infty P \infty \) of Naumann—consist almost invariably of a granular mixture of cassiterite, quartz, and decomposed feldspar. Two specimens showed respectively, the following specific gravities: 5·313 and 5·68. The first, in a blowpipe assay, yielded 48·6 per cent. of metallic tin, which is equivalent to a per-centage of 55·46 of the binoxide. In the centre of this specimen two little crystals of pure cassiterite were met with, proving the more or less chemical origin of the pseudomorphs. For their mode of production, we must look probably to the action of chlorides in solution or sublimation.


XVIII. *On Molecular Influences.*—Part I. *Transmission of Heat through Organic Structures.* By John Tyndall, F.R.S. *

The various solid substances which are met with in nature allow themselves to be classed under three general heads:—Amorphous, Crystalline and Organized. In amorphous bodies the component particles are confusedly mingled, without any regard to symmetry of arrangement. In crystalline bodies, on the contrary, the particles are symmetrically arranged; the mass appears as if built up according to certain architectural rules, and the result is an exterior form whose angular dimensions are perfectly constant for all crystals of the same class. Organized bodies, as the name implies, are bodies endowed with, or composed of, organs formed with reference to the special functions they are intended to discharge, and in the construction of which a molecular architecture of a very composite order comes into play. The granules, cells, glands, tubes, &c. of animal and vegetable tissues are all, of course, the visible products of this architecture. Crystalline bodies appear to bridge the chasm which separates the amorphous from the organized. Like the former, they are devoid of the powers of assimilation and reproduction—like the latter, their particles are arranged according to rule; as if nature, in the case of crystals, had made her first structural effort. The student of nature has ever looked upon these molecular combinations with an inquiring eye, and, perhaps, at no age of the world more than at present. The molecular peculiarities of any substance declare themselves by the manner in which a force is modified in its passage through the substance. The polarization and bifurcation of a luminous ray in doubly refracting media is an old example of molecular action; and the rotation of the plane of polarization, observed by Professor Faraday, may be the result of a mechanical change of the me-

* From the Philosophical Transactions for 1853, part ii.; having been received by the Royal Society October 20, 1852, and read January 6, 1853.
diurn, effected by the current or the magnet. Senarmont's* and Knoblauch's† experiments demonstrate the influence of crystalline structure upon the transmission of heat; and the magnetocrystalline discoveries of Plücker and Faraday receive, I believe, their true explanation by reference simply to the modification of the magnetic and diamagnetic forces which peculiarity of aggregation induces. Matter, in this aspect, may be regarded as a kind of organ through which force addresses our senses; if the organ be changed, it is reasonable to infer that the utterance will be correspondingly modified,—an inference which is abundantly corroborated by experiment. Thus, mechanical pressure will polarize a ray, and the same may be applied with success to produce all the phænomena of magnetocrystalline action. The anomalies which owe their origin to peculiarities of aggregation are indeed manifold, and constitute one of the most important subjects of study which can engage the attention of the natural philosopher.

Organic structures furnish an ample field for inquiries into molecular action. For here, as before remarked, nature, to attain her special ends, has arranged her materials in a particular manner. To ascertain what effect the molecular structure of wood has upon the transmission of heat through it, constitutes the object of the first part of this investigation.

Upwards of twenty years ago MM. De la Rive and DeCandolle instituted an inquiry into the conductive power of wood‡, and in the case of five specimens examined established the fact of the feeble conductivity of the substance, and also that the velocity of transmission was greater along the fibre than across it. The manner of experiment was that usually adopted in inquiries of this nature, and applied to metals by M. Depretz.§ A bar of the substance was taken, one end of which was brought into contact with a source of heat and allowed to remain so until a stationary temperature was assumed. The temperatures attained by the bar, at various distances from its heated end, were ascertained by means of thermometers fitting into cavities made to receive them; from these data, with the aid of a well-known formula, the conductivity of the wood was determined. Since the publication of their results by the distinguished men above mentioned, nothing, so far as I am aware of, has been done in connexion with this subject.

The mode of experiment here indicated is, however, by no means sufficiently delicate for an inquiry like the present. Some other mode must therefore be devised. I will not trouble the

* Annales de Chimie et de Physique, vols. xxi. xxii. xxiii.
§ Annales de Chimie et de Physique, December 1827.
reader with a rehearsal of the long series of trials which have led to the construction of the instrument employed in these researches, but will proceed at once to the description of it.

QQ' RR', fig. 1, is an oblong piece of mahogany 3•4 inches long, 1•8 inch wide and 0•5 of an inch deep. A is a bar of antimony, B is a bar of bismuth, each measuring 1•5 inch in length, 0•07 of an inch in breadth, and 0•3 of an inch in depth. The ends of the two bars are kept in close contact by the ivory jaws I, I', and the other ends are let into a second piece of ivory i, in which they are firmly fixed. Soldered to these ends are two pieces of platinum wire which proceed to the little ivory cups MM', enter through the sides of the cups and communicate with a drop of mercury placed in the interior. The wood is cut away, so that the bars A and B are sunk to a depth which places their upper surfaces a little below the general level of the slab of mahogany. The ivory jaws I, I' are sunk similarly. Two small projections are observed in the figure jutting from I, I'; across from one projection to the other a fine membrane is drawn, thus enclosing a little chamber m, in front of the wedge-like end of the bismuth and antimony junction; the chamber has an ivory bottom. S is a wooden slider, which can be moved smoothly back and forward along a bevelled groove, by means of the lever L. This lever turns on a pivot near Q, and fits into a horizontal slit in the slider, to which it is attached by the pin p' passing through both; in the lever an oblong aperture is cut through which p' passes, and in which it has a certain amount of lateral play, so as to enable it to push the slider forward in a straight line. A small chamber, m', is cut out at the end of the slider, and across, from projection to projection, a thin membrane is stretched; a chamber is thus formed bounded on three sides and the bottom by wood, and in front by the membrane. A thin platinum wire, bent up and down several times, so as to form a kind of micrometer grating, is laid against the back of the chamber and imbedded in the end of the slider by the stroke of a hammer; the end in which the wire is imbedded is then filed down until about half the latter is removed, and the whole is reduced to a uniform flat surface. Against the common surface of the slider and wire an extremely thin plate of mica is glued, sufficient, simply, to interrupt all contact between the bent wire and a quantity of mercury which the chamber m' is destined to contain: the ends w w' of the bent wire proceed to two small cisterns, c c', hollowed out in a slab of ivory; they enter through the substance into the cisterns, and come thus into contact with mercury which fills the latter. The end of the slider and its bent wire are shown in fig. 2. The rectangular space efgk, fig. 1, is cut quite through the slab of mahogany, and a brass plate is screwed to the latter underneath; from this plate (which, for
Dr. Tyndall on Molecular Influences.

reasons to be explained presently, is cut away, as shown by the dotted lines in the figure) four conical ivory points, abcd, project upwards; though appearing to be upon the same plane as the upper surfaces of the bismuth and antimony bars, the points are in reality 0·3 of an inch below the said surfaces.

The body to be examined is reduced to the shape of a cube, and is placed, by means of a pair of pliers, upon the four supports abcd; the slider S is then drawn up against the cube, and the latter becomes firmly clasped between the projections of the piece of ivory II', on the one side, and those of the slider S, on the other. The chambers m m' being filled with mercury, the membrane in front of each is pressed gently against the cube by the interior fluid mass, and in this way perfect contact is secured. In fact the principle here applied is the same as that made use of by Fourier* in his thermometer of contact, although both instruments have nothing else in common.

The problem which requires solution is the following:—It is required to apply a source of heat of a strictly measurable character, and always readily attainable, to that face of the cube which is in contact with the membrane at the end of the slider, and to determine the quantity of this heat which crosses the cube to the opposite face in a minute of time. For the solution of this problem two things are required:—first, the source of heat to be applied to the left hand of the face of the cube, and secondly, a means of measuring the amount which has made its appearance at the opposite face at the expiration of a minute.

To obtain a source of heat of the nature described the following method was adopted:—B is a small galvanic battery, from the negative pole of which a current proceeds to the galvanometer of tangents T; passes round the ring of the instrument, deflecting, in its passage, the magnetic needle which hangs in the centre of the ring. The strength of the current is, as is known, proportional to the tangent of the angle of permanent deflection. From T the current proceeds to the rheostat R; this instrument consists of a cylinder of serpentine stone, round which a German silver wire is coiled spirally; by turning the handle of the instrument any required quantity of this powerfully resisting wire is thrown into the circuit, the current being thus regulated at pleasure. The sole use of these two last instruments in the present series of experiments is to keep the current perfectly constant from day to day. From the rheostat the current proceeds to the cistern c, thence through the bent wire, and back to the cistern d, from which it proceeds to the other pole of the battery.

The bent wire, during the passage of the current, becomes heated; this heat is transmitted through the mercury in the chamber m' to the membrane in front of the chamber; this mem-

* Annales de Chimie et de Physique, March 1828.
brane becomes the proximate source of heat which is applied to the left-hand face of the cube. The quantity transmitted from this source, through the mass of the cube, to the opposite face, in any given time, will, of course, depend on the conductivity of the latter, and its amount may be estimated from the deflection which it is able to produce upon the needle of a galvanometer connected with the bismuth and antimony pair. G is a galvanometer used for this purpose; from it proceed wires to the mercury cups M M', which, as before remarked, are connected by platinum wires with Λ and Β. The galvanometer is a carefully constructed and delicate instrument from the workshop of that skilful mechanic, Kleiner, in Berlin.

The sole use of the mercury in the chambers m and m' is to secure good and equable contact; when the chambers are filled with pure mercury, and this is allowed to remain in them throughout an entire series of experiments, it is certain that the conditions of contact are perfectly constant, and thus the most fruitful source of doubt and error is effectually excluded. In rough experiments the chambers might be dispensed with, and the bent wire itself might be brought into contact with one face of the cube, while the other face might immediately press against the bismuth and antimony. The result however of many hundreds of experiments made with the instrument in this state, has been to prove the impossibility of preserving the conditions of experiment constant, and to compel me to devise some means of avoiding the irregularities which exhibited themselves. The instrument just described meets the requirements of the case; care is necessary in the use of it, but when care is taken, an accuracy is attainable by it which, I believe, has been hitherto unequalled.

The action of mercury upon bismuth, as a solvent, is well known; an amalgam is speedily formed where the two metals come into contact. To preserve our thermo-electric couple from this action, their ends are protected by a sheathing of the same membrane as that used in front of the chambers m m'.

Previous to the cube being placed between the two membranes, the latter, by virtue of the fluid masses behind them, bulge out a little, thus forming a pair of soft and slightly convex cushions. When the cube is placed upon its supports and the slider is brought up against it, both cushions are pressed flat, and thus perfect contact is secured. The surface of the cube is larger than the surface of the membrane in contact with it*; and thus the former is always firmly caught between the opposed rigid projections, the slider being held fast in this position by means of the spring r, which is then attached to the pin p. The exact manner of experiment is as follows:—Having first seen that the

* The edge of each cube measured 0·3 of an inch.
At the junction, the break-circuit needle at \( h \), circuit \( k \), and the current is permitted to circulate for sixty seconds; at the sixty-sixth second the voltaic circuit is broken by the left hand at \( k \), while almost at the same instant the thermo-circuit is closed by the right hand at \( k' \). The needle of the galvanometer is instantly deflected, and the limit of the first impulsion is noted; the amount of this impulsion depends, of course, upon the quantity of heat which has reached the bismuth and antimony junction through the mass of the cube during the time of action, and consequently upon the conductive power of the latter. The limit of the first impulsion being noted, the cube is instantly removed, and the instrument is allowed to cool until the needle of the galvanometer returns to zero. To expedite the cooling, the metallic surfaces of \( A \) and \( B \) are to a great extent exposed; the wood is cut away all round them, and from the space between them; they do not rest upon the wood, their sole points of support being the ivory \( i \) at one end and the jaws \( II' \) at the other. The cube, as before explained, does not touch the brass plate underneath it, but is supported on its four conical points, and the plate which bears these is itself as much as possible cut away to permit of a free circulation of air through the space \( efgh \). Time is a precious commodity to the experimenter, and by the means described the cooling is hastened and the experiments can succeed each other more quickly. To hasten the cooling further I made use of a pair of small bellows during the first minute after the removal of each cube, and, afterwards, a plate of thin glass was placed over the junction, but not in contact with it. On the glass two drops of \( æther \) were suffered to fall from a pipette; its evaporation caused a refrigeration of the air under-neath, which, in virtue of its increased density, sank and diffused itself around the place of junction. In this way the temperature at the junction was brought a little lower than that of the surrounding air; the needle of the galvanometer being thus brought back, not only to zero, but to a certain point at the other side of it; at this point the glass was removed and a new cube was introduced; the thermo-circuit was permitted to remain closed until the needle descended to zero, which it slowly did, when the cause of local cooling was removed; the thermo-circuit was then broken at \( k' \), and things stood as at the commencement of the former experiment. The voltaic circuit was once more closed, the current permitted to circulate sixty seconds, then interrupted by the left hand, the thermo-circuit being closed at the same moment with the right, and the first impulsion measured as before.

When however these artificial means of cooling are adopted
great care is necessary. We must not use the bellows in some experiments and neglect the use of it in others; and if the ether be applied once, it must be applied throughout the entire series of experiments. It must continue to act for the same time, and the same quantity should be applied in all cases. Of course such precautions are only necessary when great accuracy is required, but here they are absolutely necessary. Judging from the description, the mode of experiment may appear complicated, but in reality it is not so. A single experimenter has the most complete command over the entire arrangement. The wires from the small galvanic battery (four of Bunsen's cells) remain undisturbed from day to day; all that is to be done is to connect the battery with them, and every thing is ready for experiment.

There are in wood three lines at right angles to each other, which the mere inspection of the substance enables us to fix upon as the necessary resultants of molecular action: the first line is parallel to the fibre; the second is perpendicular to the fibre, and to the ligneous layers which indicate the annual growth of the tree; while the third is perpendicular to the fibre and parallel, or rather tangent to the layers. From each of a number of trees a cube was cut, so that every two opposite faces were parallel to one of the above lines. Thus, two faces were parallel to the ligneous layers, two perpendicular to them, while the remaining two were perpendicular to the fibre. It was proposed to examine the velocity of calorific transmission through the mass in these three directions. It may be remarked that the cubes were fair average specimens of the woods, and were in all cases well-seasoned and dry.

The cube was first placed upon its four supports abed, so that the line of flux from m' to m was parallel to the fibre, and the deflection produced by the heat transmitted in sixty seconds was observed. The position of the cube was then changed so that its fibre stood vertical, the line of flux from m' to m being perpendicular to the fibre and parallel to the ligneous layers; the deflection produced by a minute's action in this case was also determined. Finally, the cube was turned 90° round, its fibre being still vertical, so that the line of flux was perpendicular to both fibre and layers, and the consequent deflection was observed. In the comparison of these two latter directions the chief delicacy of manipulation is necessary. It requires but a rough experiment to demonstrate the superior velocity of propagation along the fibre, but the velocities in all directions perpendicular to the fibre are so nearly equal that it is only by the greatest care and, in the majority of cases, by numerous experiments, that a differential action can be securely established.

The following table contains the results of the inquiry; it will explain itself.
Table I.—Strength of Current used to heat the bent Wire, as measured by the tangent galvanometer:—constantly 35°.

Deflections, the line of flux being of:—

<table>
<thead>
<tr>
<th>Description of wood.</th>
<th>Parallel to fibre of wood.</th>
<th>Perpendicular to fibre and to ligamentous layers.</th>
<th>III.</th>
<th>Parallel to fibre and to ligamentous layers.</th>
<th>Description of wood.</th>
<th>Parallel to fibre of wood.</th>
<th>Perpendicular to fibre and to ligamentous layers.</th>
<th>III.</th>
</tr>
</thead>
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<tr>
<td>American Birch .....</td>
<td>33</td>
<td>9</td>
<td>11</td>
<td>9</td>
<td>Box-wood ............</td>
<td>31</td>
<td>Mean 9:9</td>
<td>12:0</td>
</tr>
<tr>
<td>Oak .................</td>
<td>33</td>
<td>9:5</td>
<td>11</td>
<td>10:0</td>
<td>Tamarind-wood ........</td>
<td>31</td>
<td>Mean 11:1</td>
<td>12:1</td>
</tr>
<tr>
<td>Beech ..............</td>
<td>33 Mean 8:8</td>
<td>10:8</td>
<td></td>
<td>10:0</td>
<td>Rose-wood ............</td>
<td>31 Mean 10:4</td>
<td>12:6</td>
<td></td>
</tr>
<tr>
<td>Coromandel-wood. An exceedingly hard wood from Ceylon.</td>
<td>33 Mean 9:8</td>
<td>12:3</td>
<td></td>
<td>10:0</td>
<td>Mazatlan-wood ........</td>
<td>30 Mean 10:5</td>
<td>12:5</td>
<td></td>
</tr>
<tr>
<td>Quebec Pine ........</td>
<td>33 Mean 10:0</td>
<td>11:4</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Beef-wood. A red-coloured wood from New South Wales ........</td>
<td>33 Mean 9:5</td>
<td>10:5</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black Ebony ..........</td>
<td>32 Mean 9:5</td>
<td>10:5</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bird's-eye Maple ...</td>
<td>31 Mean 11:5</td>
<td>12:0</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lance-wood ..........</td>
<td>31 Mean 10:6</td>
<td>12:1</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Zebra-wood. The produce of the Brazils ...............</td>
<td>31 Mean 8:2</td>
<td>10:0</td>
<td></td>
<td>10:5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### Table I. (continued).

<table>
<thead>
<tr>
<th>Description of wood</th>
<th>I. Perpendicular to fibre of wood.</th>
<th>II. Perpendicular to fibre and to parallel parenchymous layers.</th>
<th>III. Perpendicular to fibre and to ligneous layers.</th>
<th>Description of wood</th>
<th>I. Perpendicular to fibre of wood.</th>
<th>II. Perpendicular to fibre and to parallel parenchymous layers.</th>
<th>III. Perpendicular to fibre and to ligneous layers.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Locust-wood. From North America</td>
<td>30 Mean 10.0</td>
<td>10.0</td>
<td>10.0</td>
<td>10.0</td>
<td>10.0</td>
<td>10.0</td>
<td>Botany Bay Oak. Does not belong to the same genus as the European.</td>
</tr>
<tr>
<td>Ruby-wood. From Calcutta...</td>
<td>30 Mean 10.3</td>
<td>11.0</td>
<td>12.0</td>
<td>10.5</td>
<td>12.0</td>
<td>11.2</td>
<td>Cocoa-wood. From the West Indies</td>
</tr>
<tr>
<td>Peruvian-wood ...</td>
<td>30 Mean 10.7</td>
<td>10.0</td>
<td>11.0</td>
<td>10.5</td>
<td>11.0</td>
<td>10.0</td>
<td>Madagascar Red-wood</td>
</tr>
<tr>
<td>King-wood. Called also violet-wood, from the Brazils</td>
<td>30 Mean 10.3</td>
<td>11.0</td>
<td>12.0</td>
<td>10.5</td>
<td>11.0</td>
<td>10.0</td>
<td>Madagascar Red-wood</td>
</tr>
<tr>
<td>Bladder-nut-wood...</td>
<td>29 Mean 10.0</td>
<td>10.0</td>
<td>11.0</td>
<td>10.0</td>
<td>12.0</td>
<td>12.0</td>
<td>Sandal-wood. From Malabar...</td>
</tr>
<tr>
<td>Larch ...</td>
<td>29 Mean 11.1</td>
<td>12.0</td>
<td>13.0</td>
<td>11.5</td>
<td>12.0</td>
<td>12.0</td>
<td>Tulip-wood. From Brazil...</td>
</tr>
<tr>
<td>Princes-wood. From Jamaica...</td>
<td>29 Mean 11.4</td>
<td>13.0</td>
<td>12.0</td>
<td>11.5</td>
<td>12.0</td>
<td>11.5</td>
<td>Camphor-wood. From China...</td>
</tr>
<tr>
<td>Green Heart. From Jamaica ...</td>
<td>29 Mean 11.4</td>
<td>12.0</td>
<td>12.0</td>
<td>11.5</td>
<td>12.0</td>
<td>11.5</td>
<td>Camphor-wood. From China...</td>
</tr>
<tr>
<td>Walnut ...</td>
<td>28 Mean 10.5</td>
<td>11.0</td>
<td>13.0</td>
<td>10.0</td>
<td>12.0</td>
<td>12.0</td>
<td>Olive-wood. From Leghorn...</td>
</tr>
<tr>
<td>Drooping Ash ...</td>
<td>28 Mean 10.5</td>
<td>11.0</td>
<td>12.0</td>
<td>10.0</td>
<td>12.0</td>
<td>12.0</td>
<td>Olive-wood. From Leghorn...</td>
</tr>
<tr>
<td>Gaffe Deal ...</td>
<td>27 Mean 10.5</td>
<td>11.0</td>
<td>12.0</td>
<td>10.0</td>
<td>12.0</td>
<td>12.0</td>
<td>Olive-wood. From Leghorn...</td>
</tr>
<tr>
<td>Ash ...</td>
<td>27 Mean 9.5</td>
<td>11.0</td>
<td>12.0</td>
<td>9.0</td>
<td>12.0</td>
<td>12.0</td>
<td>Olive-wood. From Leghorn...</td>
</tr>
</tbody>
</table>
To enable the eye to detect at once the law of action established by the experiments, we will present the results in a more condensed form.
Dr. Tyndall on Molecular Influences.

Deflections.

<table>
<thead>
<tr>
<th>Description of wood</th>
<th>I. Parallel to fibre</th>
<th>II. Perpendicular to ligno-cousus layers</th>
<th>III. Perpendicular to fibrous layers</th>
<th>Description of wood</th>
<th>I. Parallel to fibre</th>
<th>II. Perpendicular to ligno-cousus layers</th>
<th>III. Perpendicular to fibrous layers</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 American Birch</td>
<td>35</td>
<td>9-0</td>
<td>11-0</td>
<td>28 Botany Bay Oak</td>
<td>28</td>
<td>9-9</td>
<td>12-4</td>
</tr>
<tr>
<td>2 Oak</td>
<td>34</td>
<td>9-5</td>
<td>11-0</td>
<td>29 Cocoa-wood</td>
<td>28</td>
<td>11-9</td>
<td>13-6</td>
</tr>
<tr>
<td>3 Beech</td>
<td>33</td>
<td>8-8</td>
<td>10-8</td>
<td>30 Madagascar Red-wood</td>
<td>28</td>
<td>10-7</td>
<td>11-3</td>
</tr>
<tr>
<td>4 Coromandel-wood</td>
<td>33</td>
<td>9-8</td>
<td>11-0</td>
<td>32 Tulip-wood</td>
<td>28</td>
<td>11-0</td>
<td>12-1</td>
</tr>
<tr>
<td>5 Quebec Pine</td>
<td>33</td>
<td>10-0</td>
<td>11-4</td>
<td>33 Camphor-wood</td>
<td>28</td>
<td>8-6</td>
<td>10-0</td>
</tr>
<tr>
<td>7 Black Ebony</td>
<td>32</td>
<td>10-5</td>
<td>12-0</td>
<td>35 Gaffle Deal</td>
<td>27</td>
<td>10-0</td>
<td>11-0</td>
</tr>
<tr>
<td>8 Bird’s-Eye Maple</td>
<td>31</td>
<td>11-0</td>
<td>12-1</td>
<td>36 Ash</td>
<td>27</td>
<td>9-5</td>
<td>11-5</td>
</tr>
<tr>
<td>9 LANCE-wood</td>
<td>31</td>
<td>10-6</td>
<td>12-1</td>
<td>37 Green Ebony</td>
<td>27</td>
<td>10-5</td>
<td>12-2</td>
</tr>
<tr>
<td>10 Zebra-wood</td>
<td>31</td>
<td>8-2</td>
<td>10-0</td>
<td>38 Black Oak</td>
<td>27</td>
<td>8-0</td>
<td>9-4</td>
</tr>
<tr>
<td>11 Box-wood</td>
<td>31</td>
<td>9-9</td>
<td>12-0</td>
<td>39 Apple-tree</td>
<td>26</td>
<td>10-0</td>
<td>12-5</td>
</tr>
<tr>
<td>12 TAMARIND-wood</td>
<td>31</td>
<td>11-1</td>
<td>12-1</td>
<td>40 Cam-wood</td>
<td>26</td>
<td>13-4</td>
<td>15-0</td>
</tr>
<tr>
<td>13 Teak-wood</td>
<td>31</td>
<td>9-9</td>
<td>12-4</td>
<td>41 Iron-wood</td>
<td>26</td>
<td>10-2</td>
<td>12-4</td>
</tr>
<tr>
<td>14 Rose-wood</td>
<td>31</td>
<td>10-4</td>
<td>12-6</td>
<td>42 Chestnut</td>
<td>26</td>
<td>10-1</td>
<td>11-5</td>
</tr>
<tr>
<td>15 Mazatlan-wood</td>
<td>30</td>
<td>10-5</td>
<td>12-5</td>
<td>43 Sycamore</td>
<td>26</td>
<td>10-6</td>
<td>12-2</td>
</tr>
<tr>
<td>16 Satin-wood</td>
<td>30</td>
<td>11-9</td>
<td>12-3</td>
<td>44 Spruce Fir</td>
<td>25</td>
<td>11-8</td>
<td>12-5</td>
</tr>
<tr>
<td>17 Brazilletto</td>
<td>30</td>
<td>9-2</td>
<td>11-0</td>
<td>45 Honduras Mahogany</td>
<td>25</td>
<td>9-0</td>
<td>10-0</td>
</tr>
<tr>
<td>18 Locust-wood</td>
<td>30</td>
<td>10-0</td>
<td>11-0</td>
<td>46 Brazil-wood</td>
<td>25</td>
<td>11-9</td>
<td>13-9</td>
</tr>
<tr>
<td>19 Ruby-wood</td>
<td>30</td>
<td>10-3</td>
<td>11-2</td>
<td>47 Yew</td>
<td>24</td>
<td>11-0</td>
<td>12-0</td>
</tr>
<tr>
<td>20 Peruvian-wood</td>
<td>30</td>
<td>10-7</td>
<td>11-7</td>
<td>48 Elm</td>
<td>24</td>
<td>10-0</td>
<td>11-5</td>
</tr>
<tr>
<td>21 King-wood</td>
<td>30</td>
<td>10-3</td>
<td>11-7</td>
<td>49 Plane-tree</td>
<td>24</td>
<td>10-0</td>
<td>12-0</td>
</tr>
<tr>
<td>22 Bladder-nut-wood</td>
<td>29</td>
<td>10-0</td>
<td>12-0</td>
<td>50 Portugal Laurel</td>
<td>24</td>
<td>10-0</td>
<td>11-5</td>
</tr>
<tr>
<td>23 Larch</td>
<td>29</td>
<td>10-0</td>
<td>11-0</td>
<td>51 Bullet-wood</td>
<td>24</td>
<td>10-0</td>
<td>11-7</td>
</tr>
<tr>
<td>24 Princes-wood</td>
<td>29</td>
<td>11-1</td>
<td>13-1</td>
<td>52 Spanish Mahogany</td>
<td>23</td>
<td>11-5</td>
<td>12-5</td>
</tr>
<tr>
<td>25 Green-heart</td>
<td>29</td>
<td>11-4</td>
<td>12-6</td>
<td>53 Scotch Fir</td>
<td>22</td>
<td>10-0</td>
<td>12-0</td>
</tr>
<tr>
<td>26 Walnut</td>
<td>28</td>
<td>11-0</td>
<td>12-0</td>
<td>54 Laurel</td>
<td>22</td>
<td>12-0</td>
<td>15-0</td>
</tr>
<tr>
<td>27 Drooping Ash</td>
<td>28</td>
<td>11-0</td>
<td>12-0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The above table furnishes us with the fullest corroboration of the result arrived at by De la Rive and DeCandolle, regarding the superior conductivity of the wood in the direction of the fibre. Evidence is also afforded as to how little mere density affects the velocity of transmission. There appears to be neither law nor general rule here. American Birch, a comparatively light wood, possesses undoubtedly a higher transmissive power than any other in the list—a result which has been established by numerous experiments, although but one appears cited in the table. Iron-wood, on the contrary, with a specific gravity of 1.426, stands low. Again, Oak and Coromandel-wood,—the latter so hard and dense that it is used for sharp war instruments by savage tribes,—stand near the head of the list, while Scotch Fir and other light wood stand low.
We further find that the lateral conductivity bears no definite ratio to the longitudinal conductivity. Indeed the tendency appears to be that those woods which possess the lowest power of transmission, along the fibre, possess the highest power across it. But here the exceptions are so numerous that we have no warranty for a general conclusion.

But the most remarkable result of the experiments remains yet to be stated. If we cast our eyes along the second and third columns of the tabular summary, we shall find that in every instance the velocity of propagation is greatest in a direction perpendicular to the ligneous layers. This result is, of course, wholly independent of the specific heat of the wood, inasmuch as it is two directions through the self-same cube which are here compared with each other. The law of molecular action, as regards the transmission of heat through wood, may therefore be expressed as follows:—

At all the points not situate in the centre of the tree, wood possesses three unequal axes of calorific conduction, which are at right angles to each other. The first, and principal axis, is parallel to the fibre of the wood; the second, and intermediate axis, is perpendicular to the fibre and to the ligneous layers; while the third and least axis is perpendicular to the fibre and parallel to the layers.

The researches of Savart on the sonorous vibrations of wood naturally suggest themselves here; for, doubtless, the same molecular structure which imparts to this substance the peculiar elastic properties discovered by Savart, must be regarded as the cause of the differential action established above. Savart* took bars of equal size, and in different directions, from a mass of wood; determining their resistance to flexure from the number of vibrations carried out by each in a certain time, he found that wood possessed three axes of elasticity. These axes coincide with the axes of calorific conduction established by the foregoing experiments. The axis of greatest elasticity coincides with that of highest conductive capacity, and the axis of least elasticity with that of lowest conductive capacity.

A few exact experiments made with a view to ascertain the influence of molecular structure upon the cleavage of wood would have formed an interesting addition to this communication; fortunately, however, the mere sense of touch, to fingers accustomed to seek for the cleavages of crystals, affords sufficient evidence here. If a piece of wood be taken, on which the rings which mark the growth of the tree plainly appear, and a penknife or a chisel be laid across the rings, it will be found that the pressure necessary to cleave the wood is less in this than in any other

* Taylor's Scientific Memoirs, vol. i. p. 139.
direction. The cohesion in the direction parallel to the layers and perpendicular to the fibre is therefore a minimum. In the same way, it will be found that of all lines perpendicular to the fibre the line of greatest cohesion is perpendicular to the ligneous layers; while the cohesion in the direction of the fibre is far greater than along either of the lines just referred to. Hence—

Wood possesses three axes of cohesion which coincide with the axes of calorific conduction—the greatest with the greatest, and the least with the least.

It would have also added interest to the inquiry to have examined the permeability of wood to fluids in various directions. Here, again, however, the experimental knowledge already amassed by housewives and cask-makers comes to our aid. It is well known that fluids would ooze with facility through a stave cut perpendicular to the fibre; a wooden plate, for instance, cut perpendicular to the axis of a tree would be totally unfit for the bottom of a vessel destined to hold spirits, water, or brine. Further precautions, however, must be taken in choosing staves for casks. If the surface of the stave be parallel to the ligneous layers, the liquid, though with greater difficulty than in the case just mentioned, will still make its way through. The stave must be cut perpendicular to the layers; for, in crossing such a stave, the resistance offered to the passage of the fluid is a maximum. Hence—

Wood possesses three axes of fluid permeability which coincide with those of calorific conduction,—the greatest with the greatest, and the least with the least.

To sum up:—In this single substance we have pointed out the existence of three new systems of axis; the axes of calorific conduction, of cohesion, and of fluid permeability; all of which coincide with a fourth system of axes of elasticity discovered by Savart. The experiments also furnish an illustration of the theory of Professor Stokes, who proves that the flux of heat through any body may be referred to three rectangular axes, which he calls the thermic axes of the body*.

MM. De la Rive and DeCandolle have remarked upon the influence which its feeble conducting power in a lateral direction must exert in preserving within a tree the warmth which it acquires from the soil. In virtue of this property a tree is able to resist sudden changes of temperature which would probably be prejudicial to it; it resists alike the sudden abstraction of heat

* Cambridge and Dublin Mathematical Journal, November 1851.
from within and the sudden accession of it from without. But
nature has gone further, and clothes the tree with a sheathing
of worse-conducting material than the wood itself, even in its
worst direction. The following are the deflections obtained by
submitting a number of cubes of bark of the same size as the
cubes of wood to the same conditions of experiment:

<table>
<thead>
<tr>
<th></th>
<th>Deflection</th>
<th>Corresponding deflection produced by the wood.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Beech-tree Bark</td>
<td>7</td>
<td>10.8</td>
</tr>
<tr>
<td>Oak-tree Bark</td>
<td>7</td>
<td>11.0</td>
</tr>
<tr>
<td>Elm-tree Bark</td>
<td>7</td>
<td>11.5</td>
</tr>
<tr>
<td>Pine-tree Bark</td>
<td>7</td>
<td>12.0</td>
</tr>
</tbody>
</table>

The direction of transmission, in these cases, was from the
interior surface of the bark outwards.

The average deflection produced by a cube of wood, when the
flux is lateral, may be taken at

\[ 12^\circ \]

a cube of rock-crystal (pure silica) of the same size produces a
deflection of

\[ 90^\circ \]

This single experiment is sufficient to show how different must
be the meteorological effects of these two substances, when they
exist in sufficient quantity to exercise an influence upon climate.
Among the more prominent influences here, Humboldt mentions
the nature of the soil and of vegetation. The general influence
of an arid and exposed soil has been long known, but the part
played by this substance, silica, has hitherto had no particular
importance attached to it. Were gypsum, however, instead of
silica, the prevalent mineral in Sahara, a very different state of
things from the present would assuredly exist. A cube of the
latter substance examined in the usual manner produces a de-
flection of

\[ 19^\circ \]

only. It is scarcely superior to wood, while there is the strongest
experimental grounds for the belief that silica possesses a higher
conductive power than some of the metals. These grounds shall
be adduced in a future paper.

Let us consider, for a moment, the process which takes place
from sunrise to the hour of maximum temperature in a region
overspread with forests, and compare it with that which must
take place in the African Desert. In the former case, the heat
slowly and with difficulty penetrates the masses of wood and
leaves on which it falls, and after the point of maximum tem-
perature is passed, the yielding up of the heat acquired is pro-
portionately slow. In the desert, however, the mass of silica
exposed to the sun becomes burning hot as the hour of maximum temperature approaches; but, after this is passed, the heat is yielded up with proportionate facility. Hence a maximum and minimum thermometer must, in the latter case, mark a far wider range of temperature during the twenty-four hours than in the former. This agrees with observation. In Sahara, to use the words of Mrs. Somerville, during "the glare of noon the air quivers with the heat reflected from the red sand, and in the night it is chilled under a clear sky sparkling with its host of stars*." Were gypsum, however, the prevailing mineral, it is à priori certain that this could not be the case to anything like its present extent.

The following experiments furnish some notion of the transmissive power of a few other organic structures; cubes of the substances were examined in the usual manner.

<table>
<thead>
<tr>
<th>Substance</th>
<th>Temperature</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tooth of Walrus</td>
<td>16</td>
</tr>
<tr>
<td>Tusk of East Indian Elephant</td>
<td>17</td>
</tr>
<tr>
<td>Whalebone</td>
<td>9</td>
</tr>
<tr>
<td>Rhinoceros's-horn</td>
<td>9</td>
</tr>
<tr>
<td>Cow's-horn</td>
<td>9</td>
</tr>
</tbody>
</table>

Considering the density and elasticity of ivory, we might be disposed to attribute to it a comparatively high conductive power; but the experiment proves it to be a very bad conductor—much inferior, indeed, to wood in the direction of the fibre. Doubtless this conduces to the animal's comfort. Exposed to the rays of a tropical sun, if these huge bony masses were capable of assuming a high temperature during the day and losing it again at night, it must be a source of the greatest inconvenience to the animal, as at present constituted. The horns of the Rhinoceros and Cow, however, still more strikingly exemplify that fitness of parts which is perpetually presented to the student of natural science. In the latter case especially, the mass of horn in close contact with the skull, and therefore capable of transmitting heat directly to the animal's brain, must be attended with very unpleasant consequences, if horn were a good conductor. Given such a constitution, the substance fixed upon by our own enlightened intellect to furnish the animal with such weapons of defence, would be just such as nature has chosen.

As a general rule, sudden changes of temperature are prejudicial to animal and vegetable health; the substances used in the construction of organic tissues are exactly such as are best calculated to resist those changes. Coal enters largely into the composition of such tissues, and it is an exceedingly bad con-

* Physical Geography, vol. i. p. 147.
ductor. Here are the deflections obtained with three different descriptions of this substance:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Deflection</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sunderland coal</td>
<td>8</td>
</tr>
<tr>
<td>Boghead cannel</td>
<td>8</td>
</tr>
<tr>
<td>Lesmahago cannel</td>
<td>8</td>
</tr>
</tbody>
</table>

The following results illustrate the subject in a still more striking manner. It is almost needless to remark that each of the substances mentioned was reduced to the cubical form, and submitted to an examination similar in every respect to that of wood and quartz. While, however, a cube of the latter substance produces a deflection of $90^\circ$, a cube of

<table>
<thead>
<tr>
<th>Substance</th>
<th>Deflection</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sealing-wax</td>
<td>0</td>
</tr>
<tr>
<td>Sole leather</td>
<td>0</td>
</tr>
<tr>
<td>Bees'-wax</td>
<td>0</td>
</tr>
<tr>
<td>Glue</td>
<td>0</td>
</tr>
<tr>
<td>Gutta-percha</td>
<td>0</td>
</tr>
<tr>
<td>India-rubber</td>
<td>0</td>
</tr>
<tr>
<td>Filbert-kernel</td>
<td>0</td>
</tr>
<tr>
<td>Almond-kernel</td>
<td>0</td>
</tr>
<tr>
<td>Boiled ham-muscle</td>
<td>0</td>
</tr>
<tr>
<td>Raw veal-muscle</td>
<td>0</td>
</tr>
</tbody>
</table>

The substances here named are all of them animal and vegetable productions; and the experiments demonstrate the extreme imperviousness of every one of them. Starting from the principle that sudden accessions or deprivations of heat are prejudicial to animal and vegetable health, we see that the materials chosen are precisely those which are best calculated to avert such changes. It is yet to be estimated what influence the extreme non-conductibility of muscular tissue exerts in producing the remarkable constancy of temperature observed in the human body in different climates. The cuticle is an exceedingly bad conductor, and this explains the insensitivity to heat of hands on which the skin has been thickened by exposure. Probably many escapes from the fiery ordeal, which have been hitherto referred to collusion, might be scientifically explained by reference to this fact. While studying at Marburg, I have sometimes heard Professor Bunsen make a good-humoured remark on the tenderness of his pupils' fingers. Accustomed as he was to the manipulation of the glass used in his admirable eudiometrical researches, his fingers had acquired an insensitivity to heat sufficient to carry him safely through an ordeal which, in other cases, would undoubtedly invoke the judicial condemnation of the middle ages. The ex-
Mr. J. J. Sylvester on the new Rule for finding Superior and Inferior Limits to the real Roots of any Algebraical Equation. * By J. J. Sylvester, F.R.S.*

The lemma accessory to the demonstration of the rule for finding limits to the roots of an equation, given in the addendum to my paper in the Magazine for this month, admits of two successive and large steps of generalization, in which the scope of the principal theorem will participate in an equal degree.

1. Whatever the signs may be of \( q_1, q_2, q_3 \ldots q_r \), the denominator of the continued fraction

\[
\frac{1}{q_1 + \frac{1}{q_2 + \frac{1}{q_3 + \ddots + \frac{1}{q_r}}}}
\]

will have the same sign as \( q_1 \cdot q_2 \cdot q_3 \ldots q_r \), provided that

\[
[q_1] > \mu_1[q_2] > \mu_2 + \frac{1}{\mu_1[q_3]} > \mu_3 + \frac{1}{\mu_2} \ldots
\]

\[
[q_{r-1}] > \mu_{r-1} + \frac{1}{\mu_{r-2}}[q_r] > \frac{1}{\mu_{r-1}},
\]

where \( \mu_1, \mu_2, \ldots \mu_{r-1} \) signify any positive quantities whatsoever; in the particular case where \( \mu_1 = \mu_2 = \mu_3 = \ldots = \mu_{r-1} = 1 \), we fall back upon the lemma as originally stated.

But 2nd. The lemma admits of another modification, which will in general impose far less stringent limits upon the arithmetical values of the series of \( q \)'s.

Let all the possible sequences of \( q \)'s be taken which present

* Communicated by the Author.
only variations of sign; e. g. if the entire series be \( q_1, q_2, q_3, q_4 \) and the corresponding algebraical signs are \(+ -- +\), we shall have the two sequences \( q_1, q_2; q_3, q_4 \). If the entire series be \( q_1, q_2, q_3 \ldots q_{15} \), and the signs be \(- -- + -- + -- + -- + -- + --\), then the sequences to be taken will be

\[ q_1 q_4 q_5 q_6; q_9 q_10 q_11; q_{14} q_{15} \]

and so in general.

Suppose, now, that \( q_{\rho+1}, q_{\rho+2}, \ldots q_{\rho+i} \) are the terms of any one such sequence. Then, provided that

\[ [q_{\rho+1}] > \mu_1; [q_{\rho+2}] > \mu_2 + \frac{1}{\mu_1} \ldots [q_{\rho+i-1}] > \mu_{i-1} + \frac{1}{\mu_{i-2}} \]

and \( q_{\rho+i} > \frac{1}{\mu_{i-1}} \),

(it being understood that the values of \( \mu_1, \mu_2, \ldots \mu_{i-1} \) are perfectly arbitrary, except being subject to the condition of being all positive, and that there are as many distinct and independent systems of such values as there are sequences of variations of sign), it will continue to be true (and capable of being demonstrated to be so by precisely the same reasoning as was applied to the demonstration of the lemma in its original form) that the denominator of \( \frac{1}{q_1 + \frac{1}{q_2 + \frac{1}{q_3 + \ldots + \frac{1}{q_r}}}} \) will have the same sign as the product \( q_1 \cdot q_2 \cdot q_3 \ldots q_r \). It will be observed that, as regards the residual quotients not comprised in any sequence, their values are absolutely unaffected by any condition whatever. As a direct consequence from this lemma, we derive the following greatly improved *Theorem* for the discovery of the limits.

Let, as before, \( f(x) = 0 \) be any given algebraical equation; \( \phi(x) \) any assumed arbitrary function of \( (x) \) of an inferior degree to that of \( f(x) \) and let

\[ \phi(x) = \frac{1}{X_1 + \frac{1}{X_2 + \frac{1}{X_3 + \ldots + \frac{1}{X_r}}}} \]

and let the leading coefficients of \( X_1, X_2, X_3, \ldots X_r \) be \( q_1, q_2, q_3, \ldots q_r \), and let this latter series be divided into sequences of variations and residual terms not comprised in any such sequence, as explained above. Let the \( X \)'s corresponding to the residual terms be called

\[ P_1, P_2, \ldots P_\omega, \]

and let the successive sets of \( X \)'s corresponding to the sequences be called respectively

\[ V_1, V_2, \ldots V_\rho \]

\[ V'_1, V'_2, \ldots V'_p \]

\[ V''_1, V''_2, \ldots V''_p' \]

\[ \ldots \]

\[ (V_1), (V_2), \ldots (V_\rho). \]
And let
\[ X = P_1 \cdot P_2 \cdots P_\omega \]
\[ \times (V_1^2 - c_1^2) \cdot (V_2^2 - c_2^2) \cdots (V_\rho^2 - c_\rho^2) \]
\[ \times (V_1^{i_2} - c_1^{i_2}) \cdot (V_2^{i_2} - c_2^{i_2}) \cdots (V_\rho^{i_2} - c_\rho^{i_2}) \]
\&c. \quad \&c.
\[ \times \left( (V_1^{2^2} - (c_1)^2) \cdot (V_2^{2^2} - (c_2)^2) \cdots (V_\rho^{2^2} - (c_\rho)^2) \right), \]
where, in general, any system of values
\[ c_1, c_2, c_3, \cdots c_{\rho-1}, c_\rho \]
represents
\[ \mu_1, \frac{1}{\mu_1}, \cdots \mu_{\rho-1} + \frac{1}{\mu_{\rho-2}}, \frac{1}{\mu_{\rho-1}}. \]
Then the largest root of \( X = 0 \) is a superior limit, and the smallest root of \( X = 0 \) is an inferior limit to the real roots of \( fX = 0 \); and if \( X = 0 \) has no real roots, neither will \( fX = 0 \) have any. For the complete demonstration and some further developments of this theorem see the forthcoming number of Terrquem's Nouvelles Annales for the present month.

Café Militaire, Clermont Ferrand, July 15, 1853.

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**XX. Proceedings of Learned Societies.**

ROYAL SOCIETY.

[Continued from p. 69.]

April 28, A PAPER was read, entitled "On the Application of 1853. the Law of the Conservation of Energy to the Determination of the Magnetic Meridian on board Ship, when out of reach or out of sight of Land." By W. J. Macquorn Rankine.

The author states that, assuming that when a ship is swung completely round, so that her head bears exactly as it did at first, the magnetism of the ship, and that of the compass-needle return to their original condition, the following theorem is necessarily true:—

*The mechanical power developed by the mutual action of the ship and of the compass-needle during a complete revolution of the ship, is equal to zero.*

If \( \xi' \) be the apparent magnetic azimuth of the ship's head, east of north; \( \alpha' \) the corresponding apparent magnetic azimuth of a distant fixed terrestrial object (or where no such object is visible, of a star, corrected by calculation for its apparent diurnal motion); \( \alpha \) the true magnetic azimuth of the same object, so that \( \alpha - \alpha' \) is the westerly deviation of the compass-needle; then the above theorem is expressed symbolically thus:—

\[ 0 = \int_0^{2\pi} \sin(\alpha - \alpha') \cdot d\xi' = \sin \alpha \int_0^{2\pi} \cos \alpha' \cdot d\xi' - \cos \alpha \int_0^{2\pi} \sin \alpha' \cdot d\xi'. \]
from which it follows that

\[ \tan \alpha = \frac{\int_{0}^{2\pi} \sin \alpha' \, d\alpha'}{\int_{0}^{2\pi} \cos \alpha' \, d\alpha'} \]

The author remarks, that for the integrals in this formula are to be substituted, in practice, the algebraical sums of the sines and cosines respectively, of the apparent magnetic bearings of the distant object, observed with the ship's head successively on the sixteen principal points of the compass (or on eight principal points, as the case may be). He considers that this method may prove useful in magnetic surveys of the ocean.

Additional remarks to the foregoing paper.

In consequence of a suggestion of Professor William Thomson, the author here investigates the modifications required in the formulae of the previous part of his paper, when the compass-needle produces by induction a sensible effect on the mutual magnetic action of the earth and the ship.

Let \( A \), as in Mr. Archibald Smith's formulae, represent the mean of the sines of the deviations of the compass-needle observed during a complete revolution of the ship. As there is reason to believe that this quantity does not vary for a given ship in different parts of the earth so long as the quantity and distribution of her iron are unchanged, it may be determined, once for all, while in port, in the usual way.

When the ship is out of reach of land, let \( s \) be the mean of the sines, and \( c \) the mean of the cosines, of the apparent magnetic azimuths of a distant object observed during a complete revolution of the ship. Then the sine of the true magnetic azimuth of the object is given by the formula

\[ \sin \alpha = \frac{s \sqrt{(c^2 + s^2) - A^2}}{c^2 + s^2} - cA. \]

When \( A = 0 \), this formula becomes

\[ \tan \alpha = \frac{s}{c}, \]

being identical with that of the previous part of this paper.

May 12.—A paper was read, entitled "A Few Remarks on Currents in the Arctic Seas." By P. C. Sutherland, M.D.

The author states that, during a voyage lately made in the Arctic seas, his attention was arrested by the power exerted by refrigeration and congelation in separating from water any saline ingredients it may contain, and of thus causing disturbances in the mean density of the waters of the ocean, which, after being influenced by currents, can be overcome only by subsequent intermixtures with water from other localities where the disturbance in the equilibrium is of an opposite character. He considers that evaporation, which is so active within the tropical and temperate zones, obviously renders the sea more dense by depressing its surface, and thus gives rise to
the necessity for currents from the two poles of the earth, where deposition of vapour predominates to a considerable extent over evaporation. This he illustrates by referring to the constant current from the Atlantic into the Mediterranean, caused by the evaporation in this sea preponderating over the supply of fresh-water. He then points out the necessity also of a current out of this sea, in order that its waters, by the constant influx of saline matters, may not become a saturated solution of the salts of the ocean; and infers that counter-currents into the polar seas must also exist to obviate the contrary tendency which the waters of these seas have to become fresh. He calls attention to the importance of ascertaining the differences that occur in many parts of the surface of the ocean in respect to its saline contents, that we may be enabled to determine to what extent the currents and counter-currents may be influenced by the comparative freshness of the iced water of the northern and southern regions, and the necessary saltness of the equatorial and other overheated basins. On this point, with respect to the Arctic seas, he refers to observations by Dr. Scoresby, Sir Edward Parry, and those recorded in tables appended to this paper, which have been extracted from the Meteorological Journal kept in the North Atlantic and Davis's Straits during the late voyage in the Isabel.

The author next refers to the remarkable difference occurring in the climate of the east and west sides of Davis's Straits, that of the latter being much the colder. In the absence of thermometric registers for the west, to compare with those on the east side, he points out how the appearance of the land and development of plants and land animals on the two coasts enable us to determine which has the warmer climate. Looking from the top of Baffin's Bay, which commands a good view of both shores, the east side at the sea-coast has many portions of land free from snow, whereas the opposite, by its snowy and icy covering, presents an appearance altogether uncongenial. On the former are found a tolerably abundant flora, hares and deer; on the latter, there scarcely appears to be a spot to receive the roots of plants or the feet of these animals; and in the productions of the sea, both vegetable and animal, the same disproportion is met with. Upon the whole, he considers complete the analogy that exists between the North Atlantic and Davis's Straits, both with respect to the climate of their shores and to their inhabitants of the animal and vegetable kingdoms. With reference to the question how this analogy is brought about, the author considers it difficult to decide whether the increase in the temperature of the water and the consequent improvement of the climate, on the east side of the strait, arise from the disposition the ice has to leave the coast, by which means the water becomes exposed to the influence of the sun; or from currents of heated water from a more southern region. He further remarks that its density here cannot be restored, if once disturbed, without admixture with a large volume of water somewhat above the mean density.

Again referring to the observations of Sir Edward Parry and those recorded in the tables, the author remarks that from these it will be
seen that refrigeration has the effect of precipitating the salts of sea-water; and further, that it appears to him very probable that the temperature at which water begins to expand by the continued application of cold is that at which saline and earthy matter begins to be precipitated in solutions of the density of sea-water.

From the immense depth to which icebergs extend in Davis's Straits, and also from their vast number, the author infers that the temperature of the water will be kept pretty uniformly the same throughout a considerable part of its depth, rarely exceeding +32°, except at the surface, where the action of the sun comes into operation, in which case the water of greatest density from saline contents would always occupy the lowest position. In illustration of his views, he describes experiments on the freezing of sea-water of the density 1·025, in glass tubes; and from these he infers that, not only does congelation precipitate the saline matter in water, but refrigeration also at temperatures from 40° down to 32°. With reference to the influence of the density of the sea-water on currents, he remarks that after the warm season has fairly set in, in the Arctic seas, nothing is more common than to observe the surface-water, in hollowed out lanes or fissures of the land-ice, moving slowly towards the open water at the edge of the fixed ice; and this seaward motion is altogether independent of tidal motion or oceanic current, depending entirely upon the diminished density of the surface-water.

In conclusion, the author states that he does not know that we are yet in a position to demonstrate the actual existence of currents into the icy seas, as well as out of them; but that the necessity for them is obvious. It is not necessary, he remarks, that these currents, as in other parts, should occupy the surface, and probably also the bottom of one of the sides of the basins whose waters require to be renewed, as the Gulf-stream occupies the east side of the North Atlantic. It is plain that the cold and hot waters of two regions can be exchanged by the latter passing underneath the former; and although the arctic current from the Greenland sea does not contain much ice to the southward of Cape Farewell, it is more than probable its chilly waters pass over a fork of the Gulf-stream, which ultimately sweeps along the shores of West Greenland.

May 26.—The following communications were read:—

1. A letter from Mr. Joule to Colonel Sabine, Treas. V.P.R.S. &c.

Acton Square, Salford, May 23, 1853.

My dear Sir,—I notice in the Proceedings of the Royal Society for April 21, a letter from M. Regnault in which some experiments of my own are referred to in a manner which I feel does me injustice. M. Regnault says, "Le nombre trouvé par M. Joule pour la chaleur spécifique de l'air sous pression constante (0·226) est beaucoup trop faible. Celui qui résulte de mes expériences très nombreuses, et faites dans des circonstances variées, afin de reconnaître et d'éliminer les erreurs constantes, est 0·237."

Now, in my paper on the air-engine, Phil. Trans. 1852, part i. p. 74, I have given the results of three series of experiments, viz. 0·23008, 0·22674, and 0·2325, and remark, "The mean of the three
results is 0.22977, or nearly 0.23, which we may take as the specific heat of air at constant pressure determined by the above experiments."

I had been informed that M. Regnault was working on the specific heat of gases, and on that account did not feel it desirable to enter upon the laborious investigation which would have been requisite in order to add a couple of decimal figures to the number I had arrived at, and which was sufficient for the object I had in view, viz. to show that the discrepancy between the actual and theoretical velocity of sound arose from the incorrectness of Delaroche and Berard's determination of the specific heat of air (0.267), and not from any notable error in my number for the mechanical equivalent of the thermal unit. Having succeeded in doing this, I calculated the Tables 3 and 4 of my paper, using 0.238944 for the specific heat of air under constant pressure. I feel much gratified that the result arrived at by so eminent an experimentalist as M. Regnault confirms the accuracy in the main of the number I adopted.

I have only to add that Professor Thomson and myself, in pursuing our research on the thermal effects of rushing elastic fluids, are following up the views on the relation between mechanical and thermal phenomena originated by ourselves; and we shall feel most happy if M. Regnault's results, in the important line of investigation he has adopted, will facilitate our labour.

I have the honour to remain, dear Sir,

Yours most truly,

Colonel Sabine, &c. &c. &c.

J. P. Joule.

2. "Experimental Researches on Vegetation." By M. Georges Ville.

After stating that it has often been asked if air, and especially azote, contributes to the nutrition of plants; and, as regards the latter, that this question has always been answered negatively, the author remarks it is however known that plants do not draw all their azote from the soil, the crops produced every year in manured land giving a greater proportion of azote than is contained in the soil itself. The question which he has proposed to himself for solution is, whence then comes the excess of azote which the crops contain, and in a more general manner, the azote of plants, which the soil has not furnished? He divides his inquiry into the three following parts:

First. Inquiry into and determination of the proportion of the ammonia contained in the air of the atmosphere.

Second. Is the azote of the air absorbed by plants?

Third. Influence on vegetation of ammonia added to the air.

1. The author remarks that since the observation of M. Théodore de Saussure, that the air is mixed with ammoniacal vapours, three attempts have been made to determine the proportion of ammonia in the air: a million of kilogrammes of the air, according to M. Gräyer, contain 0.333 kil. N\text{3}H\text{4}; according to Mr. Kemp 3.880 kil.; according to M. Frésenius, of the air of the day, 0.098 kil., and of night air, 0.169 kil. He states that he has shown the cause of these discrepancies, and proved that the quantity of ammonia con-
tained in the air is 22.417 grms. for a million of kilogrammes of the air; and that the quantity oscillates between 17.14 grms. and 29.43 grms.

2. The author states that though the azote of the air is absorbed by plants, the ammonia of the air contributes nothing to this absorption. Not that ammonia is not an auxiliary of vegetation, but the air contains scarcely 0.000000224, and in this proportion its effects are inappreciable. These conclusions are founded upon a great number of experiments in which the plants lived at the expense of the air without deriving any thing from the soil. For the present he confines himself to laying down these two conclusions:—1. The azote of the air is absorbed by plants, by the cereals, as by all others. 2. The ammonia of the atmosphere performs no appreciable part in the life of plants, when vegetation takes place in a limited atmosphere. After describing the apparatus by means of which he carried on his experiments on the vegetation of plants placed in a soil deprived of organic matter, and the manner in which the experiments were conducted, he adduces the results of these experiments in proof of the above conclusions.

3. With reference to the influence of ammonia on vegetation, the author states that, if ammonia be added to the air, vegetation becomes remarkably active. In the proportion of 4 ten-thousandths the influence of this gas shows itself at the end of eight or ten days, and from this time it manifests itself with a continually increasing intensity. The leaves, which at first were of a pale-green, assume a deeper and deeper tint, and for a time become almost black; their petals are long and upright, and their surface wide and shining. In short, when vegetation has arrived at its proper period the crop is found far beyond that of the same plants grown in pure air; and, weight for weight, they contain twice as much azote. Besides these general effects there are others which are more variable, which depend upon particular conditions, but which are equally worthy of interest. In fact, by means of ammonia we can not only stimulate vegetation, but, further, we can modify its course, delay the action of certain functions, or enlarge the development and the modification of certain organs. The author further remarks, that if its use be ill-directed, it may cause accidents. Those which have occurred in the course of his experiments appear to him to throw an unexpected light upon the mechanism of the nutrition of plants. They have at least taught him at the expense of what care ammonia may become an auxiliary of vegetation. These experiments, which were made under the same conditions as those upon the absorption of azote, are then described, and their numerical results given.

To the conclusions already stated, the author adds that there are periods to be selected for the employment of ammonia, during which this gas produces different effects. If we commence its use when several months intervene before the flowering season of the plants, it produces no disturbance; they follow the ordinary course of their vegetation. If its use be commenced at the time of flowering, this function is stopped or delayed. The plant covers itself with leaves, and if the flowering takes place all the flowers are barren.

3. "An Account of Meteorological Observations in four Balloon Ascents made under the direction of the Kew Observatory Committee of the British Association." By John Welsh, Esq. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S., President of the British Association, on the part of the Council of the Association.

The object contemplated by the Kew Committee in the balloon ascents, of which an account is given in this communication, was chiefly the investigation of the variations of temperature and humidity due to elevation above the earth's surface. Specimens of the air at different heights were also obtained for analysis.

The instruments employed were the barometer, dry- and wet-bulb hygrometer, and Regnault's condensing hygrometer.

The barometer was a siphon, on Gay-Lussac's construction, without verniers; the upper branch of the siphon being alone observed, corrections having been previously determined for inequality of the tube at different heights of the mercury.

Two pairs of dry and wet thermometers were used, one pair having their bulbs protected from radiation by double conical shades open at top and bottom for the circulation of the air, the surfaces being of polished silver. The second pair were so arranged, that by means of an "aspirator," a current of air was made to pass over the bulbs more rapid than they would be exposed to by the mere vertical motion of the balloon. The object of this arrangement was to enable the thermometers to assume with more rapidity the temperature of the surrounding air, and also to diminish the effect of radiation, in case the shades should not be a sufficient protection, especially when the balloon was stationary or rising very slowly.

The thermometers used were very delicate, the bulbs being cylinders about half an inch long and not more than 1/32 of an inch diameter. It was found on trial that when the bulbs were heated 20° above the temperature of the air in a room, they resumed their original reading in 40 or 45 seconds, when moved through the air at the rate of 5 or 6 feet in a second. It is thus probable that any error arising from want of sensibility in the thermometers will be small, and in all likelihood not more than may be expected from other accidental causes.

The observations were taken at short intervals during the ascent, it having been seldom practicable to obtain a regular series in the descent. The intervals were generally one minute, but frequently only 30 seconds, so that an observation was for the most part recorded every 200 or 300 feet. All the observations are given in detail in the tables accompanying the paper. They are also given in the graphical form in the curves.

The ascents took place on August 17, August 26, October 21, and November 10, 1852, from the Vauxhall Gardens, with Mr. C. Green's large balloon.

The principal results of the observations may be briefly stated as follows:—

Each of the four series of observations shows, that the progress of the temperature is not regular at all heights, but that at a certain height (varying on different days) the regular diminution becomes
arrested, and for the space of about 2000 feet the temperature remains constant or even increases by a small amount: it afterwards resumes its downward course, continuing for the most part to diminish regularly throughout the remainder of the height observed. There is thus, in the curves representing the progression of temperature with height, an appearance of dislocation, always in the same direction, but varying in amount from 7° to 12°.

In the first two series, viz. Aug. 17 and 26, this peculiar interruption of the progress of temperature is strikingly coincident with a large and rapid fall in the temperature of the dew-point. The same is exhibited in a less marked manner on Nov. 10. On Oct. 21 a dense cloud existed at a height of about 3000 feet; the temperature decreased uniformly from the earth up to the lower surface of the cloud, when a slight rise commenced, the rise continuing through the cloud and to about 600 feet above its upper surface, when the regular descending progression was resumed. At a short distance above the cloud the dew-point fell considerably, but the rate of diminution of temperature does not appear to have been affected in this instance in the same manner as in the other series; the phenomenon so strikingly shown in the other three cases being perhaps modified by the existence of moisture in a condensed or vesicular form.

It would appear on the whole that about the principal plane of condensation heat is developed in the atmosphere, which has the effect of raising the temperature of the higher air above what it would have been had the rate of decrease continued uniformly from the earth upwards.

There are several instances of a second or even a third sudden fall in the dew-point, but any corresponding variation in the temperature is not so clearly exhibited, probably owing to the total amount of moisture in the air being, at low temperatures, so very small that even a considerable change in its relative amount would produce but a small thermal effect.

As the existence of the disturbance in the regular progression of temperature now stated rendered it necessary, in order to arrive at any approximate value of the normal rate of diminution with height, to make abstraction of the portion affected by the disturbing cause, each series was divided into two sections, the first comprising the space below the stratum in which the irregularity existed, and the second commencing from the point where the regular diminution of temperature was resumed. It was then found that the rate of diminution was nearly uniform within each section, but that it was somewhat greater in the lower than in the upper sections.

On taking a mean of both sections for each series, giving each section a value corresponding to its extent, it is found that the number of feet of height corresponding to a fall of one degree Fahrenheit is—

<table>
<thead>
<tr>
<th>Date</th>
<th>Height (feet)</th>
</tr>
</thead>
<tbody>
<tr>
<td>August 17</td>
<td>292.0</td>
</tr>
<tr>
<td>August 26</td>
<td>290.7</td>
</tr>
<tr>
<td>October 21</td>
<td>291.4</td>
</tr>
<tr>
<td>November 10</td>
<td>312.0</td>
</tr>
</tbody>
</table>
The first three values being remarkably coincident, and the last differing from them by about \(1/3\)th of the whole.

"The air collected in the ascents was analysed by Dr. Miller; he states that "the specimens of air do not differ in any important amount from that at the earth at the same time, but contain a trifle less oxygen. All of them contained a trace of carbonic acid, but the quantity was too small for accurate measurement upon the small amount of air collected."

June 9.—The following papers were read:

1. "Further Experiments and Observations on the Properties of Light." By Lord Brougham, F.R.S., Member of the Institute of France.

1. The author considers that Sir Isaac Newton's experiments to prove that the fringes formed by inflexion and bordering the shadows of all bodies, are of different breadths when formed by the homogeneous rays of different kinds, are the foundation of his theory, and would be perfectly conclusive if the different rays were equally bent out of their course by inflexion, for in that case the line joining the centres of the fringes on opposite sides of the shadow being, as he found them, of different lengths, the fringes must be of different breadths. He found that line to be \(1/3\) inch in the red, \(1/2\) in the violet of the nearest fringe; \(1/2\) in the red, \(1/2\) in the violet of the second fringe; and these proportions he found to be the same at all distances of the chart from the hair. But if the rays are of different flexibility, if the red, for example, is bent to a greater distance from its course than the violet, the experiment becomes wholly inconclusive; and the line joining the centres may be greater in the red than in the violet, although the breadths of the two fringes are equal, or even though the violet fringe may be broader than the red.

2. A variety of experiments are adduced in the paper to show that this property of different flexibility exists, which Sir I. Newton had not remarked. These experiments are either made with two bodies acting jointly on the rays, or with a single body so acting.

3. When made with two bodies, as sharp edges, these edges must be perfectly parallel, and when placed in the axis of the prismatic spectrum they form fringes more distant in the red than in any other part; least distant in the violet. The fringes are both broadest in the least refrangible rays and most removed; narrowest and least removed in the most refrangible. They incline from the red towards the violet.

4. The same experiment is easily made with a lamp or candle, placing a prism between the flame and the edges. This renders that exact parallelism of the edges which is required in the experiment with the spectrum, comparatively immaterial; because a considerable inclination of the edges, as at an angle of half a degree or more, does not affect the action on the rays in the very small space through which they pass by the edges.

5. With a single edge, or other body as a hair, the same difference in the position, as well as in the breadth of the fringes, is found to be observable, though not so manifestly as when two act together on
the light. The manner of making the observation most conveniently is pointed out.

6. These experiments are varied so as to show the various disten-
sions of the disc of a flame subjected to flexion between two edges, according as we regard the various portions of the flame's spectrum when seen by the prism. The red part is broadest, and when the near approach of the edges to each other divides the disc into two with a dark interval between them, that interval is the broadest in the least refrangible rays.

7. The experiments are further varied by using coloured glass instead of refracting with a prism.

8. The same phenomena are found to exist in all the other cases of flexion as where it is combined with reflexion by the action of specula, or by the effect of striated surfaces. There is always the same difference in the effects produced by the different kinds of homogeneous light.

9. The same phenomena are not so easily observed in the internal fringes, or those of the shadow; but the dark gray line in the axis of the shadow, having a line of deep black on each side, is found to vary in breadth and position in the different parts of its length cor-
responding to the colours of the spectrum, when a needle or other small body is placed in that spectrum.

10. The angle of inflexion is shown to be obtained by taking the distance at which the internal fringes begin to appear; and the pro-
portion of this angle in the red to the same angle in the violet is ascertained. The deflexion (as deduced from Sir I. Newton's ex-
periments) is much greater than inflexion appears to be. He had not observed the internal fringes of Grimaldi, to whom, however, he refers.

11. The author states that the property in question, the different flexibility of light, coexists with the other property, whatever it may be, which disposes the different rays in fringes of different breadths; but he considers that the two properties are wholly independent of each other.

12. He thinks there is reason to believe that the dark intervals between the fringes made in white light are only the dark tint of the adjoining fringes, of which the red of one runs into the violet of the other. The greatest care in repeating Sir I. Newton's experiment, with the same distances and sizes both of the body and the beam, leaves little or no doubt of the fringes running into each other. In homogeneous light it is otherwise; and there appear in that case to be the intervals, as might be expected from the different flexibility of the different rays.

13. The fringes made in homogeneous light have a considerable admixture of colours from the scattered rays; so have the small spectra by refraction made when a second prism is placed behind a small hole in the screen, through which hole the rays of the spectrum made by the first prism are successively passed.

14. The phenomena of flexion by bodies placed in the portion of the spectrum near the prism, and therefore white, are stated to be not easily accounted for in any received theory.

15. The Newtonian hypothesis of the different breadths of the
fringes being caused by the action of flexion extending to different distances over the different rays, is stated to be insufficient to account for it, and also to account for the different colours in the fringes to be made by white light. It is considered that the different flexibility will account for the latter, but not for the different breadths of the fringes, without another hypothesis, namely, the different ratio of the force to the distance from the bending body, in different rays.

16. The entire difference of flexion and refraction is shown from the different breadths of the fringes, and from their formation upon any possible hypothesis being shown to have nothing similar or analogous in the phenomena of refraction, though the different flexibility is precisely similar to the different refrangibility, only applicable inversely to the different rays.

17. The relation of the doctrine of interference to the phenomena of flexion is considered; and it is shown that certain of these phenomena are at variance with the doctrine. This is particularly exemplified in the case of the phenomena observed where bodies acting on light are not placed directly opposite to each other, but one behind the other.

18. The same phenomena are adduced to disprove M. Fresnel's hypothesis, that the phenomena of flexion (termed by him diffraction) depend entirely on the size of the aperture through which the light enters. Three experiments are adduced in disproof of this; the first made on the aperture when the edges are directly opposite each other; the second, when the edges are moved to different distances from each other on a line exactly parallel to the rays; the third, when the edges are moved on a line at any inclination to the rays. In both the second and third experiment, the vertical distance of the edges (i.e. the aperture) being the same, the breadth as well as the separation of the fringes is found to vary with the distance of the edges from each other horizontally, or in the direction of the rays.

ROYAL INSTITUTION OF GREAT BRITAIN.

June 3, 1853.—On some of the Eruptive Phenomena of Iceland.

By Dr. John Tyndall, F.R.S.

The surface of Iceland slopes gradually from the coast towards the centre, where the general level is about 2000 feet above the surface of the sea. On this, as a pedestal, are planted the Jökull or icy mountains of the region, which extend both ways in a north-easterly direction. Along this chain the active volcanoes of the island are encountered, and in the same general direction the thermal springs occur, thus suggesting a common origin for them and the volcanoes. From the ridges and chasms which diverge from the mountains mighty masses of steam are observed to issue at intervals, hissing and roaring, and where the escape takes place at the mouth of a cavern and the resonance of the cave lends its aid, the sound is like that of thunder. Lower down in the more porous strata we have smoking mud pools, where a repulsive blue-black aluminous paste is boiled, rising at times into huge bladders, which on bursting scatter their slimy spray to a height of fifteen or twenty feet. From the base of the hills upwards extend the glaciers, and on their shoulders are
placed the immense snow-fields which crown the summits. From
the arches and fissures of the glaciers, vast masses of water issue,
falling at times in cascades over walls of ice, and spreading for miles
and miles over the country before they find definite outlet. Extensive
morasses are thus formed, which lend their comfortless monotony
to the dismal scene already before the traveller’s eye. Intercepted
by the cracks and fissures of the land, a portion of these waters is
conducted to the hot rocks underneath; here meeting with the volca-
nic gases which traverse these underground regions, both travel
together, to issue at the first convenient opportunity either as an
eruption of steam or as a boiling spring.

The origin of the water which feeds the springs is here hinted at.
That origin is atmospheric. The summits of the Jökull arrest and
mix the clouds, and thus cause an extraordinary deposition of snow
and rain. This snow and rain constitute the source from which the
springs are fed. The nitrogen and ammonia which occur, without
exception, in every spring, exactly as we find them in rain-water,
the proof of this; for the known deportment of these sub-
stances preclude them from being regarded as real volcanic products.

The springs of Iceland permit of being divided into two great
classes; one class turns litmus paper red, the other restores the
colour; one class is acid, the other alkaline. Periodical eruptions
are scarcely ever known to occur among the former, while to the
latter belong the Geisers of the island. Here then we have two
facts which form the termini of a certain chain of operations—the
water of the clouds and the water of the spring; in its passage from
one terminus to the other is to be sought the cause of those changes
which the water has undergone.

In seeking insight here, experiment is our only safe guide. Let
us endeavour to combine the agencies of nature, and see whether
we cannot produce her results. Sulphurous acid is one of the most
important gases which the water encounters in its passage. Now
if a piece of palagonite, the rock through which the water filters, be
heated with an excess of aqueous sulphurous acid, it dissolves in the
cold to a fluid coloured yellow brown by the presence of peroxide
of iron. On heating the fluid this peroxide is converted into proto-
oxide; a portion of its oxygen goes to the sulphurous acid, forming
sulphuric acid, which combines with the bases of the rock and holds
them in solution. This is the first stage of the fumarole process. But if the process ended here, we might expect to find the dissolved
constituents of the rock in the resultant spring, which is by no means
the case, as a glance at the following table will show.

**Relation of Bases.**

<table>
<thead>
<tr>
<th>In Palagonite</th>
<th>In the Suffion water</th>
</tr>
</thead>
<tbody>
<tr>
<td>Oxide of iron</td>
<td>36·75</td>
</tr>
<tr>
<td>Alumina</td>
<td>25·50</td>
</tr>
<tr>
<td>Lime</td>
<td>20·25</td>
</tr>
<tr>
<td>Magnesia</td>
<td>11·39</td>
</tr>
<tr>
<td>Soda</td>
<td>3·44</td>
</tr>
<tr>
<td>Potash</td>
<td>2·67</td>
</tr>
<tr>
<td></td>
<td><strong>100·00</strong></td>
</tr>
</tbody>
</table>

We see here that the rock contains a large quantity of the oxide
of iron, while the spring does not contain a trace of it. It is, however, an experimental fact that the oxide of iron has been dissolved with the rest. How is its disappearance to be accounted for? The very rock from which it was originally extracted possesses the power of re-precipitating it, when by further contact with the rock the solution which contains it has its excess of acid absorbed and has thus become neutral. In this way the aqueous sulphurous acid acts as a carrier to the iron, taking up its burden here and laying it down there; and this process of transference can be clearly traced in the rocks themselves. Where the iron has been extracted, the rock has become a mass of white clay, where the iron is re-deposited the mass exhibits the colour produced by iron. But it would weary the audience, and thus defeat the object of the lecture, were the details thus minutely dwelt upon. Let it suffice therefore to weld swiftly together the links of the great chain operations, to which the various thermal springs and gaseous eruptions of Iceland owe their existence and peculiarities.

Hydrochloric acid, though playing a far less important part in Iceland than at Vesuvius and Etna, is nevertheless present. The presence of common salt is proved by the fact of its being found as one of the products of sublimation. Now it is a well known fact that this substance, exposed to a high heat in the presence of silica and the vapour of water, is decomposed; the sodium takes the oxygen of the water and becomes soda, the chlorine takes the hydrogen and forms hydrochloric acid. There is no difficulty, therefore, in accounting for the origin of this gas, as all the conditions for its formation are present.

Sulphurous acid and sulphuretted hydrogen play a most important part in Iceland;—how can their presence be accounted for? Let a piece of one of the igneous rocks of the island be heated to redness, and permit the vapour of sulphur to pass over it. The oxide of iron of the rock is decomposed; a portion of the sulphur unites with the iron, which remains as sulphuret; the liberated oxygen unites with the remaining sulphur, and forms sulphurous acid. Let the temperature of the heated mass sink till it descends just below a red heat, and then let the vapour of water be passed over it; a decomposition of the sulphuret before formed is the consequence; the iron is reoxidized, and the liberated sulphur unites with the free hydrogen to form sulphuretted hydrogen, and thus the presence of two of the most important agents in these phenomena is accounted for. These are experimental facts capable of being repeated in the laboratory, and the chronological order of the gases thus produced is exactly the same as that observed in nature. In the active volcanoes, where the temperature is high, we have the sulphurous acid; in the dormant ones, where the temperature has sunk so far as to permit of the decompositions just described, we have the sulphuretted hydrogen. This accounts for the irregular and simultaneous appearance of these two gases in various parts of the island. At Krisuvik, for example, exhalations of sulphurous acid, sulphuretted hydrogen, steam and sulphur*, burst in wild disorder from the hot ground. The first two gases cannot

* In nature the vapour of sulphur is doubtless derived from the action of heat upon certain sulphur compounds.
exist amicably together. In Iceland they wage incessant war, mutually decompose each other, and scatter their sulphur over the steaming fields. In this way the true solfataras of the island are formed.

In process of time, however, the heat retires to greater depths, the sources of the sulphurous acid and sulphuretted hydrogen become by degrees exhausted, and at such places the acid reaction of the soil disappears. Carbonic acid is found in abundance everywhere, but as long as the more powerful sulphuric acid is present the former must remain free. But when the acid reaction has disappeared, the carbonic acid combines with the alkaline bases, the bicarbonates thus formed impregnate the thermal waters, and become solvents for the silica which these waters are known to contain in such surprising abundance, and which, as we shall presently see, furnishes the materials for the wonderful architecture of the Geisers.

Casting our thoughts back upon the foregoing description, the hypothesis of internal heat will be seen to be implied, and from this as a cause we have deduced the various chemical phenomena as consequences. Holding fast by experiment, we see that the various gases whose existence has been urged as one of the strongest proofs of the so-called chemical theory, follow in the most natural and necessary manner from the rival supposition. Given the heat and the materials the results are such as any chemist acquainted with the reactions might predict à priori. By the labours of a chemist indeed a new and wonderful light has been thrown upon the entire volcanic phenomena of Iceland. With implicit reliance on the applicability of his science to the solution of these phenomena, he has travelled side by side with nature, combined her conditions, and produced her effects. Basing all his reasoning upon experiment, he has given to his conclusions a stability which mere speculation, however plausible, could never claim. That chemist is Bunsen, to whose researches in Iceland the audience were indebted for the materials of the present discourse.

The Lecturer then adverted to the Geisers; and proposed, as his time was limited, to confine his attention to the Great Geiser. We have here a tube ten feet wide and seventy feet deep; it expands at its summit into a basin, which from north to south measures fifty-two feet across, and in the perpendicular direction sixty feet. The interior of the tube and basin is coated with a beautiful smooth plaster, so hard as to resist the blows of a hammer. The first question that presents itself is, how was this wonderful tube constructed? How was this perfect plaster laid on? A glance at the constitution of the Geiser water will perhaps furnish the first surmise. In 1000 parts of the water the following constituents are found:

<table>
<thead>
<tr>
<th>Substance</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>0.5097</td>
</tr>
<tr>
<td>Carbonate of soda</td>
<td>0.1939</td>
</tr>
<tr>
<td>Carbonate of ammonia</td>
<td>0.0083</td>
</tr>
<tr>
<td>Sulphate of soda</td>
<td>0.1070</td>
</tr>
<tr>
<td>Sulphate of potash</td>
<td>0.0475</td>
</tr>
<tr>
<td>Sulphate of magnesia</td>
<td>0.0042</td>
</tr>
<tr>
<td>Chloride of sodium</td>
<td>0.2521</td>
</tr>
<tr>
<td>Sulphide of sodium</td>
<td>0.0088</td>
</tr>
<tr>
<td>Carbonic acid</td>
<td>0.0557</td>
</tr>
</tbody>
</table>
The lining of the tube is silica, evidently derived from the water; and hence the conjecture may arise that the water deposited the substance against the sides of the tube and basin. But the water deposits no sediment even when cooled down to the freezing-point. It may be bottled up and kept for years as clear as crystal, and without the slightest precipitate. A specimen brought from Iceland and analysed in this Institution was found perfectly free from sediment. Further, an attempt to answer the question in this way would imply that we took it for granted that the shaft was made by some foreign agency and that the spring merely lined it. A painting of the Geiser, the property of Sir Henry Holland—himself an eyewitness of these wonderful phenomena,—was exhibited. The painting, from a sketch taken on the spot, might be relied on. We find here that the basin rests upon the summit of a mound; this mound is about forty feet in height, and a glance at it is sufficient to show that it has been deposited by the Geiser. But in building the mound, the spring must also have formed the tube which perforates the mound; and thus we learn that the Geiser is the architect of its own tube. If we place a quantity of the Geiser water in an evaporating basin, the following takes place: in the centre the fluid deposits nothing, but at the edges where it is drawn up the sides of the basin by capillary attraction, and thus subjected to a quick evaporation, we find silica deposited; round the edge we find a ring of silica thus laid on, and not until the evaporation is continued for a considerable time, do we find the slightest turbidity in the central portions of the water. This experiment is the microscopic representative, if the term be permitted, of nature's operations in Iceland. Imagine the case of a simple thermal spring whose waters trickle over its side down a gentle incline; the water thus exposed evaporates speedily, and silica is deposited. This deposit gradually elevates the side over which the water passes until finally the latter has to choose another course; the same takes place here, the ground becomes elevated by the deposit as before, and the spring has to go forward—thus it is compelled to travel round and round, discharging its silica and deepening the shaft in which it dwells, until finally, in the course of centuries, the simple spring has produced that wonderful apparatus which has so long puzzled and astonished both the traveller and the philosopher.

Before an eruption, the water fills both the tube and basin, detonations are heard at intervals, and after the detonation a violent ebullition in the basin is observed; the column of water in the pipe appears to be lifted up, thus forming a conical eminence in the centre of the basin and causing the water to flow over its rim. The detonations are evidently due to the production of steam in the subterranean depths, which rising into the cooler water of the tube, becomes condensed and produces explosions similar to those produced on a small scale when a flask of water is heated to boiling. Between the interval of two eruptions, the temperature of the water in the tube towards the centre and bottom gradually increases. Bunsen succeeded in determining its temperature a few minutes before a great eruption took place; and these observations furnished to his clear intellect the key of the entire enigma. A little below
the centre the water was within two degrees of its boiling-point, that is, within two degrees of the point at which water boils under a pressure equal to that of an atmosphere, plus the pressure of the superincumbent column of water. The actual temperature at thirty feet above the bottom was 122° Centigrade, its boiling-point here is 124°. We have just alluded to the detonations and the lifting of the Geiser column by the entrance of steam from beneath. These detonations and the accompanying elevation of the column are, as before stated, heard and observed at various intervals before an eruption. During these intervals the temperature of the water is gradually rising; let us see what must take place when its temperature is near the boiling-point. Imagine the section of water at thirty feet above the bottom to be raised six feet by the generation of a mass of vapour below. The liquid spreads out in the basin, overflows its rim, and thus the elevated section has six feet less of water pressure upon it; its boiling-point under this diminished pressure is 121°; hence in its new position, its actual temperature (122°) is a degree above the boiling-point. This excess is at once applied to the generation of steam; the column is lifted higher, and its pressure further lessened; more steam is developed underneath; and thus, after a few convulsive efforts, the water is ejected with immense velocity, and we have the Geiser eruption in all its grandeur. By its contact with the atmosphere the water is cooled, falls back into the basin, sinks into the tube through which it gradually rises again, and finally fills the basin. The detonations are heard at intervals, and ebulitions observed; but not until the temperature of the water in the tube has once more nearly attained its boiling-point is the lifting of the column able to produce an eruption.

In the regularly formed tube the water nowhere quite attains the boiling-point. In the canals which feed the tube, the steam which causes the detonation and lifting of the column must therefore be formed. These canals are in fact nothing more than the irregular continuation of the tube itself. The tube is therefore the sole and sufficient cause of the eruptions. Its sufficiency was experimentally shown during the lecture. A tube of galvanized iron six feet long was surmounted by a basin; a fire was placed underneath and one near its centre to imitate the lateral heating of the Geiser tube. At intervals of five or six minutes, throughout the lecture, eruptions took place; the water was discharged into the atmosphere, fell back into the basin, filled the tube, became heated again, and was discharged as before.

Sir Geo. Mackenzie it is well known was the first to introduce the idea of a subterranean cavern to account for the phenomena of the Geiser. His hypothesis met with general acceptance, and was even adopted undoubtedly by some of those who accompanied Bunsen to Iceland. It is unnecessary to introduce the solid objections, which might be urged against this hypothesis, for the tube being proved sufficient, the hypothetical cavern disappears with the necessity which gave it birth.

From the central portions of the Geiser tube downwards, the water has stored up an amount of heat capable, when liberated, of
exerting an immense mechanical force. By an easy calculation it
might be shown that the heat thus stored up could generate, under
ordinary atmospheric pressure, a column of steam having a section
equal to that of the tube and a height of nearly thirteen hundred
yards. This enormous force is brought into action by the lifting
of the column and the lessening of the pressure described above.

A moment's reflection will suggest to us that there must be a
limit to the operations of the Geiser. When the tube has reached
such an altitude that the water in the depths below, owing to the
increased pressure, cannot attain its boiling-point, the eruptions of
necessity cease. The spring however continues to deposit its silica
and forms a laug or cistern. Some of these in Iceland are of a
depth of thirty or forty feet. Their beauty is indescribable; over
the surface a light vapour curls, in the depths the water is of the
purest azure, and tints with its own hue the fantastic incrustations
on the cistern walls; while at the bottom is observed the mouth of
the once mighty Geiser. There are in Iceland traces of vast, but
now extinct, Geiser operations. Mounds are observed whose shafts
are filled with rubbish, the water having forced a way underneath
and retired to other scenes of action. We have in fact the Geiser
in its youth, manhood, old age, and death, here presented to us:—
in its youth as a simple thermal spring, in its manhood as the eru-
pative spring, in its old age as the tranquil laug, while its death is
recorded by the ruined shaft and mound which testify the fact of its
once active existence.

Next to the Great Geiser the Stokkur is the most famous eruptive
spring of Iceland. The depth of its tube is forty-four feet. It is
not however cylindrical like that of the Geiser, but funnel-shaped.
At the mouth it is eight feet in diameter, but it diminishes gradually,
until near the centre the diameter is only ten inches. By casting
stones and peat into the tube and thus stopping it, eruptions can be
forced which in point of height often exceed those of the Great
Geiser. Its action was illustrated experimentally in the lecture, by
stopping the galvanized iron tube before alluded to loosely with a
cork. After some time the cork was forced up and the pent-up
heat converting itself suddenly into steam, the water was ejected to
a considerable height; thus demonstrating that in this case the tube
alone is the sufficient cause of the phenomenon.

XXI. Intelligence and Miscellaneous Articles.

ON THE CONденSATION OF GASES AT THE SURFACE OF SOLID
BODIES. BY MM. J. JAMIN AND A. BERTRAND.

In the various experiments intended to establish the physical theory
of gases, it is implicitly supposed that their state of equi-
librium is not influenced by the walls of the vessels in which they
are contained; it is supposed that no attractive or repulsive force
exists between solid and gaseous molecules. Nevertheless the general
principles of molecular physics do not justify our thinking that this
can be the case; we have no reason to suppose that gases are de-
prived of a property so energetically manifested by liquids; and if it
were so, we could not explain many phænomena which only require to be generalized in order to demonstrate the existence of this property.

Porous bodies present, in a very small space, a considerable amount of internal surface; the gases which penetrate into these substances lose their repulsive force, and accumulate in them as though by the influence of an extremely energetic attractive force. The phænomenon of porous bodies may be compared to that of capillarity; and just as the elevation of water in a tube may serve to show the existence of attractions between liquids and glass, the absorption of gases by charcoal is a proof of the attraction which a solid, isolated and continuous surface may exert upon gases.

After ascertaining and measuring the absorption of gases by various porous bodies, De Saussure called the attention of chemists to an important fact, namely, that he had proved that gases condensed in charcoal produced abnormal chemical actions; since that time Döbereiner discovered spongy platinum: these combinations, anticipated by De Saussure, became more evident; but it was seen that they were preceded by a condensation of the gases, and, in fact, were the consequence of this; they consequently serve to prove it.

As soon as the discovery of Döbereiner was announced, Thenard and Dulong repeated his experiments with some variations. They ascertained that the properties of spongy platinum were possessed by all porous bodies; they found them to exist in thin leaves of all the metals, and even in pounded glass or porcelain. Now if these combinations be the consequence of condensation, it must be admitted that this condensation takes place upon the metallic leaves and on the fragments of glass.

To these various experiments we must add the leading fact announced by M. Pouillet,—the absorption of oxygen in a platinum thermometer, and the condensation of the vapour of water by glass.

Moreover, this general idea admitted by geométricians, has often constituted the study of physicists, who, not hoping to prove it directly, have sought to verify it by indirect but very precise experiments. M. Arago proposed to cause the interference of two rays of light passing through the air, the one at a certain distance from, the other in contact with, a solid surface; he has recently returned to the same question, making use of the oscillations of a magnetized needle.

There exist, therefore, indirect proofs, which however to us appear conclusive, of the condensation of gases by solid surfaces; thus it was with nearly a certainty of success that we undertook the following experiments.

We filled glass vessels, which had been carefully measured, with pulverized solid substances; we ascertained the densities of the powders and the quantities contained in the vessels, and we had all the elements necessary for calculating the space left free.

Thus arranged, the vessels were connected with a good air-pump and with a manometer with two branches; one of the two branches was open to the air, it allowed the pressures to be ascertained; the other was closed, and communicated with the vessel by a tube and
stopcock; it served to measure a constant volume of gas, which was then driven into the vessel, by causing the mercury to rise. At each introduction of gas the pressure increased by a quantity which was measured, and which could be calculated by Mariotte’s law; the results of the experiment and of calculation were compared.

In this manner we have operated upon very various substances,—Fontainbleau sand, pounded glass of different degrees of fineness, and metallic filings and oxides. We have always found that the pressure observed was less than that calculated; we have therefore concluded that the gases were absorbed by the solid substances.

These absorptions present great analogy with those manifested by porous bodies; they are not produced instantaneously, but continue during several hours, only attaining their limit after a period which may be prolonged at pleasure; they vary in intensity according to the nature of the gas employed, being weak with hydrogen, stronger with atmospheric air, and very considerable with carbonic acid. We shall give their measure by the following results, obtained with pounded glass, washed and dried; the free space was 590 cubic centimetres, in which a vacuum was produced, and the gas was then allowed to fill it under the atmospheric pressure; it absorbed—

<table>
<thead>
<tr>
<th>Carbonic acid.</th>
<th>Air.</th>
<th>Hydrogen.</th>
</tr>
</thead>
<tbody>
<tr>
<td>645</td>
<td>602</td>
<td>595</td>
</tr>
</tbody>
</table>

We are convinced, moreover, that the preceding results are too low, and that it is impossible to measure exactly the quantities of gas contained in such spaces. When a vacuum is produced in them, the equilibrium of pressure is evidently re-established very slowly; the air-pump must be worked several hours to obtain a vacuum within 1 millimetre; and besides this, pressure does not remain constant, it gradually increases, and the action of the machine must be recommenced without ever being able to attain the maximum vacuum which it is capable of producing. The condensation obtained is the more energetic according to the goodness of the vacuum produced; but it is necessary to remember that its exact measure is never obtained.

Carbonic acid manifests these properties very energetically; when the powder with which the glass vessel is filled, whatever may be its nature, is exposed to this gas for the first time, it absorbs it rapidly, but on a second operation it has partially lost this property. The vessel already mentioned received, after evacuation, successive equal charges of this gas; the increase of pressure which they produced were measured, and by calculating the volume of the vessel by Mariotte’s law, there were found—

721 cub. cent. 636 cub. cent. 629 cub. cent. 627 cub. cent. 622 cub. c.

After these experiments a vacuum of the same degree was again produced, and the same successive introductions of gas being effected, gave—

644 cub. cent. 630 cub. cent. 621 cub. cent. 620 cub. cent. 616 cub. c.

From these results we must conclude,—1, that the absorption takes place with the more energy in proportion as the original pressure is weaker; 2, that after having once absorbed a gas, the solid substance retains a considerable portion of it, of which it cannot be
Meteorological Observations.

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deprived, and which causes a proportional diminution in its power of condensation.

These experiments require particular care, and can only be reproduced with very accurate apparatus; we will, however, describe one which anyone may repeat without difficulty, and which will exhibit our results in a conclusive manner.

A fine powder (pounded glass or oxide of zinc) is mixed in a mortar with water which has been deprived of air, so as to form a clear paste without any bubbles of gas; this is poured into a flask with a long neck until it fills two-thirds of the bulb. After a short time the solid substance is deposited with a layer of water above it. A vacuum is then produced in the flask; at the first strokes of the piston the water rises, increases in volume so as to fill the flask, but no bubble of air makes its appearance; and if the cock of the air-pump be suddenly opened, the pressure is reproduced, and the fluid returns to its original volume with a rapidity which shakes the flask, and a sound like that of the water-hammer. If the experiment be prolonged, and the vacuum completely formed, noticeable quantities of bubbles are produced.—Comptes Rendus, June 6, 1853, p. 994.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1853.


Mean temperature of the month ........................................ 59°·16
Mean temperature of June 1852 ........................................ 58·01
Mean temperature of June for the last twenty-seven years . 60·56
Average amount of rain in June ........................................ 1·77 inch.


Mean temperature of June for twenty-six previous years ...... 52°·69
Mean temperature of June 1852 .......................... 55·33
Mean temperature of this month ................................ 55·21
Average quantity of rain in June for seven previous years ... 2·46 inches.
<table>
<thead>
<tr>
<th>Days of Month</th>
<th>1983</th>
<th>June</th>
</tr>
</thead>
<tbody>
<tr>
<td>Max.</td>
<td>29.78</td>
<td>29.78</td>
</tr>
<tr>
<td>Min.</td>
<td>29.38</td>
<td>29.38</td>
</tr>
</tbody>
</table>

**Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Neil, at Boston; and by the Rev. C. Clouston, at Sandhurst, Woking, Oxon.**

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<tr>
<th>Location</th>
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When matter is in the gaseous state, it possesses three principal properties which are common to every species of gas, namely elastic force, density, and temperature; these all depend on the quantity of caloric contained in the gas, arising from the peculiar affinity existing between the dense matter and the caloric of the gas.

There are consequently three laws of the relations of the above properties of gases, which we may expect to have been discovered in an approximate form only in the first instance. The first is the law of Boyle and Mariotte for the relation of the elastic force and volume (or density) when the temperature is constant; the second is the law of Dalton and Gay-Lussac for the relation of the volume and temperature when the elastic force is constant; the third, that for the relation of the elastic force and temperature when the density is constant, is given by Amonton's law, which consists of the two previous laws compounded into one.

The above laws are independent of any consideration of the quantity of caloric in the gas; but we have important problems which require the knowledge of a fourth law for their solution, giving the relation of the volume and temperature when the quantity of caloric is constant. An hypothetical law was assumed by Poisson† in order to obtain a solution of the problem of sound, which was adopted in another form by Laplace‡.

* Communicated by the Author.
† Journal de l'Ecole Polytechnique, vol. vii. p. 363. "Et généralement si une couche d'air éprouve une condensation très petite et désignée par γ, entre deux autres couches d'air, la température de la première devra s'élérer de 116° γ."
Poisson's assumption was, that in the sudden condensation or rarefaction of a gas the temperature was increased or diminished respectively by 1° Centigrade for every \( \frac{1}{5} \) th part of condensation or rarefaction. In the method of Laplace, the ratio of the specific heat of air at a constant pressure to the specific heat with a constant volume was required to be known. The ingenuity of MM. Désormes and Clement devised an experimental method* of determining this ratio which has received the general assent of the scientific world, and which will not be forgotten in the history of science. A receiver furnished with a vertical pipe and stopcock had also a horizontal pipe connecting it with an air-pump. To the horizontal pipe were attached a mercurial barometer-gauge and a water-gauge, which each indicated the elastic force of the air inside the receiver, within their respective limits; but the water-gauge being the most sensitive, was the one observed in the experiments, and its indications afterwards converted to mercurial pressures. A portion of air being withdrawn by the air-pump, and the reading of the water-gauge noted, the stopcock was then opened, and in two-fifths of a second the atmospheric air ceased to rush into the receiver, and the stopcock was then closed. It was seen that the water in the water-gauge descended to a certain point and then rose to a higher level, which were both noted. This latter rise of level was attributed entirely to the heat which was developed in the sudden condensation, and which gradually disappeared. We see here that the effect of the momentum of the moving column of water in the gauge in passing from one position of equilibrium to another has been attributed to the effect of heat developed in the sudden condensation.

At the request of M. Laplace†, experiments on the same principle were made by MM. Gay-Lussac and Welter, only using condensation where MM. Désormes and Clement used rarefaction.

The method of M. Dulong‡ to find the value of the above-named ratio, was to admit as demonstrated Laplace's formula for the velocity of sound; and comparing it with the observed velocities, to deduce, by the inverse process, the value of the ratio required.

The reader will judge how much need there was of a true experimental basis for determining the fourth law of the relations of the elastic force, density, and temperature in gases, for the quantity of caloric constant.

Since the publication of my papers on Hydrodynamics in this

‡ Annales de Chimie et de Physique, vol. xii. p. 131.
Journal in the spring of 1851, I have frequently considered the methods it would be the most desirable to pursue in order to determine the heat or cold produced in the sudden condensation or rarefaction respectively of gases, and then the ratio of the specific heat under constant pressure to that for a constant volume becomes known. That many precautions were requisite was generally admitted; but if the changes of temperature were, as supposed, anywhere near 1° Centigrade for a condensation or rarefaction of \( \frac{1}{100} \)th part, good indications of great change of temperature would be easily obtained for a condensation or rarefaction equal to unity. Amongst others, I thought the following method would enable me to arrive at the heat absorbed when gas escaped through a small aperture from a higher to a lower pressure.

Marcet’s boiler, the instrument for determining the relation of the pressure and temperature in high-pressure steam, is furnished with an adapter for holding a thermometer, which has the bulb inside and the scale outside the strong spherical boiler; it has also another adapter with a stuffing-box, which holds vertical a long glass tube open at both ends, the lower end being immersed in a known quantity of mercury which has been poured into the boiler; the elastic force of the vapour or gas inside the boiler, when greater than that of the atmosphere, is known from the height the mercury rises in the tube. It is also furnished with a pipe and stopcock, to which nozzles can be screwed. When steam at high pressure issues from such a nozzle, the phenomenon of the disappearance, at a short distance, of the high temperature which the steam possessed inside the boiler, is very satisfactorily witnessed.

I thought that if air were condensed into the boiler, proper thermometers being employed, the change of temperature of the air on issuing from the high pressure inside to the pressure of the atmosphere outside might be determined. The following points required attention: first, the aperture through which the air issued should be small compared with the size of the pipe, and have a sharp edge that friction might produce no sensible effect; secondly, the thermometer to indicate the temperature of the jet of air must give its indication rapidly. The slowness of the action of the common mercurial thermometer, even when made with an elongated bulb, as in the case of those inside and outside the boiler, which I used, required a particular mode of use. It was the conviction that I had found this mode that induced me to prosecute the experiments. I considered that, if a chamber containing the exterior thermometer were prepared, of a slow absorbing and conducting power for heat, and attached air-tight to the nozzle with the small aperture, but open to the
atmosphere at the further end, around the stem of the thermometer; then if the stopcock were opened for a short space of time, say not exceeding one second, the air in the chamber being replaced many times in this interval, then by watching the mercury of the thermometer with a compound microscope it would be easily seen whether it rose or fell, which was all I proposed to learn from it; for I intended to bring the chamber and its contained thermometer to a temperature, before putting it in its place at each trial, which I wished to compare with that of the issuing jet; and by successive trials I expected to find the temperature at which the jet produced no effect on the thermometer, and then its temperature would be determined.

In this mode of use, as a thermoscope only, the common mercurial thermometer is very sensitive, as the following shows. When the difference of temperature of the jet and thermometer exposed to it was several degrees, the first impression of the jet appeared the contrary of what took place the instant afterwards when the mercury became heated or cooled. The first impression was evidently due to the heating or cooling of the very thin glass of the bulb, which caused an indication the opposite of what occurred when the contained mercury became affected with the change of temperature. In most, though not all, of my observations, I noted the first as well as the second impression of the jet, and also the tendency of the thermometer to rise or fall after the jet had ceased, together with its slow continuous rise to the temperature of the surrounding objects afterwards.

The early trials I made this spring convinced me that I should not be required to go to much expense for freezing mixtures, to reduce the exterior thermometer to the temperature of the issuing jet of air; but I found results so widely different from Poisson's assumption, and so small in comparison with it, that I at first doubted the adequacy of my method of experimenting for determining the temperature of the jet in given circumstances of pressure and temperature within the receiver. From professional duties having to discontinue the experiments, I had time to think over the improvements required in order to determine such small changes of temperature with an accuracy sufficient to determine their law.

On resuming the experiments this summer, I had a fine silk thread stretched across the field of view of the microscope, which was afterwards changed for fine cross micrometer lines at right angles, and which gave great additional precision by bringing the image of the extremity of the mercury in the stem to very near apparent touching of the cross line, when the stopcock was opened. In this way a change of $\frac{1}{100}$th of a degree Fahrenheit was distinctly visible. When it was found that tenths of degrees
must be read with certainty, other precautions presented themselves; for instance, the stopcock, it was found, must not be opened and shut with the fingers, but a long wooden key must be applied for that purpose; and, again, the chamber containing the exterior thermometer must be attached to the nozzle by a wooden adapter, and must be at the temperature required when put in its place, and the jet must immediately take place. By employing these methods and others which will readily suggest themselves to an experimenter, the limits between which the temperature of the jet must lie were gradually narrowed for changes of density of about three atmospheres and two atmospheres to that of one atmosphere, or for rarefactions two and one.

The figure represents the essential parts of the apparatus which I have used.
A is the strong spherical boiler or receiver, about 5½ inches diameter.

BC is a portion of the vertical glass tube with its wooden scale; the tube was rather more than 65 inches long, so that a pressure of more than 60 inches of mercury, or more than two additional atmospheres, could be employed.

D is the ivory scale of the thermometer which has its elongated bulb inside the receiver.

E is the ivory scale of the thermometer which has its bulb of an inverted pear shape, inside the chamber a.

F is the microscope by which the motion of the mercury in the stem of the exterior thermometer E is noted. There were means of adjustment not drawn in the figure; the magnifying power used was 18 to 20 times linear.

The chamber a was constructed of a piece of brass tube, having in its interior a coil of six folds of the finest woollen fabric, and inside this woollen coil was a cylinder of the best gilt paper. The diameter of this cylinder of gilt paper was larger than the thickest part of the pear-shaped bulb by about 0.04 inch, so that there was abundant space for the escape of the jet of air all around the bulb. A small quantity of wool was also wrapped round the stem above the thermometer bulb to check the re-entry of the external air, but not to affect the state of pressure. The thermometer scale was fixed by wooden wedges inside the brass tube, which had open spaces round the thermometer stem to allow the free escape of the jet.

The adapter b was a ring of box-wood fitting tightly on the end of the nozzle c, and receiving tightly the cylinder a, but keeping them from contact.

The nozzle c was a piece of brass, cylindrical outside, but with a milled ring for screwing it to the end of the pipe of the stopcock. The aperture of the nozzle was about 1/100th of an inch. It was made by passing a fine needle through the brass, when the drill by which the pipe was formed was just about to come through the end of the brass; the aperture was then carefully finished so as to have a sharp edge. The end of the nozzle was covered, except a small space over the aperture, with two folds of woollen cloth and a disc of gilt paper cemented in their place.

The condensation was performed with a condensing syringe screwing to the pipe with the stopcock.

The sphere A, containing about 90 cubic inches, was easily injected to a density of three atmospheres in one minute of time. The temperature inside rose by this 4° to 6° Fahrenheit, according to the friction of the piston and consequent heating of the condensing syringe, and was allowed half an hour or more before it was used, in order that the whole apparatus might acquire a
uniform temperature, the rule being that no change was perceived in five minutes' time in the interior thermometer.

Previous to commencing the experiments, the calibre of the tube of the mercury gauge was found by measuring the lengths of it filled by 100 grains of mercury; and the heights of the mercurial column at different places above the level in the receiver by measurements of the heights occupied by successive 1000 grains, from 1 lb. avoirdupois up to 1 lb. plus 7000 grains; and then a formula was obtained giving the height of the mercurial column belonging to any given reading on the scale of the gauge. Care was also taken to compare the readings of the interior and exterior thermometers in their places as actually used, and after long rest, when the room where the trials were made was only undergoing small changes of temperature.

The experiments made in March showed that the difference of the temperatures of the air in the jet and in the receiver, when the mercurial gauge stood about 60 inches, could not be near so much as 8° Fahrenheit, as the mercury of the exterior thermometer rose (at the second impression) rapidly, when the jet passed into the chamber containing its bulb. In these experiments, the nozzle with the thermometer in its chamber attached were all cooled together to the lower temperature, and screwed rapidly in their place, with a pair of leather gloves on the hands, to get the full impression of cold when the stopcock was opened.

In June, when some of the precautions before mentioned had been adopted, the experiments were recommenced with the exterior thermometer and its chamber cooled 13° below the air in the receiver; and with this difference, and various intermediate ones up to a difference of 3°, the thermometer rose rapidly when the jet passed into its chamber with a rarefaction two, or when the air passed from the pressure of three atmospheres to the pressure of the external atmosphere. When the difference was only 1°, there was always a small but distinct fall of the thermometer on the jet passing into the chamber; when the differences were 1°.5 to 2°, irregularities in the results appeared, until the final precaution was adopted of passing the jet the moment the exterior thermometer was put in its place, and this required experience with the microscopic adjustments.

Experiments were also tried with a rarefaction about unity, or with the mercurial gauge indicating an additional pressure of 30 inches of mercury upon the air in the receiver. The effects were quite visible, though slower than in those before mentioned, on account of the less rapidity with which the air in the chamber was replaced. It was found that the temperature of the jet could not be nearly 1° below that of the air in the receiver.
The jet caused the thermometer in the chamber to rise with certainty when the differences were also $0^\circ\cdot6$, $0^\circ\cdot45$, $0^\circ\cdot3$; but it fell when they were only $0^\circ\cdot1$ and $0^\circ\cdot15$.

The results in the table below were obtained under the best conditions, and it will be seen that I was fortunate enough at length to get the differences of temperature when no effect was produced by the jet for rarefactions two and one nearly, and so to determine the relation required; but we must expect some correction of the constant when greater rarefactions have been employed with certainty. I think it quite probable some methods can be found for using higher rarefactions or condensations, and have in view such further experiments myself.

<table>
<thead>
<tr>
<th>Corrected height of the mercurial gauge in inches</th>
<th>Height of the barometer in inches</th>
<th>Difference of the interior and exterior thermometers</th>
<th>Result of the jet on the exterior thermometer</th>
<th>Calculated difference of temperatures</th>
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<tbody>
<tr>
<td>62·55</td>
<td>29·80</td>
<td>1·6</td>
<td>A small rise about $\frac{1}{40}$th of a degree, then a small fall, and then the very slow advance.</td>
<td>1·9204</td>
</tr>
<tr>
<td>63·40</td>
<td>29·80</td>
<td>2·0</td>
<td>Not the slightest effect at the first or second impression, but after some time the continuous rise began.</td>
<td>2·0</td>
</tr>
<tr>
<td>62·34</td>
<td>29·80</td>
<td>1·35</td>
<td>A very slight rise, then a very slight fall, then the continuous rise.</td>
<td>1·9020</td>
</tr>
<tr>
<td>59·54</td>
<td>29·80</td>
<td>2·2</td>
<td>The second impression a steady advance, and it continued to advance more slowly after the jet had ceased.</td>
<td>1·6564</td>
</tr>
<tr>
<td>44·79</td>
<td>29·80</td>
<td>0·5</td>
<td>Very little effect; if anything, a check to the very slow advance. This rather a hurried experiment.</td>
<td>0·7054</td>
</tr>
<tr>
<td>33·46</td>
<td>29·25</td>
<td>0·3</td>
<td>The very slightest advance, and it returned to the original place when the jet ceased; afterwards a slow advance.</td>
<td>0·3109</td>
</tr>
<tr>
<td>32·26</td>
<td>29·25</td>
<td>0·6</td>
<td>An advance which was maintained after the jet ceased.</td>
<td>0·2785</td>
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<tr>
<td>31·35</td>
<td>29·25</td>
<td>0·25</td>
<td>No effect seen until the jet had passed for two seconds, and then a slight fall, and a very slow rise afterwards.</td>
<td>0·2558</td>
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Density, and Temperature of Gases.

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To express the rarefaction in numbers,
Let \( v \) be the volume of a gas when the pressure is \( p \);
Let \( v' \) be the volume of a gas after an expansion and the pressure has become \( p' \);
Let also \( \delta \) be the rarefaction or negative condensation;
then \( \delta = \frac{v' - v}{v} = \frac{p - p'}{p'} \), by Boyle’s law, since the change of temperature is small.

Now comparing the last of the observations with the second, it appears that the difference of the temperatures varies as the cube of the rarefaction; or if we put \( \omega \) for the difference when the rarefaction is one, and \( \omega \) that for any other rarefaction \( \delta \), we have

\[
\frac{\omega}{\omega_1} = \left(\frac{\delta}{1}\right)^3,
\]

\[ \therefore \omega = \omega_1 \delta^3, \]

which is the fourth law discussed at the commencement of the paper.

To find \( \omega_1 \) we must employ the largest value of \( \delta \) in the best trial, which was the second in the table, where

\[
\omega = 2^\circ,
\]

\[
\delta = \frac{63 \cdot 40}{29 \cdot 80},
\]

and these give

\[
\omega_1 = 0^\circ \cdot 2077.
\]

Employing this as the constant in the formula \( \omega = \omega_1 \delta^3 \), the values of \( \omega \) in the last column of the table were calculated; and we see that they confirm the accuracy of the formula, on comparing them with the observations.

We are now able easily to find the ratio of the specific heat of air at a constant pressure to that for a constant volume.

Using a notation and method like that of Poisson, Traité de Mécanique, vol. ii. p. 644, let \( c \) = the capacity of air for heat at a constant pressure = the quantity of caloric which will raise the temperature of one grain of air, 1° Fahrenheit;

Let \( \Gamma \) be the quantity which will raise it \( \epsilon \) degrees; then

\[
\Gamma = ce.
\]

Again, let \( c' \) = the capacity of air for heat at a constant volume or density

= the quantity of caloric which will raise one grain of air 1° Fahrenheit, under this condition;

then the quantity \( \Gamma \) will raise it more than \( \epsilon \) degrees, since it

has not been allowed to expand: let the temperature it will be raised be \((e + \omega)\) degrees, and the elastic force will be also increased. Then we have

\[
\Gamma = c'(e + \omega) = ce,
\]

\[
\therefore \frac{c}{c'} = \frac{e + \omega}{e} = 1 + \frac{\omega}{e}.
\]

Now if no caloric be lost, \(\omega\) is the temperature which would be lost by the air when allowed to expand until it had got its original elastic force, and the value is got from the expression

\[
\omega = \omega_i \delta^3.
\]

Again, \(e\) is found from the law of Dalton and Gay-Lussac, since the pressure was supposed constant, in terms of the rarefaction, as follows:

Let \(v_0\) be the volume of a gas at the freezing-point,

\[
v = v_0(1 + \alpha \theta)
\]

\[
v' = v_0(1 + \alpha(\theta + \epsilon))
\]

\[
v' - v = \frac{1 + \alpha \theta + \alpha \epsilon}{1 + \alpha \theta} = 1 + \frac{\alpha \epsilon}{1 + \alpha \theta},
\]

and

\[
\frac{v' - v}{v} = \delta = \frac{\alpha \epsilon}{1 + \alpha \theta};
\]

or

\[
\epsilon = \frac{\delta(1 + \alpha \theta)}{\alpha},
\]

and

\[
\frac{c}{c'} = 1 + \frac{\omega}{e} = 1 + \frac{\alpha \omega \delta^3}{\delta(1 + \alpha \theta)} = 1 + \frac{\alpha \omega \delta^2}{1 + \alpha \theta}.
\]

Putting the value of \(\alpha = \frac{1}{490}\), and that of \(\omega_i = 2077\), we have

\[
\frac{c}{c'} = 1 + \frac{\delta^2}{2359(1 + \alpha \theta)};
\]

or the ratio of the two specific heats is unity very nearly for small condensations and rarefactions.

London, July 28, 1853.
XXIII. On the Discovery of Quinine and Quinidine in the Urine of Patients under Medical treatment with the Salts of these mixed Alkaloids. By W. Bird Herapath, M.D.*

[With a Plate.]

It has long been a favourite subject of inquiry with the professional man to trace the course of remedies in the system of the patient under his care, and to know what has become of the various substances which he might have administered during the treatment of the disease. Whilst some of these remedies have been proved to exert a chemical change upon the circulating medium, and to add some of their elements to the blood for the permanent benefit of the individual, others, on the contrary, make but a temporary sojourn in the vessels of the body, circulating with the blood for a longer or shorter period, but are eventually expelled and eliminated from it at different outlets and by various glandular apparatus; some of these substances are found to be more or less altered in chemical composition in consequence of having been subjected to the manifold processes of vital chemistry during their transit through the system, whilst others have experienced no alteration in their constitution, but have resisted all the destructive and converting powers of the animal laboratory, and by appropriate means have been again separated from the various excretions by the physiological and pathological chemist in their pristine state of purity.

It has recently been somewhat more than a subject of conjecture that, in common with several others of the vegetable alkaloids, quinine may be included in this last class of remedial agents, and several methods of discovering its presence have emanated from different scientific observers. It has been repeatedly traced in the urine of patients suffering from intermittent fever, to whom large doses of this substance have been necessarily administered.

The nature of the tests hitherto employed and the various processes adopted require large quantities of the fluid for examination, and the imperfection of the evidence resulting from the experiment still threw considerable doubt upon the value of the conclusions arrived at. It is merely necessary to allude to tannic acid and tincture of iodine as the usual tests employed, both being very far from efficient means of detecting minute quantities of quinine in organic fluids.

Having been struck with the facility of application and the extreme delicacy of the reaction of polarized light, when going through the series of experiments upon the sulphate of iodo-

* Communicated by the Author.

† Papers published in Philosophical Magazine for March 1852, and Analysis of Crystals in ditto for September 1852.
quarine, I determined upon attempting to bring this method practically into use for the detection of minute quantities of quarine in organic fluids; and after more or less success by different methods of experimenting, I have at length discovered a process by which it is possible to obtain demonstrative evidence of the presence of quarine, even if in quantities not exceeding the \( \frac{1}{100,000} \)th part of a grain—in fact, in quantities so exceedingly minute, that all other methods would fail in recognizing its existence. The same process with a slight modification has also enabled me to prove the fact, that quarine escapes from the system by the kidneys in an unaltered state, which, as far as I am aware, has not hitherto been observed, although it might have been almost assumed from the great analogical resemblance existing between these alkaloids.

Chemical examination of the urine of a man suffering from tetanus, in consequence of an injury to the great toe. Amputation was performed at the infirmary by Mr. Morgan. The patient's name was R. Alexander.

The tetanic symptoms were treated by the exhibition of 5 grains of disulphate of quarine with \( \frac{1}{2} \) a grain of Cannabis Indica every three hours; he consequently took 40 grains of sulphate of quarine in the period of twenty-four hours.

The urine had a greenish-yellow appearance, and upon standing deposited a brownish-yellow sediment; it was slightly acid, and had a specific gravity of 1.032. The sediment examined by the microscope showed prisms and lozenges of uric acid, with amorphous urates of ammonia.

The deposit treated upon the field of the microscope with ammonia instantly became changed; the crystals of uric acid were rendered more clearly defined in consequence of the amorphous urates being dissolved. The phosphate of ammonia and magnesia was subsequently deposited upon the slide as a cloudy mass, when seen by the unassisted vision, but as a magma of very minute radiating needles when magnified sixty diameters.

The fluid urine was carefully decanted from the amorphous and crystalline deposit.

A. Half a pint of this urine was treated with liquor potassae until decidedly alkaline; it was then repeatedly agitated with pure washed æther; the æthereal solution having been allowed to separate by twelve hours' repose, was carefully removed by a pipette, and then transferred to a counterpoised glass tube and evaporated by a warm water bath; 79 grain of extract was left.

B. A magma of phosphates and adhering alkaloid still remained above the urinous substratum; this was also removed by a pipette and transferred to a porcelain capsule; evaporated to dryness at 212°, and this residue exhausted by æther; the
æthereal solution evaporated to dryness by a warm water bath, as before, gave .61 additional alkaloid.

Therefore \( \frac{a}{.79} + \frac{b}{.61} = 1.4 \) grain of alkaloid were obtained by these two operations from the 8 fluid ounces of urine.

Now to determine whether it were quinine, the following process was followed:—

**Test-fluid.** A mixture of 3 drachms of pure acetic acid, with 1 fluid drachm of rectified spirit of wine, to which 6 drops of diluted sulphuric acid were added.

One drop of this test-fluid placed on a glass slide, and the merest atom of the alkaloid added; time given for solution to take place; then upon the tip of a very fine glass rod an extremely minute drop of the alcoholic solution of iodine added; the first effect is the production of the yellow or cinnamon-coloured compound of iodine and quinine, which forms as a small circular spot; the alcohol separates in little drops, which by a sort of repulsive movement drive the fluid away; after a time the acid liquid again flows over the spot, and the polarizing crystals of sulphate of iodo-quinine are slowly produced in beautiful rosettes. This succeeds best without the aid of heat.

To render these crystals evident, it merely remains to bring the glass slide upon the field of the microscope with the selenite stage and single tourmaline beneath it; instantly the crystals assume the two complementary colours of the stage; red and green supposing that the pink stage is employed, or blue and yellow provided the blue selenite is made use of. All those crystals at right angles to the plane of the tourmaline producing that tint which an analysing plate of tourmaline would produce when at right angles to the polarizing plate; whilst those at 90° to these educe the complementary tint, as the analysing plate would also have done if revolved through an arc of 90°. Vide Plate II. figs. 1 and 2.

This test is so ready of application and so delicate, that it must become the test, par excellence, for quinine. Vide figs. 1 and 2.

Not only do these peculiar crystals act in the way just related, but they may be easily proved to possess the whole of the optical properties of that remarkable salt of quinine, so fully described by me in the Philosophical Magazine for March 1852, and the chemical analysis of which was published in the number for September of the same year; in fact, these crystals are perfectly identical with the sulphate of iodo-quinine in every respect.

To test for quinidine, it is merely necessary to allow the drop of acid solution to evaporate to dryness upon the slide, and to examine the crystalline mass by two tourmalines, crossed at
right angles and without the stage. Immediately little circular discs of white, with a well-defined black cross very vividly shown, start into existence, should quinidine be present even in very minute traces. Vide fig. 3.

This is generally the case if hospital quinine or that of the British Alkaloid Company has been employed—these severally contain a very large per-centage of quinidine; the former at least 50, the latter about 20; but Howard's disulphate of quinine scarcely contains 5 per cent. of quinidine, according to my experiments. These substances are easily separated in consequence of the much greater solubility of the disulphate of quinidine in cold water.

Water.

One part of disulphate of quinine requires 740 at 60°; One part of disulphate of quinidine requires 340 at 55°; so that the latter is more than twice as soluble.

If we employ the selenite stage in the examination of this object, we obtain one of the most gorgeous appearances in the whole domain of the polarizing microscope; the black cross at once disappears, and is replaced by one which consists of two colours, being divided into a cross having a red and green fringe, whilst the four intermediate sectors are of a gorgeous orange-yellow. These appearances alter upon the revolution of the analysing plate of tourmaline; when the blue stage is employed, the cross will assume a blue or a yellow hue according to the position of the analysing plate.

These phenomena are analogous to those exhibited by certain circular crystals of boric acid and to those circular discs of salicine (prepared by fusion*), the difference being that the salts of quinidine have more intense depolarizing powers than either of the other substances, besides which the mode of preparation effectually excludes these from consideration. Quinine prepared in the same manner as the quinidine has a very different mode of crystallization; but it occasionally presents circular corneous plates, also exhibiting the black cross and white sectors, but not with one-tenth part of the brillianey, which of course enables us readily to discriminate the two.

Having shown in my previous papers that none of the vegetable alkaloids, when treated with sulphuric acid and iodine, possess the power of forming crystalline compounds of similar properties, and these artificial quinine tourmalines being pre-eminent in their action on light, it follows that the existence of these crystals is a positive demonstration of the presence of quinine. It has also been proved by me that quinidine (β qui-

* I am indebted to my friend Mr. John Thwaites for this fact.
nine) cannot produce them, therefore we perceive that quinine passes out of the system without experiencing any elementary change.

One subject is worthy of remark: the patient was taking 40 grains of the disulphate of quinine (and quinidine); there were found 1·4 grain of mixed alkaloids, which would be equivalent to 1·884 grain of the disulphate of commerce; and if the patient voided 3 pints of urine in twenty-four hours, we should only account for 11·304 grains of the remedy used, leaving a deficiency of 30 grains; three-fourths of the substance being yet unaccounted for, which has either been assimilated in the body, or has been destroyed in its transit through the vascular system. It would be interesting to undertake a series of quantitative analyses in the healthy subject to determine these points:—

1st. Whether the whole of the quinine ingested disappears by the kidnies.

2ndly. If not, whether it passes out by other excretory organs, and which.

3rdly. To discover at what period after ingestion all evidence of its elimination from the kidnies ceases.

These questions having been answered, in health, they must be repeated when quinine has been used remedially, and by so doing we shall perhaps be in a position to say what the medical equivalent of quinine may be in a given disease.

32 Old Market Street, Bristol,
July 10, 1853.

XXIV. On the Classification of Minerals. By E. J. Chapman, Professor of Mineralogy and Geology in University College, Toronto, Canada West; late Professor of Mineralogy in University College, London*.

A
CLASSIFICATION of natural objects should evidently fulfil the two following conditions: it should enable us to make out readily the name of any one of the included bodies, and to group these in accordance with their natural attributes and affinities. It should be thus, at the same time, a classification for the discrimination, one from another, of the objects in question, and a classification for the study of their analogies. So far as regards Mineralogy—speaking always as to the present state of our knowledge—these two conditions do not seem to be attainable in a single classification. In Zoology and Botany, the great leading divisions of Cuvier, on the one hand, and those of De Jussieu and DeCandolle on the other, bearing, as they do, the undeniable impress of truthful interpretation, are universally adopted; although, from time to time, a few subordinate modi-

* Communicated by the Author.
fications may necessarily have to be introduced into the general plan by the natural progress of discovery. In no case, however, do the most important of these modifications, any more than the slight differences of opinion which prevail in regard to some of the minor subdivisions, invalidate the general fact, that there exist for these sciences certain primary or fundamental groupings of universal recognition. In Mineralogy, on the contrary, we have nothing of the kind. No broad, well-marked and acknowledged divisions, which strike the mind at once as being natural and true. No agreement, indeed, amongst the cultivators of the science as to what should constitute the basis of a mineral classification. This arises principally from the double nature, so to say, possessed by minerals; or perhaps, more correctly, from the prominence and individuality of each of these natures in every mineral species. For if animals and plants admit also of being considered both as chemical and as physical bodies, their respective chemical natures either offer merely a few inessential points of dissimilarity, or where a real difference of the kind does actually exist—as in the solid parts of the Vertebrata compared to those of the Articulata, Mollusca and Radiata—the difference affects not individuals only, but whole classes, and is in conformity with the physical differences exhibited by the same; so that in the classification of plants and animals, it is to their physical attributes solely—in contradistinction at least to their chemical qualities—that we have to look. In the classification of minerals, on the contrary, both natures must be taken into consideration, and it is here that the principal difficulty lies; because, as a general rule, no immediately perceptible relations can be traced between the two.

That a satisfactory classification cannot be founded solely upon chemical characters is becoming daily more and more evident; whilst, at the same time, the so-called "natural-historical" divisions of Mohs and his followers are seen, in numerous instances, to be equally unsatisfactory. In affecting to deny the right of chemical characters to be considered a classification-element in Mineralogy, it is yet very obvious that many of their groupings have been influenced by, if not entirely based upon, a consideration of these characters. The incongruous union in the order "Ore," for example, of specular iron, magnetic iron ore, ruby copper, and tinstone—minerals which have really no affinities in common, is a striking case in point. The separation, again, of apatite from pyromorphite, of siderose (carbonate of iron) from calcite, of barytine from anglesite, amongst numerous other equally salient examples, are amply sufficient to show how little the system merits the appellation of a natural one. If the chemical nature of minerals, on the other hand, be only taken into account, it is as yet impossible, with all the assistance to be
derived from a full recognition of isomorphism, to avoid the most striking incongruities. A few examples will be sufficient to establish this assertion:— (1.) Silver, arsenic and sulphur, as "simple substances," must be placed in one and the same group. It is very evident, however, that silver has a far greater affinity to the argyrose or sulphide of silver, in every point of view, than to either sulphur or arsenic. What, moreover, has sulphur in common with these other "simple substances" that we should place it with them?—and thus separate it more or less widely from orpiment, a mineral to which it is allied not only by general physical characters and crystallization, but also by the kindred conditions under which both occur in nature. (2.) Iron pyrites and molybdenite are bisulphides, and, as such, fall into the same chemical division; all natural analogies are, however, against this union, and in favour of the grouping of molybdenite with many of the simple sulphides. (3.) Cinnabar belongs chemically to these latter, but crystallographically and physically it has little affinity with them. It is closely allied, on the other hand, through xanthocone, to the ruby silvers, from which, in a chemical point of view, it should be altogether removed. (4.) The usually adopted chemical group of "oxides" includes a considerable number of minerals, which appear to have little or no natural connexion with one another. Red zinc ore, corundum, magnetic iron ore, cassiterite, and minium, for instance, can scarcely be said to have two characters of any importance in common; and yet, with many other equally opposite substances, the chemical classification places them together, and, indeed, almost side by side.

It seems hopeless, therefore, in the present state of chemical theory to attempt to found a successful mineral classification on chemical grounds alone; and although a perfectly satisfactory distribution of minerals may at present be unattainable by any method, yet it can only be by the employment of every character capable of shedding light on the true nature of minerals and their mutual relations and affinities, that we can ever hope to accomplish this desirable end. The greater the number of physico-chemical classifications, the greater the ultimate chance of success: each attempt yielding its quota towards the end in view, and striking out perhaps some new idea for future elaboration. Even utterly unworthy classifications of this kind have their negative merit, in setting before us in a more palpable light the errors which are especially to be avoided. Without further apology, therefore, I will briefly point out the general features of a system which appears to me to fulfil the two-fold object of discrimination and natural grouping as effectually as this object can be fulfilled by any single classification. That some

of its groups—that its leading divisions, indeed—are more or less groups of convenience, cannot be denied; but with our present restricted knowledge, there is no remedy for this.

I propose to adopt four principal divisions or classes; each class including a certain number of families, and each family a certain number of types or groups. The classes consist of—(1), Metals and Thionoids; (2), Oxygen compounds; (3), Haloids; and (4), Carbonoids. The term "thionoid" is used, as a mineralogical term of general convenience, for sulphur and sulphides, selenides, tellurides, antimonides, &c.

In the present note, the first of these classes only will be considered. It comprises the ten following families:—I. Ferroids; II. Pyrites; III. Blends; IV. Glances; V. Argyroids; VI. Platinoids; VII. Arsenoids; VIII. Antimony Glances; IX. Ruby Blends; X. Sulphoids. A certain natural sequence has been aimed at in the collocation of these families, and more especially in that of their included types and species. The linear arrangement necessarily presents an insurmountable obstacle to any great success in this particular view; but it is hoped that, with a few manifest and unavoidable exceptions, each species will be found more or less related to that which immediately precedes, and to that which immediately follows it; and this, independently of the distribution of the species into types and families. Many interesting relations, however, could only have been exhibited by a parallel system of arrangement.

I. Ferroids.

Native iron, native nickel, native copper.

II. Pyrites.

1. Chalkopyrite Type:—Dimetric.

Chalkopyrite Cu²S₂, Fe²S³; cubanite (monometric?) Cu²S₂, Fe²S³ + 2FeS.

2. Phillipsine Type:—Monometric.

Phillipsine* (Buntkupfererz) 3Cu²S₂, Fe²S³.

3. Stannine Type:—Monometric.

Stannine 2Cu²S₂, SnS² + 2FeS, SnS². Cubanite, phillipsine, and stannine might perhaps be placed under one and the same type. By simplifying the formule, the first shows RS, the second RS + 2R, the third RS. This latter is also the proportional composition of chalkopyrite.

4. Placodine Type:—Monoclinic.

Placodine Ni⁴As.

* Phillipsite, Beudant, 1830.
5. **Nickeline Type**: — Hexagonal.
Nickeline Ni²As; breithauptite Ni²Sb; millerite NiS. (Appendix; eisen-nickelkies.)

6. **Pyrrhotine Type**: — Hexagonal.
Pyrrhotine 5FeS + Fe²S³? This species should perhaps be placed under the preceding type.

7. **Linnaeite Type**: — Monometric.
Linnaeite (Ni, Co, Fe)S + (Ni, Co, Fe)²S³; skutterudite Co²As³; (amoibite; grünauite).

8. **Marcasite Type**: — Trimetric.
Chloanthite, NiAs; mohsine* Fe As? (or R⁴As³); glaucodot (Co, Fe), S² + (Co, Fe), As; mispickel FeS² + FeAs, or with part of the Fe replaced by Co (danaite); markasite Fe S².
The linear arrangement breaks, in a degree, the relations existing between the members of this, and those of the following type. The two should be taken in parallel order.

9. **Pyrites Type**: — Monometric.
Hartmannite (ullmannite) Ni S² + Ni (Sb, As); disomose† Ni S² + Ni As; rammelsbergite Ni As; smaltine Co As; cobaltine CoS² + Co As; pyrites FeS².

**III. Blendes.**

1. **Hauerite Type**: — Monometric.
Hauerite ZnS² (isomorphous with pyrites).

2. **Blende Type**: — Monometric.
Blende ZnS; voltzine 4ZnS + ZnO; alabandine MnS. This type is related both to the pyrites type of the preceding family, and to the galena type of the Glances; but more strongly to the former.

3. **Greenockite Type**: — Hexagonal.
Greenockite CdS.

**IV. Glances.**

1. **Chalkosine Type**: — Trimetric (D : D about 120°).
Discrase (Antimonsilber) Ag⁴Sb; sternbergite AgS + 2Fe²S³;

* Mohsine, Chapman 1843. Subsequently named leucopyrite by Dana, and lölingite by Haidinger. The mineral was first distinguished by Mohs.
† Disomose, Beudant 1830. Subsequently named gersdorffite by Haidinger.
chalkosine* $\text{Cu}_2\text{S}$; berzeline $\text{Cu}_2\text{Se}$; stromeyerine $\text{Cu}_2\text{S} + \text{AgS}$; eukairite $\text{Cu}_2\text{Se} + \text{AgSe}$.

2. Covellite Type:—Hexagonal.
Covellite $\text{CuS}$; (riolite $\text{AgSe}_2$)?

3. Molybdenite Type:—Hexagonal.
Molybdenite $\text{MoS}_2$.

4. Elasmose Type:—Dimetric.
Elasmose $\text{Pb, Au, Te, S}$.

5. Sylvanite Type:—Trimetric.
Sylvanite, $\text{Ag, Au, Te}$.

6. Galena Type:—Monometric.
Argyroide $\text{AgS}$; naumannite $\text{AgSe}$; galena $\text{PbS}$; cuproplumbite $\text{Cu}_2\text{S} + 2\text{PbS}$; clausthullite $\text{PbSe}$; zorgite ($\text{Pb, Cu}_2$), $\text{Se}$; lerbachite, $\text{Pb Se, Hg Se}$; onofrite $\text{HgS, HgSe}$; hessite $\text{Ag Te}$; petzite ($\text{Ag, Au}$) $\text{Te}$; altaite $\text{Pb Te}$.

ARGYROIDES.

Monometric.

Native lead; mercury; amalgam ($\text{Ag Hg}$); silver; gold.

VI. PLATINOIDS.

Monometric and Hemi-hexagonal.

Native platinum; palladium; platinum-iridium; osmium-iridium.

VII. ARSENIOIDS.

Hemi-hexagonal.

Native bismuth; arsenic; antimony; tellurium; tetradyminite ($\text{Bi, Te}$).

VIII. ANTIMONY GLENCES.

1. Stibine Type:—Trimetric.
Stibine $\dagger \text{SbS}_3$; bismuthine, $\text{BiS}_3$.

* Chalkosine, Beudant 1830; redruthite, Miller 1852.
† Elasmose, Beudant 1830; nagyagite, Haidinger.
‡ Stibine, Beudant 1830. Haidinger’s subsequent name of antimonite is altogether inadmissible, as this is the generic appellation for an entire class of chemical compounds—combinations of antimonious acid with bases. The names bestowed by this eminent mineralogist on many other minerals are unfortunately, open to a similar objection.
2. **Bournonite Type:** —Trimetric (D : D 96°–102°).

- Bournonite 3(Cu²Pb)S₃, SbS³; aikinite* 3(Cu², Pb)S, BiS³; wolchite; wolfsbergite Cu₃S, SbS³; jamesonite 3PbS, 2SbS³.
- Boulangerite; kobellite; heteromorphite (plumosite); berthierite.

3. **Zinkenite Type:** —Trimetric (D : D about 120°).

- Psaturose 6AgS, SbS³; zinkenite PbS, SbS³; geocronite (schulzite) 5PbS, (Sb, As)S³. This type has many relations to the chalkosine type in the family of the Glances.

4. **Panabase Type:** —Monometric.

- Dufreynosite 2PbS, AsS³; steinmannite xPbS, SbS³; panabase (fahlerz) 4(Pb, Fe, Zn, Cu², Ag, Hg)S + (Sb, As) S³; tennantite 4RS, AsS³.

5. **Freislebenite Type:** —Monoclinic (Miller).

- Plagionite 4PbS, 3SbS³; freislebenite x(Pb, Ag)S, xSbS³.

6. **Polybasite Type:** —Hexagonal.

- Polybasite 9(Ag, Cu²)S, (Sb, As)S³? or 2Cu²S + 6AgS, (Sb, As)S³?

**IX. Ruby Blendses.**

1. **Argyrythrose Type:** —Hexagonal.

- Argyrythrose 3AgS, SbS³; proustite 3AgS, AsS³; xanthocone; cinnabar HgS.

2. **Miargyrite Type:** —Monoclinic.

- Fire-blende; miargyrite AgS, SbS³.

**X. Sulphoids.**

1. **Sulphur Type:** —Trimetric.

- Native sulphur; selen sulphur; orpiment AsS³.

2. **Realgar Type:** —Monoclinic.

- (Monoclinic sulphur); realgar AsS².

* Aikinite, Chapman 1843. Subsequently named aciculite by Mr. Nicol, and patrinite by Haidinger. Where the name of any distinguished cultivator of the science has been substituted for a prior name having reference merely to the locality or general properties of the mineral, courtesy may recognise the substitution; but in all other cases, the original name—unless manifestly inappropriate—should undoubtedly be retained. The nomenclature of mineralogy has much need of a work of purification like that lately rendered to a large portion of paleontological science by M. Alcide d’Orbigny. (*Prodrome de Paléontologie stratigraphique universelle des Animaux Mollusques et Rayonnés.*)
Mr. A. Cayley on a Theorem for

3. Kermesome Type:—Monoclinic.

Kermesome $2\text{SbS}_3 + \text{SbO}_3$. This is not the exact formula of the "Kermes" of chemistry—hence the slight alteration in the name. The true position which the kermesome should occupy in the system has yet to be ascertained. It is placed here quite provisionally.

XXV. On a Theorem for the Development of a Factorial.

By A. Cayley, Esq.*

The theorem to which I refer is remarkable for the extreme simplicity of its demonstration. Let it be required to expand the factorial $x-a x-b x-c \ldots$ in the form

$$x-a x-\beta x-\gamma \ldots + Bx-\alpha x-\beta \ldots + Cx-\alpha \ldots + D \ldots &c.$$

We have first

$$x-a = x-a + \alpha -a.$$

Multiply the two sides of this by $x-b$; but in multiplying by this factor the term $x-a$, write the factor in the form $x-\beta + \beta -b$; and in multiplying the term $\alpha -a$, write the factor in the form $x-\alpha + \alpha-b$; the result is obviously

$$x-a x-b = x-a x-\beta + (\alpha -a + \beta -b)x-\alpha + \alpha-a -b.$$

Multiply this by $x-c$, this factor being in multiplying the quantity on the right-hand side written successively under the forms $x-\gamma + \gamma -c$, $x-\beta + \beta -c$, $x-\alpha + \alpha-c$; the result is

$$x-a x-b x-c = x-a x-\beta x-\gamma$$

$$+ (\alpha -a + \beta -b + \gamma -c)x-\alpha x-\beta$$

$$+ (\alpha-a \alpha-b + \alpha-a \beta-c + \beta-b \beta-c)(x-\alpha)$$

$$+ (\alpha -a)(\alpha-b)(\alpha-c),$$

which may be thus written,

$$(x-a)(x-b)(x-c) = (x-a)(x-\beta)(x-\gamma)$$

$$+ [\alpha, \beta, \gamma] x-a x-\beta + [\alpha, \beta, \gamma] x-a + [\alpha, \beta, \gamma].$$

Consider, for instance,

$$[\alpha, \beta, \gamma] = [\alpha-a \alpha-b + \alpha-a \beta-c + \beta-b \beta-c].$$

Then, paying attention in the first instance to the Greek letters only, it is clear that the terms on the second side contain the

* Communicated by the Author.
combinations two and two with repetitions of the Greek letters \( \alpha, \beta, \) and these letters appear in each term in the alphabetical order. Each such combination may therefore be considered as derived from the primitive combination \( \alpha, \) \( \alpha \) by a change of one or both of the \( \alpha \)'s into \( \beta; \) and if we take instead of the mere combination \( \alpha, \) \( \alpha \) the complete first term \( \alpha-a \) \( \alpha-b, \) and simultaneously with the change of the \( \alpha \) of either of the factors into \( \beta \) make a similar change in the Latin letter of the factor, we derive from the first term the other terms of the expression on the right-hand side of the expression. It is proper also to remark, that, paying attention to the Latin letters only, the different terms contain all the combinations two and two without repetitions of the letters \( a, b, c. \) The same reasoning will show that

\[
\begin{align*}
&x-a \ x-b \ x-c \ x-d=x-\alpha \ x-\beta \ x-\gamma \ x-\delta \\
&+ [\alpha, \beta, \rho, \delta] \ x-\alpha \ x-\beta \ x-\gamma \\
&+ [\alpha, \beta, \rho] \ x-\alpha \ x-\beta \\
&+ [\alpha, \beta] \ x-\alpha \\
&+ [\alpha, \beta, \rho, \delta] \ x-\alpha \ x-\beta \ x-\gamma \ x-\delta,
\end{align*}
\]

where, for instance,

\[
\begin{pmatrix}
\alpha, \beta \\
a, b, c, d
\end{pmatrix}_3 = (\alpha-a)(\alpha-b)(\alpha-c) \\
+ (\alpha-a)(\alpha-b)(\beta-d) \\
+ (\alpha-a)(\beta-c)(\beta-d) \\
+ (\beta-b)(\beta-c)(\beta-d), \ &c.
\]

It is of course easy, by the use of subscript letters and signs of summation, to present the preceding theorem under a more condensed form; thus writing

\[
\begin{pmatrix}
\alpha_1, \alpha_2 \ldots \alpha_r \\
a_1, a_2 \ldots a_r \ldots a_{r+s}
\end{pmatrix}_{s+1} = S_i \left\{ \frac{\alpha_{k_s} - a_{k_s+s}}{\alpha_{k_s-1} - a_{k_s-1+s-1} \ldots a_{k_0} - a_{k_0}} \right\}
\]

where \( k_s, k_{s-1}, \ldots, k_0 \) form a decreasing series (equality of successive terms not excluded) of numbers out of the system \( r, r-1 \ldots 3, 2, 1; \) the theorem may be written in the form

\[
x-a_1 \ x-a_2 \ldots x-a_p = S_{q_0} p \left\{ \begin{pmatrix}
\alpha_1, \alpha_2 \ldots \alpha_{p-q+1} \\
a_1, a_2 \ldots \ldots a_{p-q}
\end{pmatrix}_{p-q} \right\}
\]

but I think that a more definite idea of the theorem is obtained
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through the notation first made use of. It is clear that the above theorem includes the binomial theorem for positive integers, the corresponding theorem for an ordinary factorial, and a variety of other theorems relating to combinations. Thus, for instance, if \( C_q(a_1 \ldots a_p) \) denote the combinations of \( a_1 \ldots a_p \) \( q \) and \( q \) together without repetitions, and \( H_q(a_1 \ldots a_p) \) denote the combinations of \( a_1 \ldots a_p, q \) and \( q \) together with repetitions, then making all the \( \alpha \)'s vanish,

\[
x-a_1 \ldots x-a_p = S_{q_0}^p(-)^qC_q(a_1 \ldots a_p)x^{p-q};
\]

and

\[
(x-a)^p = S_{q_0}^p(-)^qC_q(a, a \ldots \text{plures})x^{p-q} = S_{q_0}^p(-)^q[p]_q^q[\alpha]^q[\alpha]^{p-q},
\]

the ordinary binomial theorem for a positive and integral index \( p \).

So making all the \( \alpha \)'s vanish,

\[
x^p = S_{q_0}^pH_q(\alpha_1 \ldots \alpha_{p-q+1})x^{p-q};
\]

If \( m \) be any integer less than \( p \), the coefficient of \( x^m \) on the right-hand side must vanish, i. e. we must have identically

\[
0 = S_{q_0}^{p-m}(-)^qC_{p-q-m}(\alpha_1 \alpha_2 \ldots \alpha_{p-q})H_q(\alpha_1 \alpha_2 \ldots \alpha_{p-q+1}).
\]

So also

\[
C_{p-m}(a_1 a_2 \ldots a_p) = S_{q_0}^{p-m}(-)^qC_{p-q-m}(\alpha_1 \alpha_2 \ldots \alpha_{p-q})H_q(\alpha_1 \alpha_2 \ldots \alpha_{p-q+1}).
\]

Suppose

\[
a_1 = 0, a_2 = 1 \ldots a_p = p-1; \alpha_1 = k, \alpha_2 = k-1, \ldots \alpha_p = k-p+1,
\]

then

\[
[k, k-1 \ldots k-p+q] = [p]_q^q[\alpha]_q^{p-q}
\]

\[
x-a_1 \ldots x-a_{p-q} = [x]^{p-q}.
\]

And hence

\[
[x+k]^p = S_{q_0}^p[p]_q^q[\alpha]_q^q[x]^{p-q},
\]

the binomial theorem for factorials.

A preceding formula gives at once the theorem

\[
H_q(0, 1 \ldots p-q) = \frac{1}{[p-q]_q^{p-q}}\Delta^{p-q} 0^p.
\]

It may be as well to remark, with reference to a demonstration frequently given of the binomial theorem, that in whatever way the binomial theorem is demonstrated for integer positive indices,
On a Method of Estimating Minute Quantities of Iodine. 185
it follows from what has preceded that it is quite as easy to de-
monstrate the corresponding theorem for the factorial \([m]\). But
the theorem being true for the factorial \([m]\), it is at once seen
that the product of the series for \((1 + x)^m\) and \((1 + x)^n\) is iden-
tical with the series for \((1 + x)^{m+n}\), and thus it becomes unnec-
essary to employ for the purpose of proving this identity the so-
called principle of the permanence of equivalent forms; a
principle which, however, in the case in question may legiti-
mately be employed.

XXVI. Note on a Generalization of the Binomial Theorem.
By A. Cayley, Esq.*

THE formula (Crelle, vol. i. p. 367) for the development of
the binomial \((x+a)^n\), but which is there presented in a
form which does not put in evidence the law of the coefficients,
is substantially equivalent to the theorem given by me as one of
the Senate House Problems in the year 1851, and which is as
follows:—
"If \(\{a+b+c+\ldots\}\) denote the expansion of \((a+b+c+\ldots)^n\),
retaining those terms \(Na\beta^\gamma\delta^d\ldots\) only in which \(b+c+d+\ldots p-1,\)
c+d+ \ldots \(p-2, \&c., \) then
\[
x^n = (x+a)^n - \frac{n}{1} \{a\} \frac{1}{1} (x+a+b)^{p-2} + \frac{n}{1} \{a+b\} \frac{1}{2} (x+a+b+\gamma)^{n-2} - \frac{n}{1} \frac{1}{2} \frac{1}{3} \{a+b+\gamma\} \frac{3}{2} (x+a+b+\gamma+\delta)^{n-3} + \&c."
\]
The theorem is, I think, one of some interest.

XXVII. On a quick Approximative Method of Estimating Minute
Quantities of Iodine. By Thornton J. Herapath†.

THE estimation of minute quantities of iodine is not always
accomplished without difficulty. I have, however, recently
discovered a simple means of effecting this object, which I think
is worthy of being generally known. The method in question
is based on the mode of analysing silver coin proposed by Gay
Lussac, and on that employed by Mr. Horsford for the determina-
tion of lead in potable waters; modifications of which processes
have been since successfully applied to the quantitative estimation
of many other substances. It is the method of graduated solu-
tions. The reagent I use is a salt of palladium, which, as is

* Communicated by the Author.
† Communicated by the Author.
well known, produces in solutions of iodine and the soluble iodides a brown or brownish-black precipitate of iodide of palladium. When, however, the quantity of iodine is small, the iodide instead of being immediately precipitated, remains suspended in the solution, to which it communicates a brownish tinge, more or less deep according to the proportion of iodine present. Consequently, then, by ascertaining the depth of tint of such a solution, by comparing it with that of standard solutions properly prepared with known quantities of iodine, the proportion of the latter substance that is contained in the matters tested may be ascertained with the greatest exactness.

In an investigation of this kind, therefore, the first thing to be done is to prepare certain standard solutions. 1·309 grain of perfectly pure iodide of potassium—which is equivalent to one grain of iodine—is accordingly dissolved in 10,000 grains of water. This constitutes solution No. 1, and contains a little less than 0·01 gr. per cent. of iodine. By diluting this with water, other solutions—Nos. 2, 3, 4, 5, 6, &c.—are formed. The iodine in the substance to be tested having been converted into hydriodic acid or a soluble iodide, the latter is introduced into a colorimeter and diluted with water to the necessary degree,—that is to say, until it occupies exactly 100, 500, 1000, 10,000 or more water grain measures; the precaution being taken to first add the palladium solution drop by drop until no deepening of colour ensues. The tint is then compared with that of the standard solutions before mentioned contained in tubes or phials of similar diameter, in which certain known quantities of iodine are contained in the same bulk of water. Though it might rarely be possible to identify it with either one of two solutions in the scale, there can be no difficulty in deciding between which two it should fall, or nearest to which one of two it should be placed.

These standard solutions, it should be observed, may be sealed up in glass tubes, and thus rendered available in future investigations; the only precaution necessary to be taken in such cases being to well agitate the contents of the tubes, so as to again get the precipitated iodide of palladium in suspension in the fluid. Operating in this way, it is possible to estimate the $\frac{1}{20,000}$-th of a grain of iodine with the greatest readiness. It is sometimes preferable to employ but one standard solution. The proportion of iodine in the liquid analysed is then determined by measuring the volume of water that is required to lighten the tint, so as to render it identical with that of the normal solution or vice versa.

Mansion House, Old Park, Bristol,
July 27, 1853.
XXVIII. On the Constitution of the Colouring Matters of Madder.  
By Edward Schunck, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the Annales de Chimie et de Physique, 3rd sér. vol. xxxvi., M. Laurent has published his views on the constitution of several series of bodies which have been made the subjects of investigation by other chemists. The first series subjected by him to criticism comprises the bodies obtained by me from madder, some of them being substances previously known and others being new; some contained as such in the plant, others products of decomposition. The chief object of my investigation of madder was to ascertain the nature of the chemical processes which take place when madder is subjected to various agencies; and secondly, to discover the connexion, if any, subsisting between the different substances of the plant which are endowed with colour, and, if possible, to trace them back to some common origin, so as to be able to exhibit them from a common point of view. The direction in which the investigation ought to be conducted in order to yield results of importance was first indicated by Berzelius, who, while confessing his inability, notwithstanding the time he had spent on experimenting to clear up this complicated subject, expressed his conviction that the colouring matters of madder possess great analogy of composition, and may perhaps be mutually convertible. With what success my endeavours have been attended I do not pretend to say; but I do maintain, without hesitation, that my view of the composition of the bodies examined by me explains their formation and mutual relations more simply and naturally than does M. Laurent's; and I should have replied much sooner to his objections had I not preferred waiting until after the publication of the second part of my paper "On Rubian and its Products of Decomposition*," in which a number of facts are mentioned which will go further in confirming the views maintained in the first part than any mere statement of opinions could.

I have described under the name of Rubian a substance contained in madder of a very complex nature and high atomic weight, which very readily undergoes decomposition. This substance by its decomposition gives rise to the formation, not only of alizarine, but of a number of other bodies, most of which possess, either in a combined or uncombined state, a red colour. In fact, all the bodies endowed with colour which have been obtained

* Philosophical Transactions, part 1, 1853; Philosophical Magazine for June 1853.
from madder, either by myself or others, may be produced from rubian; and I think, therefore, I am justified in considering this substance as the link which unites them all together in one system. No formula can be proposed for any one of them which is not in accordance with that of the parent substance. By the action of sulphuric or muriatic acid rubian is decomposed into alizarine, verantine, rubiretine, rubianine and sugar, in accordance with the following equations:—

Rubian.  Alizarine.
1. \( C^{36} H^{34} O^{30} = 4C^{14} H^5 O^4 + 14HO. \)

2. \( C^{36} H^{34} O^{30} = 2C^{14} H^5 O^6 + 2C^{14} H^6 O^4 + 12HO. \)

Rubian.  Sugar.  Rubianine.
3. \( C^{36} H^{34} O^{30} + 9HO = 2C^{12} H^{12} O^{12} + C^{32} H^{19} O^{15}. \)

By decomposition with alkalies, rubian yields the same products with the exception of rubianine, which is replaced by a similar body, rubiadine, differing from the former by the elements of 7HO.

Rubian.  Sugar.  Rubiadine.
4. \( C^{36} H^{34} O^{30} + 2HO = 2C^{12} H^{12} O^{12} + C^{32} H^{12} O^{8}. \)

By the action of a peculiar fermentative substance contained in madder, which I have called erythrozym, rubian also undergoes complete decomposition. The products are in the main the same, but the rubianine and rubiadine of the preceding processes are replaced by two substances of very similar properties, differing from one another and from the two former in composition by the elements of HO. These two bodies I have called rubiafine and rubiaigne.

Rubian.  Sugar.  Rubiafine.
5. \( C^{36} H^{34} O^{30} + 3HO = 2C^{12} H^{12} O^{12} + C^{32} H^{15} O^{9}. \)

6. \( C^{36} H^{34} O^{30} + 4HO = 2C^{12} H^{12} O^{12} + C^{32} H^{14} O^{10}. \)

For rubiafine and rubiac acid, which I have proved to be derivatives of rubiafine, I have adopted the formule \( C^{32} H^{11} O^{10} \) and \( C^{32} H^{9} O^{17} \; \text{for rubiacate of potash,} \; C^{32} H^{8} O^{16} + KO. \)

Now to these formule M. Laurent has brought forward objections, and he proposes new ones. For rubian he proposes the formula \( C^{32} H^{18} O^{18} \; \text{for alizarine,} \; C^{10} H^9 O^9 \; \text{for rubiretine,} \; C^{10} H^4 O^3 \; \text{for rubianine,} \; C^{20} H^{11} O^9 \; \text{for verantine,} \; C^{20} H^7 O^7 \; \text{for rubiafine,} \; C^{18} H^5 O^6 \; \text{for rubiac acid,} \; C^{18} H^5 O^9 \; \text{and for rubiacate of potash,} \; C^{18} H^{43} O^{81} + \frac{1}{2} KO. \) The following equation would explain, according to M. Laurent, the formation of alizarine and rubiretine from rubian:—
the Colouring Matters of Madder.

C₁₀₂ H₁₈ O₁₈* = C₁₂ H₁₂ O₁₂ + C₁₀ H₃ O₃ + C₁₀ H₄ O₃ - 2aq.

In this equation verantine may be substituted for the alizarine, since, according to M. Laurent, the formula of the former, C₂⁰ H₇ O₇, differs merely by HO from the double of that of alizarine, C₁₀ H₃ O₃. In the same manner, the rubiretine, by adopting M. Laurent's formulæ, may be replaced in this equation by rubianine.

M. Laurent has prefaced his observations by the enunciation of several propositions or laws, as, for instance, "In all organic substances the number of atoms of each simple substance is even," &c. It is in accordance with these propositions that he has made the alterations in the formulæ proposed by me.

It will be conceded by most chemists, that the chemical formulæ of a body is of no interest or importance except as exhibiting the relation in which the body stands to others. Indeed we may go further, and say that in many cases, especially those of bodies of high atomic weight, it is immaterial for most purposes whether the formula be absolutely correct, provided it be relatively so. What is required of a chemical formula is, that it should be in accordance with the analysis, that it should show the connexion subsisting between the composition of the body and that of others, and that it should be in harmony with what we know of its mode of formation and its products of decomposition. Whether it is in accordance with any empirical law regarding the number and arrangement of atoms represented by it, or not, is quite immaterial; for the object of chemical investigation, of which formulæ shortly express the results, is not the discovery of mathematical laws, but of chemical facts. Now the formulæ proposed by M. Laurent for the substances described by me do not fulfill any of the conditions required. They do not agree as well as mine with the results of analysis, and they quite fail to explain the mutual interdependence which I have shown to exist between these various bodies, as well as the nature of the processes whereby one original substance splits up into so many derivatives. I readily concede, that, looking at each substance alone, it would be possible to discover a number of formulæ agreeing better than mine with the results of analysis; but it is impossible, as I have shown, to consider the composition of any one of them out of connexion with that of the others; it is necessary to look at them from a point of view common to all, to construct some system which shall include them all, even by

* This formula has been given three times, or else it might be concluded that there was a misprint in the numbers, as the equation is impossible as it stands.
sacrificing somewhat of the minute agreement usually expected between experiment and theory. Those conversant with this species of research will indeed expect some discrepancies between the calculated and experimental composition of substances, a sufficient quantity of which for the purposes of analysis can seldom be obtained without a great expenditure of time and labour.

During my examination of alizarine, I discovered that this substance is converted by the action of nitric acid into a crystallized acid, which I called alizaric acid, and to which I attributed the formula $C_{14}H_5O_7$. MM. Laurent and Gerhardt have proved that this acid is identical with Laurent’s phtalic acid, and I am obliged to them for the correction; but when M. Laurent proceeds to say, that, having made this discovery, they held in their hands the key to all the difficulties of the subject, he is very much mistaken. The idea of drawing any inferences regarding the constitution of an organic substance from the nature of its products of decomposition with nitric acid, is one worthy only of the infancy of organic chemistry. Conclusions as to the composition of cane-sugar, for instance, drawn from a consideration of that of oxalic acid, would not be accepted at the present day. Nevertheless the formation of phtalic acid as a product of decomposition of alizarine is sufficient in itself to prove that M. Laurent’s view of the composition of rubian is perfectly untenable. M. Laurent considers rubian to be a compound of sugar, alizarine and rubiretine minus water; just as heliocidine may be regarded as a compound of sugar, hydruret of salicyle, and saligerine minus water. Now if this view were correct, on acting on rubian with nitric acid, the sugar would be converted into oxalic acid, the alizarine would yield phtalic acid; and the rubiretine, which is not affected by dilute nitric acid, nor in any great degree even by concentrated acid, would be obtained as an insoluble residue. Now on decomposing rubian with nitric acid, the only solid product obtained is phtalic acid. Not a trace of oxalic acid is contained in the liquid, not a trace of matter insoluble in cold nitric acid is formed. Therefore neither sugar nor rubiretine can be considered as constituents of rubian. The same experiment proves that neither rubianine nor verantine exist ready formed in rubian; for as neither of them is decomposed by nitric acid (rubianine being soluble in boiling nitric acid, and crystallizing out again on the acid cooling), they would, if they did, be obtained after the action is completed in the shape of an insoluble residue.

Pursuing his favourite idea that phtalic acid is the bond which unites all the compounds derived from madder, M. Laurent arrives at the following conclusions. He takes it for granted that alizarine, by the action of nitric acid, is changed into phtalic
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acid and oxalic acid; and as the formula of rubiretine differs from that of alizarine, according to him, merely by one equivalent of H, it may easily be conceived that, when exposed to the influence of oxidation, it may yield the same products:

\[ \text{C}_1^6 \text{H}_3 \text{O}_3^+ + \text{HO} + 4\text{O} = \text{C}_8 \text{H}_3 \text{O}_4^+ + \text{C}_2 \text{HO}_4^+ \].

\[ \text{C}_1^6 \text{H}_4 \text{O}_3^+ + 5\text{O} = \text{C}_8 \text{H}_3 \text{O}_4^+ + \text{C}_2 \text{HO}_4^+ \].

In like manner purpurine, the existence of which M. Laurent still admits, and rubiacine may, when subjected to the action of nitric acid, resolve themselves into the same ultimate products, since purpurine may be considered as an amide (to use M. Laurent's expression) of phtalic acid and alizarine:

\[ \text{C}_1^8 \text{H}_6 \text{O}_6^+ = \text{C}_8 \text{H}_3 \text{O}_4^+ + \text{C}_1^0 \text{H}_4^0^3 - \text{HO} \].

\[ \text{C}_1^8 \text{H}_5 \text{O}_6^+ = \text{C}_8 \text{H}_3 \text{O}_4^+ + \text{C}_1^0 \text{H}_3^0^3 - \text{HO} \].

Now every one of these conclusions is entirely hypothetical, their only basis being an accidental correspondence between the formulæ invented by M. Laurent. In the first place, I may remark that alizarine, by the action of nitric acid, yields phtalic acid only, without a trace of oxalic acid. It is certainly true, that, in acting on madder or garancine with nitric acid, oxalic acid is also obtained as one of the products; but its formation is due to other constituents of the root, not to the alizarine. In this respect, therefore, the presumed analogy between alizarine and chloronaphthalic acid does not hold good. As regards rubiretine and rubiacine, anyone reading my description of these bodies with attention, will, I have little doubt, arrive at the conclusion that their conversion into phtalic acid by means of nitric acid is extremely improbable, if not impossible. Rubiretine is not sensibly affected by boiling dilute nitric acid. Concentrated nitric acid in a boiling state changes it slowly into a yellow substance, which is insoluble in water, and very little soluble in alcohol. Rubiacine dissolves in boiling dilute nitric acid, and crystallizes out again unchanged on the solution cooling. Concentrated nitric acid decomposes it with difficulty. Moreover, by the action of persalts of iron, rubiacine passes into rubiacic acid, whereas alizarine by the same agency is changed into phtalic acid. As regards the so-called purpurine, I think I have proved synthetically as well as analytically that this substance is a mixture in variable proportions of alizarine and verantine, and that therefore its forming phtalic acid when decomposed by nitric acid is not remarkable. I may state, nevertheless, for the information of those who are still inclined to believe in the
existence of this colouring matter, that by acting on it with nitric acid no rubiretine is formed, but that the second product of decomposition, which is obtained as an insoluble yellow powder, consists of verantine only.

From the circumstance that rubian as well as alizarine, when decomposed by nitric acid, yield no other solid product but phtalic acid, and considering that alizarine is in every case a derivative of rubian, I inferred that it would be possible, if not experimentally at least theoretically, to convert rubian entirely into alizarine; that by this conversion none of the carbon of the rubian is eliminated; that the whole of this element enters into the composition of the alizarine formed, and that, in fact, the two bodies differ from one another merely by the elements of water. This conclusion, which I consider to be the most important result of my investigation, derives confirmation from the experiments made by me on the action of ferments on rubian. I have found that, by the action of emulsine on rubian, though only a portion of the latter is decomposed, still that portion yields almost pure alizarine. Again, when the action of erythrozym on rubian is retarded by the addition of some antiseptic substance, such as corrosive sublimate or alcohol, the rubian is entirely decomposed, though much more slowly than without such addition. In its results, however, the action is very different. If the retardation was very great, then the only products of decomposition formed are rubiretine and verantine, without a trace of alizarine, sugar, or any other body. If the action was moderately retarded, then besides rubiretine and verantine, sugar and rubiafine or rubiagine are found among the products of decomposition; but in no case can more than a trace of alizarine be discovered. By the addition, however, of a small quantity of alkali during the process of fermentation, the quantity of alizarine formed is very much increased. From these experiments it must be inferred that the quantity of each product of decomposition which rubian is capable of forming is by no means constant; that rubian may yield, according to circumstances, either alizarine alone, or rubiretine and verantine alone, or rubiafine and sugar alone. The decomposition of rubian, therefore, cannot be represented by one equation, it must be represented by three equations. In one of these equations the rubiafine may be replaced by rubiagine, which is an occasional product of the fermentation of rubian, or by rubiadine when an alkali is the decomposing agent, or by rubianine when the decomposition is effected by acids; for these bodies differ from one another in composition merely by the elements of water; and they are so similar in their properties, that it is evident they must belong to the same class of compounds. To represent the decomposition by one equation, as M. Laurent
has done, is evidently impossible; for no formula of ordinary dimensions can be devised for rubian, which shall include within itself the formulae of its five products of decomposition. If his formula for rubian, \( C^{92} H^{18} O^{18} \), be correct, then his formula for alizarine, \( C^{10} H^{3} O^{3} \), must be incorrect, as the number of atoms of C in rubian must be a multiple of that in alizarine. On the other hand, if his formulae for alizarine, rubiretine, and verantine be admitted, then rubian must contain \( C^{30} \). The only decomposition which M. Laurent's formulae explain is that of rubian into rubianine and sugar, which would then be expressed by the following equation:

\[
\text{Rubianine. Sugar. Rubian.}
\]

\[
C^{20} H^{11} O^{9} + C^{12} H^{12} O^{12} = C^{32} H^{19} O^{17} + 4HO.
\]

Even in this case M. Laurent's formula for rubian requires some alteration.

M. Laurent objects to the equation \( C^{56} H^{34} O^{30} = 4C^{14} H^{5} O^{4} + 14HO \), whereby I explain the formation of alizarine from rubian, because there is no authentic instance of a reaction with fourteen equivalents of water. All that I can say in reply to this objection is, that this must be the first authentic instance, as I am constrained to admit it as an inference from positive facts. M. Laurent has not given any confirmation of his formula for alizarine. Had he, however, subjected a specimen of the pure substance to analysis, he would have found that his formula cannot for an instant be sustained, and that it does not contain the same number of equivalents of hydrogen and oxygen. M. Strecker, who adopts the same formula as M. Laurent, has certainly given one analysis of alizarine; but this analysis was made with only 0·0650 grm. of substance, a quantity too small to admit of any safe inference being drawn from its analysis. The purest specimen of alizarine which I have ever prepared was obtained by the decomposition of rubian with sulphuric acid. The following table shows the composition of this specimen as compared with that found by Strecker and Debus (as an average of three determinations), and with the calculated composition according to the two formulae:

<table>
<thead>
<tr>
<th></th>
<th>Strecker</th>
<th>Debus.</th>
<th>( C^{14} H^{5} O^{4} )</th>
<th>( C^{10} H^{3} O^{3} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>C</td>
<td>69·37</td>
<td>68·4</td>
<td>69·97</td>
<td>68·96</td>
</tr>
<tr>
<td>H</td>
<td>4·07</td>
<td>3·79</td>
<td>4·13</td>
<td>3·45</td>
</tr>
<tr>
<td>O</td>
<td>26·56</td>
<td>27·24</td>
<td>26·45</td>
<td>27·59</td>
</tr>
</tbody>
</table>

It will at once be seen that the numbers found in my analysis differ so widely from those calculated according to the formula \( C^{10} H^{3} O^{3} \), that the latter cannot possibly be accepted as the true one. An excess of 0·6 per cent. of hydrogen, as this formula would presuppose, is unusually large; an excess of 0·4 per cent.
of carbon is never obtained in the analysis of a pure crystallized substance. I have subjected alizarine in various states of purity so repeatedly to analysis, that I may be allowed to express a decided opinion on the question of its composition. Now I maintain without any hesitation, that the purer the alizarine the more nearly will its composition correspond to my formula, and that only in an impure state does its composition approach that ascribed to it by M. Laurent. The impurity which usually accompanies alizarine consists of verantine. An admixture of the latter, even in considerable quantity, does not deprive alizarine of its crystalline form; it merely imparts to the crystals a red colour, and causes it to become more or less soluble in alum liquor. It is only with great difficulty that alizarine can be deprived of the last trace of verantine accompanying it. The formula of verantine being $C^ {14} H^5 O^5$, it will be seen that, in any mixture of this substance and alizarine, the quantity of carbon and hydrogen must be lower than in pure alizarine with the formula $C^{14} H^{5} O^{4}$. No specimen of alizarine can be considered as absolutely pure which contains less than 69 per cent. of carbon and less than 4 per cent. of hydrogen.

M. Laurent objects to the equation—

$$C^{56} H^{34} O^{30} + 9HO = 2C^{12} H^{12} O^{12} + C^{32} H^{19} O^{15},$$

or

$$\Lambda = 2B + C - 9Aq,$$

whereby I explain the formation of rubianine and sugar from rubian, as being without any analogy in science. Nevertheless, he accepts without hesitation M. Streeker's view of the decomposition of tannic acid into gallic acid and sugar, which is represented by the equation—

$$C^{40} H^{18} O^{26} + 6HO = C^{12} H^{12} O^{12} + 4C^{7} H^{3} O^{5},$$

or

$$\Lambda = B + 4C - 6Aq,$$

an equation which does not differ very widely in its form from the one just given. And surely no assumption which I have made can be so extravagant as some of M. Laurent's; as, for instance, when he supposes rubiate of potash to contain half an equivalent of potash, a view which is and always will remain without any analogy.

It is remarkable, that of the whole series of bodies, the only one whose formula M. Laurent does not change is that which is the most indeterminate in its nature, viz. the sugar. Now instead of being the centre or fixed point of the system, this body is the unknown quantity, whose value can only be determined when that of all the others has been discovered. In itself
this substance presents no characteristic properties to observation. It is a perfectly uncrystalline, glutinous, neutral substance, hardly possessing a sweet taste, and not capable of entering into combination with any other body. There would be absolutely no means of ascertaining whether its formula is $C^{12}H^{12}O^{12}$, $C^{14}H^{14}O^{14}$, or $C^{2}H^{2}O^{2}$, if it did not occupy a fixed place as part of a system of bodies. It is the last member of the series whose position can be determined; but that of the others being known, it falls at once into the only place it can occupy. The formula of rubian being given, by its decomposition into alizarine and water, and into rubiretine, verantine and water, it only remains to ascertain the formula of the group of bodies comprising rubianine, rubiadine, rubiafine and rubiagine, which mutually replace each other according to circumstances, as I mentioned above. Now one of these bodies, rubiafine, gives by simple oxidation with persalts of iron, rubiaic acid, without losing any carbon, the rubiaic acid being reconvertible into rubiafine by means of reducing agents. In rubiacate of potash, the potash is to the carbon as 1 : 32; consequently all the bodies of this group must contain 32 equivalents of carbon, and the formulae then only differ from one another by certain multiples of HO. Rubianine, for instance, must be $C^{32}H^{19}O^{15}$; and if this be deducted from the formula for rubian, after adding to the latter 9HO, the remainder will be $C^{24}H^{24}O^{24}$, which represents two equivalents of sugar. I fear it is through no merit of mine that M. Laurent has been led to adopt the same view of the constitution of this substance; for if this were the first instance on record of the formation of sugar from other bodies, we should without doubt have been entertained with disquisitions by chemists of M. Laurent's school on all kinds of possible formulae which might be attributed to it. Nevertheless there is no instance of the formation of sugar from bodies of a more complex nature, as in the decomposition of salicine and phloridzine, in which its formula has not resulted from a similar process of reasoning to mine, instead of from direct experiment.

As regards rubiacine, M. Laurent is not to be blamed for adopting a formula which leaves unexplained its formation from rubian, as he was not aware of its being a derivative of the latter substance. The relation, however, in which rubiacine stands to rubiaic acid fixes the number of equivalents of carbon which it contains at 32, and consequently determines the formula. Concerning the true composition of rubiaic acid there can be no doubt. M. Laurent's formula for the potash salt is impossible; for without regarding the absurdity of assuming it to contain half an equivalent of base and a fraction of an equivalent of oxygen, it presupposes an error in the determination of the car-

O 2
bon amounting to 1 per cent., and in that of the potash of 1·5 per cent., errors which can hardly occur in the analysis of a pure, crystallized potash salt.

I must apologise for the length of this communication. But though the subject may not be of great importance, still correct views as to the proper method of investigating the processes of nature, and a correct standard whereby to measure the labours of others, are matters of importance. In chemistry, as in every other natural science, the groundwork of the system must consist of facts. From correctly observed phenomena, laws will develope themselves spontaneously. But if, in obedience to certain assumed à priori laws, we abandon the safe ground of observation and experiment, we embark on a boundless ocean without star or compass to guide us.

I am, Gentlemen,
Belfield Hall, Roeadale,
August 12, 1853.

Your obedient Servant,
E. SCHUNCK.


[Continued from vol. v. page 522.]

No. III.

THE next Journal is simply endorsed "Reuben Burrow," and on the inside of the cover an entry is made of several books he had lent to Messrs. Coote and Linsing. The Journal itself commences by stating that "on the 30th of April 1782" he "left the Drawing Room in the Tower to go to Colonel Watson," and then gives at considerable length his applications to the Duke of Richmond for "additional pay," and the circumstances which led to his resignation as Mathematical Master; but since these particulars have already formed the subject of a paper in the Mechanics' Magazine, by Senex, on "The Board of Ordnance in other days," there is no necessity for again transcribing them. From the opening notice, it appears that Mr. Burrow "received £50 from Mr. Wildman to buy necessaries for the voyage, and he (Mr. W.) paid 100 guineas to Capt. Hoare for the passage to Bengal. He likewise received 50 guineas back on condition of my (Mr. B.) teaching the Captain's two sons during the voyage; this 50 guineas only just served to clear my debts and expenses."

The following extracts are of a more melancholy character;
and if literally true, do not convey a very pleasing impression of the state of naval discipline at that period.

"August 29, 1782. The Royal George, a 100 gun-ship, at Spithead, sunk in about thirteen fathom water, owing to their giving her too great an inclination in scrubbing her bottom and the carelessness of Captain Waghorn. Admiral Kempenfelt was on board, and was drowned with the rest: about 800 men and 200 women were drowned. I was told by several people that the Captain had notice given him of his danger two or three times; once or twice by the other ships, and also by his carpenter and the cockswain. The ship is laid on her side, making an angle of about 40° with the horizon, but no attempts have been made to right her. A Diving Bell was made in the Dock Yard for the purpose of . . . I never heard what . . . but no steps were taken that had any apparent utility. I therefore wrote the following letter to the Board of Admiralty:

""To the Right Honorable Lords of the Admiralty.

"May it please your Lordships,—

"As raising the Ship lately sunk at Spithead is a matter of no great difficulty, I am surprised no steps are taken towards it that have any appearance of success. To right the Ship, nothing more is necessary than to moor two or more vessels on that side of the ship which lies uppermost and in a direction perpendicular to her; and to fasten ropes and cables to her masts, &c.; and not only to employ the force of each of their capstans, but also to take the advantage of the tide, and likewise the force of the wind, if necessary. When she is thus righted and retained so, which may easily be done, the upper deck will not be far from the surface of the water, and it will be much easier to get anything out of the Ship, either by means of the Diving Bell, or otherwise, than in the manner she lies at present.

"As to raising her after she is righted, I know no better methods than those of Tartaglia and De La Hire, but the detail of these may be greatly improved, and I should recommend the Board to employ Mr. James Creasy, of Boston in Lincolnshire, in this business; as he is not only well acquainted with matters of this kind, having been employed in Engineering and Shipbuilding, but is likewise a man of very extensive views and uncommon abilities, and every way capable either of inventing methods of his own or improving on those of others. Since the mode is simple, the expense will not be very great; and if it be thought proper to employ Mr. Creasy, I shall communicate my ideas to him more particularly in writing; I have no manner of doubt of his success, unless the business is delayed too long; in
that case it will be best to convey a barrel of gunpowder into the vessel, and by means of a saucisson to blow her up. The guns may thus be come at by means of the Diving Machine, and the place may likewise be cleared of the wreck and remains of the vessel. I have only given a hint of the method, as I do not expect any attention will be paid to it.

"'I am, &c.,
"'Reuben Burrow.'"

Some of Mr. Burrow's plans for the recovery of the Royal George or blowing her up appear feasible enough; but I have not been able to ascertain whether any of his suggestions were ever carried into effect. His object would seem to have been to obtain the greatest effect from the least force possible; and in the absence of galvanic agency, the water-tight sausage of gunpowder was no unworthy contrivance.

"We stayed along with Lord Howe's fleet, and did not make way with the wind as we might have done. There was some rough weather, and through a neglect of our Carpenter we had like to have sunk, for the rascal had forgot to fasten the ports, and the water came in at an amazing rate, and the officers were terribly frightened. We hung out a flag to say that we were in danger, and that the other ships should get out of our way, and we then changed our course for some time and ran before the wind. The weather continued pretty much the same till the end of September, and the wind was sometimes favourable. On September 30 we were in Lat. 48° 6', and the French West India fleet was expected every moment with a convoy of five ships of the line. I took the earliest opportunity of trying the method of finding the Latitude by observing how long the Sun took in ascending or descending its diameter, according to the rule given by Mr. Lyons; but I did not find it answer nearer than by several degrees. I also attempted the method by the Moon, but not having a watch that could be depended upon, and having nobody on board capable of helping me, I never got a good observation. I took the distance of the Moon from the Sun without using the telescope; but the Moon's Altitude was very bad, owing to a ship being in the way of the horizon: however, it gave the Longitude about 17°, and the dead reckoning gave 14 or 15 degrees.

"October 9. The altitude of the Thermometer was 72°.
"October 12. This day we were, at noon, in Latitude 29° 41', and last night, at seven o'clock, an observation of the Moon's distance from Tromalhaut gave the Longitude 19° 2' west of Greenwich. I deduced the time from the Altitude of the Moon. I took the distance and two of the Mates took the Altitudes, but out of three sets of observations only one was anything like right,
for some of the Altitudes were palpably erroneous, owing to the stupidity of the observers.

I expected, when I came on board, to have found some of the officers qualified for making such observations and calculations as were at least absolutely necessary; but, except the Captain, I did not find any one that had the least knowledge of such matters. They did not even so much as know how to allow for the change in the Sun's declination, nor how to take out the proper refraction in finding the Latitude from the Sun's meridian altitude; and they were likewise so conceited and ignorant as to be above being shown. I endeavoured to teach them better, and the Captain, with very great liberality of sentiment, advised them to learn, but they only made ridicule of it and pretended that they could carry a ship to India without it. The worst of these was Newton, the second mate. I had sent for him one afternoon to take the Sun's Azimuth while I took his Altitude, and he began at night to accuse me, before Captain Shippey, for breaking in upon his time to take observations. He seemed to think that I did him a great injury, and that his time was too sacred and of too much consequence to himself to be employed in making useless observations, and that he prided himself in being ignorant of such matters.

"I gave him to understand that I always expected my offer of showing them the methods of observing or calculating to be looked upon as a favour that I expected different treatment and as to his despising observations, &c., since it was looked upon as of such consequence by the East India Company as to be ordered in their directions, and considered as absolutely necessary to every officer, I could not conceive how he could vindicate his conduct to his employers he might be the means of destroying the lives of thousands if there was nobody on board better informed than himself.

"They had another piece of stupidity, which was, to take the Amplitudes when the Sun was as low as possible, instead of taking them at a proper time. I do not think the Chief Mate knows how to observe Azimuths at all—he attempted it one day, and I formed the opinion from his awkwardness.

"I had some time before investigated a proper rule for finding the fittest time of observing the Azimuth between the Tropics, and informed them about it, but they were all obstinate, and would have it that the lower the Sun was and the better would be the Azimuths. Let S = pole; mnt, circle of declination; z = zenith, and let zv touch the parallel of declination in O. Then, when the Sun
rises perpendicular in $\odot$, $S \odot Z$ is a right-angled spherical triangle; whence the following rules may be deduced for finding the proper time of making the observation; the Sun's altitude at that time and the azimuth angle $SZ\odot$.

"Now, tan. $ZS : \tan. \odot : r : \cos. ZS\odot$."

"Or, $r : \cot. SZ : tan. \odot : \cos. ZS\odot$."

"Again, $cos. \odot : \cos. SZ : r : \cos. Z\odot$."

"Or, $r : sec. \odot : \cos. SZ : \cos. Z\odot$."

"Similarly, $sin. ZS : r : sin. \odot : sin. SZ\odot$."

"Or, $r : cosec. ZS : sin. Z\odot : sin. SZ\odot$."

"Hence the following rules:—"

"1. To find the time.

"Add the tangent of the Latitude to the cotangent of the Declination; the sum is the cosine of the hour-angle from noon.

"2. To find the Azimuth Angle.

"Add the secant of the Latitude to the cosine of the Declination; the sum is the cosine of the Azimuth.

"3. To find the Altitude.

"Add the cosecant of the Declination to the sine of the Latitude; the sum is the sine of the Altitude.

"General Limitation.

"It is evident that the polar distance $Sn$ must be less than $SZ$ the colatitude, otherwise the vertical circle $Z\odot v$ cannot touch the parallel of Declination, and hence the Latitude and Declination must be of the same name, and the Latitude less than the Declination.

"This rule saves a great deal of trouble when it can be used; for a number of observations may be taken a little before and a little after the proper time found above, and there is no trouble of calculation whatever; for the difference between the constant Azimuth angle found above and the angle observed gives the Variation. A Table might be readily made with the Declination at the top and the Latitude at the side; whence the proper time and constant angle might be taken out. I think it would be proper to put such a table in the Nautical Almanack, not because it is anything either new or difficult, but because sailors are such idle scoundrels that they neglect thousands of opportunities of finding the Variation because they will not be at the trouble of working the Azimuths, and such a Table would save them that labour."

The object of the preceding problem evidently is "to find the time when the apparent motion of a celestial body is perpendicular to the horizon, from having its declination and the latitude of the place given;"—this being the most favourable time for making the observations required. Under this enunciation the
problem is considered by Professor Young as Prop. V, pp. 142–3 of his Plane and Spherical Trigonometry (1833), who also instances the same general limitation; and in a subsequent part of this work similar observations are made respecting the variation of the compass as are to be found in the preceding extract.

Notwithstanding these discouragements, Mr. Burrow "continued to make observations for the Longitude and Variation, though with very great difficulty, as scarcely anybody would help to take Altitudes except the Captain, and he had a good deal of trouble about it."

"November 26, 1782. In the afternoon we got into the Bay of St. Salvador. Several Catamarans and boats came out to see us, but the Portuguese governor ordered that we should not go beyond the White Fort.

"December 8, 1782. I addressed the following note to the Governor.

"'Mr. Reuben Burrow, an English Astronomer, on board the General Coote, East Indiaman; humbly begs leave to represent to His Excellency The Marquis of Valencia, Governor of St. Salvador, &c.; that he should be very happy to have the liberty of making some Astronomical Observations on shore for the purpose of determining the Latitude and Longitude, Magnetical Variation, &c., and other similar matters. This Mr. Burrow hopes will be complied with, and the more so, as Observations of this kind are incapable of being applied to any sinister purposes, and are allowed and encouraged by every enlightened government as being of general utility. A few days will be sufficient, if the weather is favourable, and Mr. Burrow will be very ready to submit his Observations to any inspection His Excellency the Governor may think proper after they are made, if required.

"'Bay of Bahia, 8 December 1782.'"

Whether he was successful or otherwise in this application does not appear, since the Journal here terminates abruptly with several pencil sketches of the appearance of the small islands of Martinvaz and Trinidad;—on two of the next pages are two series of observations on the Sun's Altitude, Azimuth, &c.; and the following memorandum on the cover of the volume affords sufficient evidence that he lost no opportunities of making appropriate observations, notwithstanding the inefficiency of his pupils and assistants.

"January 3, 1783. Gave Captain Burney the Longitude 29° 44' West, Latitude 20° 31' South. Variation of Compass 2° 3' West; of the island of Trinidad. The Longitude was deduced from 15 observations of the Sun and Moon; the Latitude
from 2 observations of the Sun and reduced to the Latitude of 
The Ninepin (a tall portion of rock so called), and the Longitude 
was also reduced to the same place."

On crossing the Line the rites of Neptune were not omitted 
by the crew, for Mr. Burrow has a note to the effect that he 
"borrowed half-a-crown of Mr. Hoare" on the occasion, which 
was no doubt expended in purchasing an exemption from the 
ceremonies usually performed in honour of his Oceanic Majesty.

The third Journal in my possession contains but few articles of 
importance. It appears to relate to the early portion (1784—
1788) of his residence in India, and has numerous entries re-
specting the hire and pay of Hindoo servants, household 
expenses, &c. Among the payments is one for "Jaggernaut Dass, 
two gold mohurs in full to the 8th of December;"—another 
entry certifies the receipt of "32 sicca rupees from A. Colvin on 
account of Almanacks sold by Colonel Martin;"—a third states 
that "the Sanscrit-Persian Moonshy has had three gold mohurs, 
and is to have five in the whole for writing the whole of Gladwin's 
Dictionary;"—whilst a fourth exhibits Mr. Burrow as receiving 
from "Colonel Watson's tailor" the somewhat un-mathematical 
costume of "one yellow silk coat without lining; twelve white 
waistcoats; twelve pairs of white nankeen breeches; and twenty 
pairs of cotton stockings." In another portion of this Journal 
I find the following "List of Books sent to Woolwich to De-
posit:"—

"Furtenbach Halinitro Pyrobolia. Folio, 1627.
Venn's Military and Maritime Discipline. Folio. London, 
1672.
Binning's Gunnery. 4to.
Soldato Christiano. 4to. Venet. 1604.
Peckham. Perspectiva Comm.
Il Bombardiero Veneto. 1631.
Del Tomear a Piedi; &c.
Flagello Militare, da Martena. Napoli, 1693.
Agrippa Scientia d'Arme. Roma, 1553.
Baroccio Problema Geometricum. Venet. 1586."

With regard to this curious and in many respects valuable 
donation, Dr. Rutherford very kindly informs me that "nearly 
half a century ago the Library then belonging to the Royal Mi-
litary Academy was transferred to the Royal Artillery and formed 
the foundation of the present excellent Library belonging to that
establishment.” He has seen most of the works mentioned in
the preceding list, endorsed as “Deposited by Reuben Burrow,”
and he has no doubt “that all of them are still in preservation
in the Library of the Royal Artillery.”

Several rude sketches of instruments used in surveying, and
a few diagrams relating to some now well-known mechanical
problems, occur in different portions of this volume, but none of
them of sufficient importance to merit transcription. A memo-
randum to “put the method of taking sections through hills, and
also with the Level,” taken in connexion with the “sale of Alma-
nacks” previously noticed, renders it probable that he published
something similar to Carnan’s Diary during his residence in
India. Should this conjecture prove correct, the following pro-
blems, with which I will conclude the notices of these Journals,
were no doubt intended to form a portion of the contents of one
of the numbers.

Problem I.

“Given the angles BRA, ARS, BSR, BSA, and the side AB,
to construct the figure:—that is to find the points R and S.

"Construction.—On
AB describe segments
of circles to contain
the given angles BSA
and BRA, also pro-
duce AB both ways to
m and n. Make the
angle mBN = the angle
RSA, and draw BN
meeting the circle BSA in N;—also make the angle nAM = the
angle SRB, and draw AM meeting the circle BRA in M; join NM,
and it will meet the segments in R and S, the points required.”

Problem II.

“If the three points A, B, C are given, and from D and E the
angles ADC, CDE, DEB,
and BEC be observed; required to determine
the points D and E.”

Construction. — Mr.
Burrow merely enu-
nicates this problem, but
its construction may rea-
dily be supplied as fol-
ows:

On AC and BC de-
scribe segments of cir-
cles to contain the angles
ADC and BEC respectively. Produce AC to \( n \), and make the angle \( nCM = ADC + CDE \), and draw CM cutting the circle on AC in M: again produce BC to \( m \), and make the angle \( mBN = DEB + BEC \); also draw BN cutting the circle on BC in N. Join MN, and it will cut the circles in D and E, the points required.

The demonstrations need not be added, since they are sufficiently obvious from the constructions and the known property that the exterior angle of any inscribed quadrilateral is equal to the opposite interior angle*.

Burnley, Lancashire,
June 29, 1853.

XXX. Note on the Preparation of Propionic and Caproic Acids.
By Professor Williamson†.

In the August Number of the Philosophical Magazine I observe a paper by Dr. Wrightson, describing the action of an alcoholic solution of hydrate of potash upon a mixture of the cyanides of æthyle and amyle, and employing the result of that experiment as an argument against the othyle theory. The author reasons on the assumption, that, of two results which he conceived possible, the one he obtained must be unfavourable to the othyle theory, the other favourable to it. It is to be regretted that the author did not endeavour to describe the grounds of this assumption—to show us the connexion between each result and the corresponding theory; for I think he could then hardly have failed to perceive that his assumption is erroneous. The result which he failed in obtaining is incompatible with the othyle theory of which he conceived it a consequence, and the result he obtained is decidedly confirmatory of the theory which he expected it to upset.

The othyle theory represents acetic acid as containing the radical \( C^{3}h^{3}O \) (derived from æthyle, \( C^{2}h^{5} \), by the substitution of O for \( h^2 \)). It connects the formulæ of the acetic series with that of water, \( h_{h}O \), by writing them in the following manner:—

\[
\begin{align*}
\text{Formic acid} & & \cdot & \cdot & C_{h}O & O \\
\text{Acetic acid} & & \cdot & \cdot & C^{3}h^{3}O & O \\
\end{align*}
\]

* Mr. Burrow's journals will shortly be presented to the Library of the Royal Astronomical Society, and may there be consulted by those who are desirous of seeing more of the occasional jottings of this somewhat eccentric but able mathematician.
† Communicated by the Author.
Propionic and Caproic Acids.

Propyllic acid...\(\text{C}_3\text{H}_5\text{O}_2\)  
Butyric acid...\(\text{C}_4\text{H}_7\text{O}_2\)

each term containing \(\text{C}_1\text{H}_2\) more than the preceding.

Now Dr. Wrightson assumed that if this theory were correct, other acids ought to exist, lying between these in their composition; whereas it is evident that, if such bodies existed, we could not consistently with any theory represent them by fractional formulae such as

\(\text{C}_3\text{H}_5\text{O}_2\)

but must double the formulae of the original series to make room for the insertion of these new terms with entire numbers; so that the existence of these putative acids would have been a decisive argument against the correctness of the othyle theory. This theory ought hardly to be blamed for the non-existence of two alcohol-radicals in an acid, where it denies the existence of even one.

As regards the other theory which Dr. Wrightson considers in connexion with the above reaction, I must frankly state, that, although the existence of the imaginary acids might be conceived consistently with it, I do not consider the non-existence of those acids as affording any evidence against it. If acetic acid contained two atoms of methyle \(\text{C}_1\text{H}_5\), and propyllic acid two atoms of æthyle \(\text{C}_2\text{H}_5\), it would be quite conceivable that an intermediate acid might exist containing one atom of methyle and one of æthyle; but I could not, even according to Kolbe's theory, conceive it formed from the mixture of two cyanides. Kolbe's original theory, by which acetic acid was represented as oxalic acid and methyle, has the merit of accounting in a very satisfactory manner for several important reactions, and has undoubtedly been of great service to our progress in the subject; but the recent modification with which we have been favoured by that distinguished chemist is so complicated, and introduces such unintelligible symbolical distinctions, that it seems likely to serve chiefly as an extinguisher of the original theory.

As Kolbe's and Frankland's beautiful reaction of the cyanides with potash is the process most usually adopted for the preparation of propyllic or caproic acids, I may be permitted to describe a slight modification in the method employed for the formation of cyanide of æthyle or amyle, by which large quantities of these substances may be prepared with far less trouble and annoyance for the purpose of that reaction. Everybody who has distilled sulphovinate of potash with cyanide of potassium must be aware,
that not nearly all the æthyle in the salt is obtained in the form of cyanide, and that the process is rendered still more objectionable by the insufferable odour which accompanies it. Messrs. Routledge and Denman have prepared in the following manner several pounds of the cyanide of æthyle and amyle in the laboratory of University College without incurring any appreciable loss of the substance, and without any odour being perceptible. Crude iodide of æthyle is mixed with about four times its bulk of spirits of wine, and introduced into a bolthead containing more than an equivalent of powdered cyanide of potassium. The bolthead is connected with a Liebig's condenser, in such a manner, that, when it is heated to ebullition in a water-bath, the vapours will run back into it in proportion as they condense. A drop or two of the distillate is collected from time to time by inclining the apparatus, and tested for iodine by long boiling with alcoholic potash, after which the stench test is applied in the usual manner. When the reaction is complete, the mixture is distilled to dryness, and the residual salts washed out by a little fresh alcohol being distilled off them. The distillates are decomposed in the usual manner by potash. We have found it advisable to use in this operation an apparatus similar to the above-mentioned, with the addition of a tube connecting the upper end of the condenser with one or two Woulf's bottles containing hydrochloric acid.

In the preparation of cyanide of amyle, the chloride of that radical was used instead of the iodide, as a small loss of iodine must necessarily occur in the successive actions above described, and in the subsequent reproduction of the elementary body from its potassium salt. It is not unlikely that the chloride of æthyle might also be used with advantage in this reaction, if the vapour were led into the alcohol and cyanide of potassium slowly enough to allow time for the decomposition.

XXXI. Researches on Granite. By A. Delesse*.

FROM the special study of the granitic rocks of the Vosges Mountains, the author has made some generalizations of great interest upon the relation of the proportion of silex, and of the nature of the mica, to the age of the mass and to its circumstances of crystallization, also upon the varieties of feldspar.

There are in the Vosges at least two types of granite, distinguishable by their mineralogical constitution and geological position.

The first is the granite of the Ballons; it forms the summits and the central part of the ridge of the Vosges; its greatest de-
velopment is between Sainte Marie aux Mines and Guebwiller; it contains quartz, orthose, feldspar of the sixth system, dark mica, and sometimes hornblende.

The quartz is hyaline, and of a gray colour; it is most abundant in the highly crystalline varieties; those varieties which are porphyritic and least crystalline contain little or no quartz, the greater part of the silica having remained in combination with a feldspathic paste.

The orthose is the preponderating mineral of this granite. It is white or reddish-yellow; both kinds, containing oxide of iron, turn red by alteration; it sometimes becomes greenish, and by decomposition passes into a halloysite. The orthose is the most persistent mineral of this granite; its crystals sometimes attain a decimeter in length: the analysis of three specimens from different localities gave the following result:

<table>
<thead>
<tr>
<th></th>
<th>SiO²</th>
<th>Al²O³</th>
<th>Fe²O³</th>
<th>CaO</th>
<th>MgO</th>
<th>NaO</th>
<th>KO</th>
<th>HO</th>
<th>Sum</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>64·91</td>
<td>19·16</td>
<td>traces</td>
<td>0·78</td>
<td>0·63</td>
<td>2·49</td>
<td>11·07</td>
<td>0·30</td>
<td>99·36</td>
</tr>
<tr>
<td>II</td>
<td>64·66</td>
<td>19·58</td>
<td>traces</td>
<td>0·70</td>
<td>15·18</td>
<td>0·58</td>
<td>100·00</td>
<td></td>
<td></td>
</tr>
<tr>
<td>III</td>
<td>64·00</td>
<td>20·55</td>
<td>0·68</td>
<td>13·49</td>
<td>1·28</td>
<td>100·00</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The proportions given in this table differ but slightly from each other or from previous determinations; orthose is then a mineral whose composition is very constant, and independent of that of the rock in which it is produced.

The granite of the Ballons contains also a feldspar of the sixth system; its colour on a fresh fracture is greenish; it is translucent, and has a greasy lustre; its crystals show parallel striae, which characterize the isomorphous feldspars of the sixth system; it becomes red by atmospheric alteration, afterwards white, and the mineral passes into kaolinite. The analysis of it gave the following composition:

<table>
<thead>
<tr>
<th></th>
<th>SiO²</th>
<th>Al²O³</th>
<th>Fe²O³</th>
<th>CaO</th>
<th>MgO</th>
<th>NaO</th>
<th>KO</th>
<th>HO</th>
<th>Sum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>58·55</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>99·29</td>
</tr>
<tr>
<td>Alumina</td>
<td>25·26</td>
<td></td>
<td></td>
<td></td>
<td>11·807</td>
<td>0·092</td>
<td></td>
<td></td>
<td>30·422</td>
</tr>
<tr>
<td>Oxide of iron</td>
<td>0·30</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>Oxide of manganese</td>
<td>trace</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>8</td>
</tr>
<tr>
<td>Lime</td>
<td>5·03</td>
<td>1·412</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Magnesia</td>
<td>1·30</td>
<td>0·517</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Soda</td>
<td>6·44</td>
<td>1·648</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Potash</td>
<td>1·50</td>
<td>0·255</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Loss by burning</td>
<td>0·91</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Sum</td>
<td>99·29</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
</tbody>
</table>

It contains less of silica and of alkalies, with more of lime, than oligoclase; moreover, its atomic proportions of oxygen are very nearly that of andesite. This strengthens a remark I have made
before, that all the feldspars of the sixth system are isomorphous, and that their proportions of silica may vary indefinitely between that of albite and that of anorthite. This feldspar of the sixth system occurs in the most crystalline granite, and appears also to be especially associated with hornblende.

The granite of the Ballons contains but one mica, of a dark colour, with sometimes a greenish shade. In the polariscope of Amici it shows two optic axes, forming a very small angle. Its dominant bases are magnesia and iron: it is affected by hydrochloric and sulphuric acids.

The accidental minerals of this granite are hornblende, sphene, zircon.

It is very little broken or veined. The mean composition of some of its varieties are—

<table>
<thead>
<tr>
<th></th>
<th>SiO₂</th>
<th>Al₂O₃</th>
<th>Fe₂O₃</th>
<th>CaO</th>
<th>KO, NaO, MgO</th>
<th>Loss by burning</th>
<th>Sum</th>
</tr>
</thead>
<tbody>
<tr>
<td>I.</td>
<td>70·8</td>
<td>15·3</td>
<td></td>
<td>0·5</td>
<td>12·4</td>
<td>1·0</td>
<td>100</td>
</tr>
<tr>
<td>II.</td>
<td>68·5</td>
<td>...</td>
<td>1·3</td>
<td>0·6</td>
<td>13·3</td>
<td>0·9</td>
<td>...</td>
</tr>
<tr>
<td>III.</td>
<td>67·3</td>
<td>16·1</td>
<td>1·9</td>
<td>0·6</td>
<td>13·3</td>
<td>0·8</td>
<td>100</td>
</tr>
<tr>
<td>IV.</td>
<td>64·8</td>
<td>20·0</td>
<td>1·1</td>
<td>0·7</td>
<td>12·7</td>
<td>1·4</td>
<td>100</td>
</tr>
<tr>
<td>V.</td>
<td>64·8</td>
<td>21·1</td>
<td>...</td>
<td>0·7</td>
<td>12·7</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>VI.</td>
<td>63·3</td>
<td>20·2</td>
<td>1·8</td>
<td>0·7</td>
<td>11·8</td>
<td>2·9</td>
<td>100</td>
</tr>
<tr>
<td>VII.</td>
<td>63·8</td>
<td>18·7</td>
<td>2·3</td>
<td>1·4</td>
<td>13·8</td>
<td>1·4</td>
<td>100</td>
</tr>
</tbody>
</table>

The dots indicate that the quantitative determination was not made.

The loss of silica is replaced by alumina and lime. These variations depend very much (as I have proved elsewhere, Bull. de la Soc. Géol. 2nd sér. vol. ix. p. 464) upon the position in the mass, the more central and elevated being the more siliceous, and upon the nature of the rocks in junction.

The second type of granite is the granite of the Vosges. I group under this name the varieties which have been called common granite, leptynite and gneiss.

Its essential minerals are quartz, orthose, feldspar of the sixth system, two micas—a dark and a bright.

The quartz is in grayish-white grains. The orthose is the preponderating mineral; it occurs in minute lamellæ or grains, the analysis of which gave—

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Silica</td>
<td>66·08</td>
</tr>
<tr>
<td>Alumina and traces of peroxide of iron</td>
<td>18·70</td>
</tr>
<tr>
<td>Oxide of manganese</td>
<td>trace</td>
</tr>
<tr>
<td>Lime</td>
<td>0·93</td>
</tr>
<tr>
<td>Magnesia</td>
<td>0·45</td>
</tr>
<tr>
<td>Soda</td>
<td>3·77</td>
</tr>
<tr>
<td>Potash</td>
<td>9·11</td>
</tr>
<tr>
<td>Sum</td>
<td>99·04</td>
</tr>
</tbody>
</table>
The large amount of silica is no doubt due to quartz mechanically mixed.

Orthose and quartz are found in the most degraded varieties of this granite.

The feldspar of the sixth system is rare, and only found in the most crystalline varieties.

The granite of the Vosges, although its grain is fine and its minerals generally smaller than those of the porphyritic granite, contains no feldspathic paste. Its essential character is to contain two micas, the one dark the other bright. The first is identical with the mica of the Ballons. The second is silver-white or violet-gray; its dominant base is potash; it resists the action of sulphuric and hydrochloric acids, and is altogether the same as that I have described before as occurring in the veins of pegmatite (*Ann. des Mines*, 4th ser. vol. xvi. p. 100). The clear is less abundant than the dark mica, and is less uniformly disseminated.

The accidental minerals are garnet, pinitie, and, in the schistose varieties, hornblende, graphite, fibrolite. Some minerals of subsequent origin are common to the two granites, as chlorite, carbonate and oxides of iron, heavy spar, fluor spar, &c.

The granite of the Vosges is very much fissured and cut up by veins and lodes. Its density is about that of quartz, and is less than that of the granite of the Ballons. Its average composition may be computed from the accompanying table: for each analysis a large mass of the stone was reduced to powder, and the assay taken from this.

<table>
<thead>
<tr>
<th>SiO₂</th>
<th>Al₂O₃</th>
<th>Fe₂O₃</th>
<th>MnO</th>
<th>CaO</th>
<th>MgO</th>
<th>KO</th>
<th>NaO</th>
<th>LoiS by burning</th>
<th>Sum</th>
</tr>
</thead>
<tbody>
<tr>
<td>I.</td>
<td>76·3</td>
<td>12·8</td>
<td>1·5</td>
<td>0·8</td>
<td>trace</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>II.</td>
<td>75·4</td>
<td>12·7</td>
<td></td>
<td>0·6</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>III.</td>
<td>73·8</td>
<td>15·8</td>
<td></td>
<td>0·9</td>
<td>0·9</td>
<td>7·8</td>
<td>...</td>
<td>0·80</td>
<td>100·00</td>
</tr>
<tr>
<td>IV.</td>
<td>73·3</td>
<td>10·4</td>
<td>1·6</td>
<td>0·7</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>V.</td>
<td>72·0</td>
<td>15·3</td>
<td>0·4</td>
<td>0·9</td>
<td>0·60</td>
<td>7·70</td>
<td>2·00</td>
<td>0·40</td>
<td>99·50</td>
</tr>
<tr>
<td>VI.</td>
<td>70·4</td>
<td>16·6</td>
<td></td>
<td>0·6</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>VII.</td>
<td>70·0</td>
<td>17·3</td>
<td></td>
<td>0·6</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>VIII.</td>
<td>67·3</td>
<td>16·2</td>
<td></td>
<td>1·9</td>
<td>0·6</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>IX.</td>
<td>66·7</td>
<td>...</td>
<td>1·8</td>
<td>0·9</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>...</td>
</tr>
</tbody>
</table>

The dots show that the quantitative determination was not made.

The phenomena of the rock veins in the masses of granite are rather complicated.

These veins appear generally to have formed at the time of crystallization of the granite; their great richness in quartz favours this opinion, it being the last mineral of the rock to remain in a fluid state.

The granite of the Vosges forms smaller eminences around the bosses of the Ballons, and is itself covered by stratified rocks, into which it graduates by insensible degrees. The granite of the Ballons has evidently penetrated with violence into the granite of the Vosges; this is well seen at Mehachamp. In some places the junction of the two is not discoverable.

Of these rocks, then, that containing the smaller proportion of silica and the greater of alumina is the more recent.

The distinction of two granites in the chain of the Vosges is not of mere local interest; the remark may be extended to most granitic regions, of which I will only mention the right bank of the Rhine, Normandy, Brittany, Auvergne, Ireland, &c.

The general application of the above observations shows that the same geological phenomena are reproduced after long intervals of time and in widely separated districts. It is not then surprising that we should find in most granitic regions two granites: the one porphyritic, and containing but one mica; the other granular (genu), and containing two micas; the former being the more recent, and generally poorer in silica.

XXXII. Note on the New Rule of Limits.

By J. J. Sylvester, F.R.S.*

It may appear like harping too long on the same string to add any further remarks on the rule relating to so simple and elementary a matter as that of assigning limits to the roots of a given algebraical equation; but it will be remembered that some of the greatest masters of analysis, including the honoured names of Newton and Cauchy, have not disdained to treat, and to give to the world their comparatively imperfect results on this very subject. I hope, therefore, to stand excused of any undue egotism in adding some observations which may tend to present, under a clearer aspect and more finished form, the new and beautifully flexible rule laid before the readers of this Magazine in the two preceding Numbers.

1st. I observe that any succession of signs may be considered as made up of, and decomposable into, sequences of changes exclusively, if we agree to consider, where necessary, a single iso-

* Communicated by the Author.
lated sign $+$ or $-$ as a sequence of zero changes. Thus, for instance, $+-+-+--+---$ may be treated as made up of the variation sequences

$+--; -+; ++; +++; +--; --*;

2ndly. I observe that if $X_1, X_2, \ldots X_i$ be all linear functions of $x$, and the signs of the coefficients of $x$ in these functions constitute a single unbroken series of variations, the denominator of the continued fraction

$$\frac{1}{X_1 + \frac{1}{X_2 + \frac{1}{X_3 + \cdots + \frac{1}{X_i}}}}$$

(reduced to the form of an ordinary algebraical fraction) will have all its roots real.

3rdly. Suppose, for greater simplicity, that $\phi x$ is of one degree in $x$ lower than $f x$, and that by the ordinary process of common measure we obtain

$$\frac{\phi x}{f x} = \frac{1}{X_1 + \frac{1}{X_2 + \frac{1}{X_3 + \cdots + \frac{1}{X_n}}}},$$

where $X_1, X_2, X_3, \ldots X_n$ are all of them linear functions of $x.$

Let $X_1, X_2, \ldots X_n$ be divided into distinct and unblending sequences,

$X_1 X_2 \ldots X_i; X_{i+1} X_{i+2} \ldots X_{\psi}; X_{\psi+1} \ldots X_{\psi}; X_{(i+1)} X_{(i+2)} \ldots X_n;$

so that in each sequence the signs of the coefficients of $x$ present a single unbroken series of variations, which by virtue of observation (1), may be considered to be always capable of being done, and let

$$\frac{\phi x}{f_1 x} = \frac{1}{X_1 + \frac{1}{X_2 + \frac{1}{X_3 + \cdots + \frac{1}{X_i}}}}$$

$$\frac{\phi x}{f_2 x} = \frac{1}{X_{i+1} + \frac{1}{X_{i+2} + \cdots + \frac{1}{X_i}}},$$

$$\cdots \cdots \cdots \cdots \cdots$$

$$(\phi) x = \frac{1}{X_{(i+1)} + \cdots + \frac{1}{X_n}};$$

then, according to observation (2), the equations

$$f_1 x = 0 f_2 x = 0 \ldots (f) x = 0$$

have each of them all their roots real; and the observation now to be made is, that the highest of the highest roots and the

* The rule is, that the given series of signs is to be separated into distinct sequences of variations, so that the final term of one sequence and the initial term of the next shall form a continuation, i.e. we must have variation sequences connected together by continuations at their joinings.

P 2
lowest of the lowest roots of these equations furnish respectively a superior and inferior limit to the roots of \( fx = 0 \).

N.B. The single root of any one or more of these which may be of the first degree in \( x \) is to be treated, in applying the preceding observation, as being at the same time the highest and the lowest root of such equation or equations.

4thly and lastly. The problem of assigning limits to the roots of \( fx = 0 \) reduces itself to that of finding limits to

\[
f_1x = 0 \quad f_2x = 0 \ldots (f)x = 0;
\]

for the greatest and least of these collectively will evidently, à fortiori, by virtue of the preceding observation, be limits to the roots of \( fx = 0 \). Of any such of these as are linear, the root or roots themselves may be treated as known; leaving these out of consideration, the functional part of any other of them, such as \( f_1x \), is the denominator of a continued fraction of the form

\[
\frac{1}{(a_1x + b_1) + (a_2x + b_2) + (a_3x + b_3) + \cdots (a_4x + b_4)},
\]

in which \( a_1, a_2, a_3, \ldots a_4 \) present a single sequence of variations of sign, and the limits to the roots of \( f_1x = 0 \) may be found as follows.

Form the two systems of equations (in which \( \mu_1, \mu_2, \ldots \mu_i-1 \) are numerical quantities having all the same algebraical sign, but are otherwise arbitrary and independent),

\[\begin{align*}
U_1 - U_2 - U_3 - \cdots - U_i+1 - U_{i+2} - \cdots - U_{(i) + 1} - U_{(i) + 2} - \cdots - U_n = & -\infty \\
\end{align*}\]

The theorem under this form suggests a much more general one relating to para-symmetrical determinants, i.e. determinants partly normal and partly gauche, which will be given hereafter; one example among the many confirming the importance of the view first stated in this Magazine by the author of this paper, whereby continued fractions are incorporated with the doctrine of determinants.
Mr. J. J. Sylvester on the New Rule of Limits.

\[ a_1 x + b_1 = \mu_1 \]
\[ a_2 x + b_2 = -\mu_2 - \frac{1}{\mu_1} \mu_2 \]
\[ a_3 x + b_3 = \mu_3 + \frac{1}{\mu_2} \mu_3 \]
\[ \ldots \ldots \ldots \ldots \]
\[ a_{i-1} x + b_{i-1} = (-i-2)\mu_{i-1} + \frac{(-i-2)}{\mu_{i-2}} \mu_{i-2} \]
\[ a_i x + b_i = \frac{(-i-1)}{\mu_{i-1}} \mu_{i-1} \]

then (supposing \( \mu_1 \) to have the same sign as \( a_1 \)) the highest of the values of \( x \) obtained from the first system, and the lowest of the values of \( x \) found from the second system of these equations, will be a superior and inferior limit respectively to the roots of \( f_i x = 0 \); and so for all the rest of the equations

\[ f_2(x) = 0 \quad f_3(x) = 0 \quad \ldots \quad (f)x = 0, \]

excluding those of the first degree.

It will be seen that the theorems contained in the observations (3) and (4) combined (which presuppose the statements made in observations (1) and (2)), contain between them the theorem given in the last Number of the Magazine, but rendered in one or two particulars more simple and precise, and, as it were, reduced to its lowest terms. In the whole course of my experience I never remember a theory which has undergone so many successive transformations in my mind as this very simple one, since the day when I first unexpectedly discovered the germ of it in results obtained for quite a different purpose. In fact, it never entered into my thoughts that in so beaten a track, and in so hackneyed a subject as that of finding numerical limits to the roots of an equation, there was left anything to be discovered; and my sole merit, if any, in bringing the new rule to light, consists in having been able to detect the presence and appreciate the value of a truth which fortune or providence had put into my hands.

7 New Square, Lincoln’s Inn,
August 6, 1853.
XXXIII. The Algebraical Theory of the Secular-inequality Determinative Equation generalized. By J. J. Sylvester, F.R.S.*

Art. 1. \( \text{L} \text{E} \text{T} \)

\[
X_1 = ax + \alpha \quad X_2 = \begin{bmatrix} ax + \alpha; & bx + \beta \end{bmatrix} \quad X_3 = \begin{bmatrix} ax + \alpha; & bx + \beta; & dx + \delta \end{bmatrix}
\]

and let the first coefficients of \( X_1, X_2, X_3, \&c. \) have all the same sign; then I say that the roots of any such function as \( X_t \) will be all real, and will lie respectively in the intervals comprised between \(+\infty\) the successive descending roots of \( X_{t-1} \) and \(-\infty\). [When \( a=1, c=1, f=1, \&c., \) and \( b=0, d=0, e=0, \&c., \) \( X_t = 0 \) becomes the well-known secular-inequality equation.]

**Demonstration.** For greater simplicity, let all the first coefficients be taken positive, and suppose the theorem proved up to \( i \), it will be true for \( i+1 \). For by a well-known property of symmetrical determinants, when \( X_t = 0, X_{t-1}, \) and \( X_{t+1}, \) will have contrary signs. Let the roots of \( X_{t-1} \) be

\[
h_1 \quad h_2 \ldots \quad h_{i-1} \quad h_i
\]

When \( x = h_1 \), which is greater than \( h_1 \), the greatest root of \( X_{t-1} \) will be positive; when \( x = k_2 \), which lies between the first and second root of \( X_{t-1}, X_{t-1} \) will be negative; and so on, \( X_{t-1} \) alternately becoming positive and negative as we pass from root to root of \( X_t \).

Hence \( X_{t+1} \), which is positive when \( x = \infty \), becomes negative when \( x = h_1 \), positive again when \( x = h_2 \), and so alternately; being finally, when \( x = h_i \), positive or negative, and when \( x = -\infty \), negative or positive, according as \( i \) is even or odd. Hence \( X_{t+1} \), which changes sign \( i+1 \) times between \(+\infty\) and \(-\infty\), must have all its roots real, and lying severally in the intervals included between \(+\infty\), the successive roots of \( X_i \) and \(-\infty\). Hence if the theorem be true for \( i-1 \) and \( i \), it is true for all numbers above \( i \); but if we take

\[
ax + \alpha \quad \text{and} \quad \begin{bmatrix} ax + \alpha; & bx + \beta; \end{bmatrix}
\]

the latter is \((ax + \alpha)(cx + \gamma) - (bx + \beta)^2\), which is positive for \( x = \infty \), negative for \( ax + \alpha = 0 \), and positive for \( x = -\infty \). Hence the theorem is true for \( X_1 \) and \( X_2 \), and therefore universally.

In the above demonstration it was supposed that the leading coefficients are all positive; but the demonstration will be precisely the same, mutatis mutandis, if they are all negative.

* Communicated by the Author.
The Secular-inequality Determinantine Equation generalized. 215

Art 2. And much more generally it may be shown, in like manner, that if the successesions of signs in the series consisting of the sign + followed by the signs of the principal coefficients in \(X_1, X_2, \ldots X_{m+n}\) consist of \(m\) variations and \(n\) continuations, the number of real roots of the equation \(X_{m+n}=0\) will be at least as great as the positive value of the difference between \(m\) and \(n\). This theorem, moreover, remains true if \(X_1, X_2, X_3, \&c.\) be formed from a symmetrical matrix, in which the terms, instead of being linear functions of \(x\), are any odd-degreed rational integral functions of \(x\), or fractional functions of which the numerators (when rendered prime to their denominators) are odd-degreed functions of \(x\). My friend M. Borchartd, who has so beautifully effected the decomposition of my formulæ for the Sturmian criteria of reality into the sums of squares for the secular-inequality form of the equation, may now, if he pleases, tax his ingenuity to effect a similar decomposition for the general case supposed in art. 1 *.

Art. 3. It is obvious that, in applying the theorem contained in arts. 1 and 2, it is indifferent whether we look to the signs of the successive determinants \(a; b, c; \&c., \) or to those of \(a; b, \beta; \gamma; \&c.; \) or, more generally, to those of \(a+a\theta; b+b\beta; c+c\gamma; \&c., \) \(\theta\) being any arbitrary but real quantity. Conversely we obtain the remarkable theorem, that when any homogeneous quadratic function, whose coefficients are linear functions of \(\theta\), is linearly converted by real substitutions into a sum of positive and negative squares, the greatest difference for any value of \(\theta\) between the num-

* So, too, my own more simple method for proving the Omni-reality of the roots of the secular-inequality equation given in a previous Number of this Magazine, August 1852, ought to be capable of being extended to the general form in art. 1, i. e. we ought to be able to prove that the equation whose roots are the squares of the roots of \(X_1=0\) will have all its coefficients alternately negative and positive. If we take ex. gr. \(i=2,\) the equation to the squares of the roots becomes

\[
(ac-b^2)x^2 - \{ay+ca-2b\beta\}^2 + 2(b^2-ac)(\alpha y-\beta^2)\right\}x + (\alpha y-\beta^2) = 0;
\]

and we have to prove that the coefficient of \(\alpha x\) in this equation is essentially positive when \(ac-b^2\) is positive: this may be shown by various modes of decomposition; amongst others, by writing the coefficient in question under the form

\[
\frac{1}{\epsilon^2} \{ (c^2+\gamma b^2-2bc\beta)^2 + \gamma^2(ac-b^2)^2 + 2(b\gamma - c\beta)^2(ac-b^2) \}.
\]

In general, if \(L\) is essentially positive when \(L_1, L_2, \ldots L_i\) are positive, then, discarding all artifices of calculation, this must be capable of being proved by virtue of an identity of the form

\[
L = \Sigma m^2 \cdot L_1 + \Sigma m^2 \cdot L_2 + \ldots + \Sigma m^2 \cdot L_i.
\]
ber of positive and the number of negative squares has for its limit the number of real roots of \( \theta \) in the Determinant (otherwise called the Determinant) of the given function. The theorem actually demonstrated above teaches only this much, viz. that the maximum difference in number between the two species of squares (which depends only on the value given to \( \theta \)) cannot exceed the number of real roots in the discriminant; it admits, however, of an easy proof that this maximum difference is equal to the number of real roots, so that the one number is, in the strict sense of the word, an exact limit to the other.

Art. 4. I was led to the theorem, as given in art. 1, by having to consider the following curious and important question.

"Given i linear functions of \( x \), say \( X_1, X_2, \ldots X_i \), to find the \( i-1 \) positive quantities, say \( \mu_1, \mu_2, \ldots \mu_{i-1} \), which shall give the least value to the greatest root, or the greatest value to the least root of the equation

\[
(X_1^2-\mu_1^2)(X_2^2-\left(\mu_2+\frac{1}{\mu_1}\right)^2)(X_3^2-\left(\mu_3+\frac{1}{\mu_2}\right)^2)\ldots (X_i^2-\left(\frac{1}{\mu_{i-1}}\right)^2) = 0."

The theorem in art. 1 enables me easily to demonstrate, that if we take \( X'_1, X'_2, \ldots X'_i \) identical with

\[
\sqrt{1}.X_1, \sqrt{1}.X_2, \ldots \sqrt{1}.X_i,
\]

the sign of the square root being selected in each case so that the coefficients of \( x \) in \( X'_1, X'_2, \ldots X'_i \) shall have all the same sign, then the least value of the greatest root, and the greatest value of the least root, of the given equation will be respectively the greatest and least finite roots of the equation

\[
X'_1-\frac{1}{X'_2} = \frac{1}{X'_3} \ldots \frac{1}{X'_i} = 0*;
\]

the two systems of values of \( \mu_1, \mu_2, \ldots \mu_{i-1} \) required being the two systems of values of

\[
X'_1, X'_2 = \frac{1}{X'_1}, X'_3 = \frac{1}{X'_2} \ldots X'_{i-1} = \frac{1}{X'_{i-1}} X'_i = \frac{1}{X'_i} \ldots \frac{1}{X'_{i-2}} \frac{1}{X'_{i-3}} \ldots \frac{1}{X'_1},
\]

corresponding respectively to these two values of \( x \).

And it is by means of this solution that the statement of the rule for finding the superior and inferior limits to the real roots of an algebraical equation made in the last August Number of the Magazine, is capable of being converted into the statement contained in the third observation on the same rule in the present Number.

7 New Square, Lincoln's Inn,
August 7, 1853.

* The finite roots of this are the same as those of

\[
X'_1 = \frac{1}{X'_{i-1}, \frac{1}{X'_{i-2}} \ldots \frac{1}{X'_1} = 0.\]
XXXIV. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 150.]

June 9, "RESEARCHES on the distribution of the Blood-vessels, 1853. &c. in the Lungs." By James Newton Heale, M.D.

After referring to the discrepancies in the opinions entertained by anatomical writers both with respect to the distribution and to the functions of the blood-vessels with which the lungs are supplied, the author states the leading features in which the observations made by him differ from those which have hitherto been published. He finds that:

1st. The pulmonary artery makes no anastomosis whatever with any other artery, nor do its own branches anastomose together; its branches go direct to the air-cells, and are there distributed, and terminate as arteries; none of its branches go to any other tissues of the lungs besides the air-cells, except some few which perforate the sub-pleural cellular tissue, and are distributed to the pleura; some of these also cross the posterior mediastinum beneath the pleura, and reach the thoracic pleura.

2ndly. The bronchial (so-called) arteries have their own special distribution, which will be described further on; they do not supply, in the smallest degree, any portion of the bronchial mucous membrane, and they form no sort of communication, either with the pulmonary arteries or veins, except as supplying their cellular sheaths, and therefore in all probability furnishing their vasa vasorum.

3rdly. The bronchial mucous membrane is very freely supplied with an exceedingly vascular plexus, of a peculiar and very characteristic description, which is found to ramify in every part of the bronchial membrane, and which may be traced even as high as the trachea. The whole of this plexus is derived from the air-cells, and terminates ultimately by means of minute radicles, which form trunks and join the pulmonary veins. No trace whatever of any branches of the pulmonary artery, previous to this becoming capillary in the air-cells, is found in any part of the bronchial membrane.

4thly. The blood being brought to the air-cells by means of the pulmonary artery, is wholly returned by the pulmonary veins; but the trunks of these latter are formed by the junction of two distinct sets of radicles, namely, one set which comprises those which are formed from the perimeters of the air-cells (i.e. that part of the air-cell which is distant from the bronchial tube to which it is connected), and which at once form trunks which are visible on the surface of the lungs, and of all the lobules, and especially of the surfaces which adjoin the interlobular fissures; the other set consists of those which are derived from the bases of the air-cells, and which supply the bronchial membrane, and then terminate by radicles forming trunks, which join the before-mentioned set of pulmonary veins; and from these conjoined, the larger venous trunks are derived, which at length accompany the larger bronchi, and the pulmonary arteries, and which finally terminate in the left auricle of the heart; so that
the blood brought to the left auricle is formed of that portion which comes direct from the air-cells, and that which, after leaving the air-cells, has undergone the further process of distribution on the bronchial membrane, and has been the source from whence the epithelium and the bronchial mucus have been derived, and may therefore be considered as somewhat altered in character from that which, coming direct from the air-cells, has not been exposed to such changes.

5thly. It is possible to inject entirely the pulmonary artery and veins, without injecting the bronchial artery or veins; and it is also possible thoroughly to inject the latter without at all injecting the former; and when that is the case, i.e. the (so-called) bronchial arteries are injected while the pulmonary vessels are empty, it will be found that the bronchial membrane is wholly uninjected, however perfectly the (so-called) bronchial vessels may have been filled.

6thly. By injecting the lung through the pulmonary veins, the bronchial membrane becomes thoroughly injected, even before the air-cells are so; on the contrary, when the pulmonary artery is alone injected, the air-cells become injected long before the liquid reaches the bronchial membrane. In neither of these cases are the bronchial arteries, i.e. those derived from the aorta or veins which correspond to them, in the slightest degree injected.

7thly. The bronchial arteries, which are injected by filling the aorta, terminate in veins, which ramify in the subpleural cellular tissue; the greater part of these, after ramifying on the surface of the lung beneath the pleura, pass along the broad band of pleura, which extends from the peduncle of the lung to the posterior mediastinum, and encloses the root of the lung, and which may be called the mesopleura, and the veins then empty themselves into the oesophageal veins and other veins in the posterior mediastinum. It is probable also that some terminate in the azygos veins, the jugular veins, the diaphragmatic veins, and the vena cavae; in short, wherever they can meet with a systemic vein situated conveniently; but they form no sort of communication with the pulmonary veins, either in their capillaries or their larger trunks.

8thly. It is found that the coats of the lymphatic vessels of the lung are supplied by blood-vessels which are derived from the air-cells, and which terminate in the pulmonary veins, and the distribution of the blood-vessels on the coats of the lymphatics bears a strong resemblance to those distributed on the bronchial membrane.

The author then describes his apparatus for injection; the subjects injected; and the drawings which accompany his communication. In conclusion he states, that sufficient has been adduced to confute the opinion that there is one set of vessels for the nutrition of the lung in its ordinary acceptation, and another for the respiratory function. Without doubt the bronchial (so-called) vessels and the pulmonary are distinct, both as to their distribution and functions; the one being for the purposes of breathing, while the other solely supplies the cellular tissue of the organ.

"Theory of the reciprocal Action between the Solar Rays and
the different Media by which they are reflected, refracted or absorbed." By Joseph Power, Esq., M.A., Fellow of Clare Hall and Librarian of the University of Cambridge, &c.

For the train of thought which suggested the considerations in this communication, the author states that he is more particularly indebted to the researches of Professor Draper of New York, contained in his work "On the Organization of Plants, the Chemical effects of the Solar Rays," &c., his experiments tending to show that the law of action and reaction which prevails so generally in other departments of nature is no less true in all the varied phenomena of the sun-beam, so that the latter cannot be reflected, refracted, much less absorbed, without producing some effect upon the recipient medium. Whilst however he acknowledges the information he has received from that work, he differs in opinion with its author, as to the necessity of admitting more than one imponderable, being strongly of opinion that all the effects of the solar rays may be attributed to some or other of the infinite variety of undulations of which the universal æther is capable, and which, in the case of the sun-beam, are impressed upon it at the surface of the sun. He considers that the vis viva, which has its origin in these vibrations, is transmitted through the æther, with the velocity of light, in extremely minute undulations of different lengths and periods. If then a sun-beam, fraught with a vast variety of such undulations, be incident upon a medium so constituted that particles are capable of vibrating in unison, or even in harmonious consonance less perfect than unison, with some or other of the æthereal vibrations of the incident beam, it must necessarily happen that one system of vibrations will be called into existence by the other according to the laws of Resonance. He states that there may be a difficulty in explaining, but there can be no doubt of the fact, that the vis viva due to such induced vibrations may, like those of heat, become more or less persistent in the medium, producing at one time the phænomenon of fixed chemical action; at another that of permanently latent heat; at another that of less permanently latent or retarded heat; at another that of coloration and absorption; at another that of phosphenic action. The remarkable phænomena lately discovered by Professor Stokes seem to him to be closely allied to the latter, differing however in the circumstance that they cease to exist the moment the exciting rays are withdrawn. Guided by analogy, he is, however, inclined to think that these phænomena will be found hereafter to possess some slight though insensible duration, while he regards all action which is really momentary as expending itself upon the passing rays as they emerge in the form of reflected or refracted rays.

But all these effects, of whatever kind, the author regards as due to one and the same cause, which can be no other than the expenditure or distribution of the vis viva originally derived from the sun, and conveyed by the æther. Such expenditure he considers we may regard as of two kinds, according as the vis viva is retained by the medium, or transmitted with the emergent rays. If it be expended solely upon the emergent rays, the vis viva of the incident ray ought
to be exactly equal to the sum of the *vires vivae* of the reflected and refracted rays. But if it be partly expended on the medium and partly upon the emergent rays, the *vis vivae* of the incident ray ought to exceed the *vires vivae* of the reflected and refracted rays by a certain quantity. The object of his present investigation was to take into account the effect of such supposed excess, in the hope of arriving at some explanation of the Stokcsian phenomena. The remarkable result he has obtained, that every loss of *vis vivae* will be accompanied with a diminution of the refractive index, is quite in the direction of Professor Stokes's own idea of "a change of refrangibility," but throws no light on the change of period. This the author is inclined to think is due to an action of the nature of harmonic resonance, and from some calculations which he has made, he thinks it probable that the light produced in the Stokcsian experiments may be due to resonant vibrations which are about a major or minor third lower in pitch than those of the invisible rays producing them.

The mode of procedure which appeared to the author most likely to lead to a successful result, was to assume, in the first instance, the hypothesis that the *vis vivae* is expended solely on the reflected and refracted rays, and afterwards to modify, if possible, the steps of the process so as to adapt them to the hypothesis that a portion of it is expended on the medium. In adopting the more simple hypothesis, he was much struck by the formulæ at which he arrived; for not only did the general law of refraction spring out most unexpectedly, but the very same expressions for the intensity of the reflected rays, which were first discovered by Fresnel, and subsequently verified by the experiments of Brewster and Arago, were an immediate consequence of the formulæ. His results however differ in some particulars from those of Fresnel. In the first place, the index of refraction is not the simple quotient of the velocities of undulation, but of those velocities each multiplied by the density of the æther in the corresponding medium. In the second place, the vibrations of the aetheral particles are performed in the plane of polarization, and not perpendicular to that plane, as Fresnel supposed. Further, the expressions for the intensities of the refracted rays differ slightly in other respects from those of Fresnel, as given in Airy's Tracts.

The author states that he confines his attention to an isotropical singly refracting medium, though he thinks, if he had more time at his disposal, he could extend the theory to doubly refracting crystals. A very simple integration gives him a general expression for the *vis vivae* of an elementary cycloidal wave, in terms of the amplitude and the constants of the periodical function. By help of this he obtains two equations of *vis vivae*; one for a wave whose vibrations are in the planes of incidence, and the other for a wave whose vibrations are perpendicular to that plane, both vibrations being transverse to the axis of the ray. By the principle of superposition these two equations will hold true simultaneously when the above waves are regarded as the components of one and the same wave. He obtains three other equations between the amplitudes from the simple con-
sideration that a particle situated in the common surface of the two media cannot vibrate in more than one way at once. Of these three equations two involve the amplitudes of the first component wave, and the third those of the second. The five equations serve to determine, in terms of the angle of incidence and the component amplitudes of the incident wave, the five following quantities, namely, the angle of refraction, the two component amplitudes of the reflected wave, and those of the refracted wave.

By the aid of the result referred to, that every loss of *vis viva* is always accompanied by a diminution of the refractive index, coupled with the general view which he takes of the cause of absorption, the author is enabled to give an explanation of Frauenhofer's lines, and in general of the lines of absorption in coloured media; and also to explain the phenomena discovered by Sir David Brewster, that violet light may exist in the blue spaces, and blue light in the red.

In the course of the investigation, the altered expressions for the intensities of the reflected and refracted rays, so far as they are affected by the coefficient of absorption, are given. The expressions for the intensities of the two component reflected waves are very little affected; but those for the intensities of the two component refracted waves are materially altered in value.

The theory likewise affords an explanation, the first the author believes that has ever been offered, of the remarkable properties of saccharine solutions and of certain crystals, such as right-handed and left-handed quartz, which exhibit the phenomenon of circular polarization.

June 16.—The following papers were read:—

1. "On the Anatomy and Physiology of *Cordylophora*, a contribution to our knowledge of the Tubularian Zoophytes." By George James Allman, M.D., M.R.I.A., Professor of Botany in the University of Dublin, &c.

The author, after pointing out the necessity of giving greater definiteness to the terminology employed in the description of the true zoophytes, proceeds to the anatomical details of *Cordylophora*, a genus of *Tubulariidae*. He demonstrates that *Cordylophora* is essentially composed in all its parts of two distinct membranes enclosing a cavity, a structure which is common to all the *Hydroida*. For greater precision in description, he finds it necessary to give to these membranes special names, and he therefore employs for the external the name of *ectoderm*, and for the internal that of *endoderm*. Each of these membranes retains its primitive cellular structure. In the ectoderm *thread-cells* are produced in great abundance; these are formed in the interior of the ectodermal cells by a process of endogenous cell-formation, and are afterwards set free by the rupture of the mother-cell. The thread-cells in a quiescent state are minute ovoid capsules, but under the influence of irritation, an internal sac is protruded by a process of evagination; the surface of the evaginated sac is furnished with a circle of curved spicula, and from its free extremity a delicate and long filament is emitted. The thread-cells of *Cordylophora* thus closely resemble the "hastigerous organs" of *Hydra*.
The polypary is a simple unorganized secretion deposited in layers from the ectoderm. In the endoderm, the author points out a distinct and well-developed glandular structure composed of true secreting cells, which are themselves produced in the interior of mother-cells, and elaborate a brown granular secretion which he assumes as representing the biliary secretion of the higher animals. He describes, as a system of special muscles, certain longitudinal fibres, which may be distinctly seen in close connection with the inner surface of the ectoderm. The tentacula are shown to be continuous tubes communicating with the cavity of the stomach, and thus possess the same essential structure as those of _Hydra_; they are formed of a direct continuation of the ectoderm of the polype, lined by a similar continuation of the endoderm. The appearance of transverse septa at regular intervals, which is so very striking in these tentacula, must not be attributed to the existence of true septa. It is due to a peculiar condition of the endodermal layer, but the author has not been able to give a satisfactory explanation of it. Through the whole of the canal which pervades the axis of the stems and branches, a constant though a regular rotatory movement is kept up in the contained fluid; this movement is not due to the propulsive action of vibratile cilia, and is explained by the author as the effect of the active processes going on in the secreting cells of the endoderm, processes which can scarcely be imagined to take place without causing local alterations in the chemical constitution of the surrounding fluid, and a consequent disturbance in its stability.

The reproductive system of _Cordylophora_ consists of ovoid capsules situated on the ultimate branches at some distance behind the polypes; some of these capsules contain _ova_, others _spermatosoa_; they are plainly homologous with the ovigerous sacs of the marine _Tubulariidae_; they present a very evident, though disguised medusoid structure, having a hollow cylindrical body, whose cavity is continuous with that of the polype-stem, projecting into them below, and representing the proboscidiform stomach of a Medusa, while a system of branched tubes which communicate at their origin with the cavity of the hollow organ, must be viewed as the homologues of the radiating gastro-vascular canals, and the proper walls of the capsule will then represent the disc. From comparative observations made on other genera of _Hydroida_, the author maintains the presence of a true medusoid structure in the fixed ovigerous vesicles of all the genera he has examined, and he arrives at the generalization, that for the production of true _ova_ in the hydroid zoophytes, a particular form of zooid is necessary, in which the ordinary polype-structure becomes modified, and presents, instead, a more or less obvious medusoid conformation, _Hydra_ being at present the only genus which appears to offer an exception to this law, though the author believes that the exception is only apparent, and that further observations will enable us to refer the reproductive organization of this zoophyte to the same type with that of _Cordylophora_ and the marine _Hydroida_. The author has satisfied himself that the ova-like bodies
contained in the capsules of Cordylophora are true ova, and not gemma; he has demonstrated in them a distinct germinal vesicle, and has witnessed the phenomenon of yolk-cleavage; and the paper details the development of the embryo to the period of its escape from the capsule in the form of a free-swimming ciliated animacule, and traces its subsequent progress into the condition of the adult zoophyte.


The author remarks, that in treating a great problem of approximation, such as that presented to us by the investigation of the moon's motion, experience shows that nothing is more easy than to neglect, on account of their apparent insignificance, considerations which ultimately prove to be of the greatest importance. One instance of this occurs with reference to the secular acceleration of the moon's mean motion. Although this acceleration and the diminution of the eccentricity of the earth's orbit, on which it depends, had been made known by observation as separate facts, yet many of the first geometers altogether failed to trace any connexion between them, and it was not until he had made repeated attempts to explain the phenomenon by other means, that Laplace himself succeeded in referring it to its true cause.

The accurate determination of the amount of the acceleration is a matter of very great importance. The effect on the moon's place, of an error in any of the periodic inequalities, is always confined within certain limits, and takes place alternately in opposite directions within very moderate intervals of time, whereas the effect of an error in the acceleration goes on increasing for an almost indefinite period, so as to render it impossible to connect observations made at very distant times.

In the 'Mécanique Céléste,' the approximation to the value of the acceleration is confined to the principal term, but in the theories of Damoisau and Plana, the developments are carried to an immense extent, particularly in the latter, where the multiplier of the change in the square of the eccentricity of the earth's orbit, which occurs in the expression of the secular acceleration, is given to terms of the seventh order.

As these theories agree in principle, and only differ slightly in the numerical value which they assign to the acceleration, and as they passed under the examination of Laplace, with especial reference to this subject, it might be supposed that only some small numerical rectifications would be required in order to obtain a very exact determination of this value.

It has not been, therefore, without surprise, which he has no doubt will be shared by the Society, that the author has lately found that Laplace's explanation of the phenomenon in question is essentially incomplete, and that the numerical results of Damoisau's and Plana's theories, with reference to it, consequently require to be very sensibly altered.

Laplace's explanation may be briefly stated as follows. He shows
that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but also on the eccentricity of the earth's orbit. Now this eccentricity is at present (and for many ages has been) diminishing, while the mean distance remains unaltered. In consequence of this, the mean disturbing force is also diminishing, and therefore the moon's gravity towards the earth at a given distance is, on the whole, increasing. Also the area described in a given time by the moon about the earth is not affected by this alteration of the central force; whence it readily follows that the moon's mean distance from the earth will be diminished in the same ratio as the force at a given distance is increased, and the mean angular motion will be increased in double the same ratio.

This, the author states, is the main principle of Laplace's analytical method, in which he is followed by Damoiseau and Plana; but it will be observed that this reasoning supposes that the area described by the moon in a given time is not permanently altered, or in other words, that the tangential disturbing force produces no permanent effect. On examination, however, he remarks it will be found that this is not strictly true, and he proceeds briefly to point out the manner in which the inequalities of the moon's motion are modified by a gradual change of the disturbing force, so as to give rise to such an alteration of the areal velocity.

As an example, he takes the case of the variation, the most direct effect of the disturbing force. In the ordinary theory, the orbit of the moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the sun, and the areal velocity generated while the moon was moving from quadrature to syzygy, would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused, depends in some degree on the eccentricity of the earth's orbit; and as this is continually diminishing, the disturbing forces at equal intervals before and after conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction, and therefore the effects of the tangential force before and after conjunction no longer exactly balance each other.

The other inequalities of the moon's motion will be similarly modified, especially those which depend, more directly, on the eccentricity of the earth's orbit, so that each of them will give rise to an uncompensated change of the areal velocity, and all of these must be combined in order to ascertain the total effect.

Since the distortion of the orbit just pointed out is due to the change of the disturbing force consequent upon a change in the eccentricity of the earth's orbit, and the action of the tangential force permanently to change the rate of description of areas is only brought into play by means of this distortion, it follows that the alteration of the areal velocity will be of the order of the square of
the disturbing force multiplied by the rate of change of the square of the eccentricity. It is evident that this alteration of areal velocity will have a direct effect in changing the acceleration of the moon's mean motion.

Having thus briefly indicated the way in which the effect now treated of originates, the author proceeds with the analytical investigation of its amount. In the present communication, however, he proposes to confine his attention to the principal term of the change thus produced in the acceleration of the moon's mean motion, deferring to another, though he hopes not a distant opportunity, the fuller treatment of this subject, as well as the determination of the secular variations of the other elements of the moon's motion, which, arising from the same cause, have also been hitherto overlooked.

In the usual theory, the reciprocal of the moon's radius vector is expressed by means of a series of cosines of angles formed by combinations of multiples of the mean angular distance of the moon from the sun, of the mean anomalies of the moon and sun, and of the moon's mean distance from the node; and the moon's longitude is expressed by means of a series of sines of the same angles, the coefficients of the periodic terms being functions of the ratio of the sun's mean motion to that of the moon, of the eccentricities of the two orbits and of their mutual inclination.

Now, if the eccentricity of the earth's orbit be supposed to remain constant, this is the true form of the expressions for the moon's coordinates; but if that eccentricity be variable, the author shows that the differential equations cannot be satisfied without adding to the expression for the reciprocal of the radius vector, a series of small supplementary terms depending on the sines of the angles whose cosines are already involved in it, and to the expression for the longitude, a series of similar terms depending on the cosines of the same angles; all the coefficients of these new terms containing as a factor the differential coefficient of the eccentricity of the earth's orbit taken with respect to the time.

The author first determines as many of these terms as are necessary in the order of approximation to which he restricts himself, and then takes them into account in the investigation of the secular acceleration. The expression which he thus obtains for the first two terms of this acceleration, is

\[-\left(\frac{3}{2}m^2 - \frac{371}{64}m^4\right)\int (e^{i\omega} - E^{i\omega})dt.\]

According to Plana, the corresponding expression is

\[-\left(\frac{3}{2}m^2 - \frac{2187}{128}m^4\right)\int (e^{i\omega} - E^{i\omega})dt.\]

It will be observed that the coefficient of the second term has been completely altered in consequence of the introduction of the new terms.

The numerical effect of this alteration is to diminish by 1''·66 the Phil. Mag. S. 4. Vol. 6. No. 38. Sept. 1853. Q
coefficient of the square of the time in the expression for the secular acceleration; the time being, as usual, expressed in centuries.

It will, of course, be necessary to carry the approximation much further in order to obtain such a value of this coefficient as may be employed with confidence in the calculation of ancient eclipses.

In conclusion, the author states, that the existence of the new terms in the expression of the moon's coordinates occurred to him some time since, when he was engaged in thinking over a new method of treating the lunar theory, though he did not then perceive their important bearing on the secular equation. His attention was first directed to this subject while endeavouring to supply an omission in the Theory of the Moon given by Pontécoulant in his 'Théorie Analytique.' In this valuable work, the author, following the example originally set by Sir J. Lubbock in his tracts on the lunar theory, obtains directly the expressions for the moon's coordinates in terms of the time, which are found in Plana's theory by means of the reversion of series. With respect to the secular acceleration of the mean motion, however, Pontécoulant unfortunately adopts Plana's result without examination. On performing the calculation requisite to complete this part of the theory, the author was surprised to find that the second term of the expression for the secular acceleration thus obtained, not only differed totally in magnitude from the corresponding term given by Plana, but was even of a contrary sign. His previous researches, however, immediately led him to suspect what was the origin of this discordance, and when both processes were corrected by taking into account the new terms whose existence he had already recognized, he had the satisfaction of finding a perfect agreement between the results.

3. "On the frequent occurrence of Indigo in Human Urine, and on its Chemical and Physiological Relations." By Arthur Hill Hassall, M.D., Physician to the Royal Free Hospital, &c.

The author was led to the investigations laid before the Society in the above communication, by the following circumstances: —

Some three or four years since, when examining urinary deposits under the microscope, he frequently observed in the field of vision particles of a deep blue colour; so often did this occur, that he could not even then help suspecting that their presence was not accidental; however, no analysis of the blue colouring matter was made at that time, and the circumstance was in a fair way of being forgotten, until the recollection of it was revived by another occurrence.

In June 1852, a sample of urine, freely exposed to the air in an open vessel, was observed after four or five days' exposure, gradually to change colour; the pellicle or scum which had formed upon the surface of the urine became first slate-coloured, and at length deep blue, with here and there a rusty-red tint. The urine also underwent at the same time some remarkable changes; it became thick and turbid, deep brown, greenish, bluish-green, and finally of a faded yellowish-green colour; a considerable sediment was found at the bottom of the glass; this was deep brown, intermixed with a
little blue colouring matter, and it had a medicinal smell resembling somewhat that of Valerian. In this state, without undergoing any further material changes, the urine remained for many days.

Examined with the microscope, the scum or pellicle on the surface was found to consist of vibriones, innumerable animalcules, and crystals of triple phosphate, with a great many fragments and granules of a deep and bright blue colour.

A second sample of the same urine was therefore procured, taking every precaution to avoid fallacy. Gradually the same changes ensued as in the first sample, and this likewise became blue. Having thus ascertained that the changes observed were due to something contained in the urine itself, the author next proceeded to set aside in open vessels, a series of urines all from the same patient, noting the alterations which occurred from day to day; these samples underwent nearly similar changes; but the quantity of blue colouring matter and brown extractive gradually diminished, until at length they were present in such small amount as to be visible only under the microscope, and at last they entirely disappeared.

The results obtained from an examination of the urine, the blue colouring matter and the brown extractive, are then given by the author; they are as follow:—

The urine.—The urine of the second sample at the time of analysis, when shaken, had a dark, greenish-brown colour; but on standing at rest for some time, the colouring matter fell to the bottom, forming bluish-green flocculi, while the supernatant liquid was of a deep wine-red colour. The bottle was set aside corked, for ten days, at the end of which time the bluish-green precipitate had entirely disappeared; but on removing the cork, and allowing free access of air for some time, the coloured deposit was again produced. This was washed with water, drenched with hydrochloric acid, and finally dried; by this means was obtained a rich blue powder possessing all the chemical characters and properties of indigo.

The urine that was filtered off from the above precipitate was allowed to evaporate spontaneously, by which means it yielded an additional quantity of indigo, which adhered in the form of very small flakes to the sides of the dish. It also gave a rather large proportion of a deliquescent brown colouring matter, which was treated, first with alcohol, and then with water.

The alcohol acquired a deep brownish-red colour, and the water a dark brownish-green. Both of these solutions were evaporated at a temperature of 160° Fahr. The alcoholic solution furnished a rich brown extractive which was soluble in water, but not in dilute acids, and nitric acid did not produce that play of colours which is characteristic of bile pigment; nor did the precipitate formed with basic acetate of lead furnish a purple liquid with alcohol and free acid. A strong solution of potash dissolved the extractive and yielded a deep blood-red fluid, which was rendered green and opalescent by boiling. These reactions show that the brown pigment was somewhat like hæmatine in its chemical manifestations.

While the aqueous solution of the brown matter was undergoing
evaporation, it gave a further supply of indigo, which was formed most freely at the edge of the liquid. The residue was made black by concentrated sulphuric acid and deep brown by potash.

The blue colouring matter.—Of two samples of this in a dry state, mixed with a large quantity of earthy phosphates, vibronics, mucus, and epithelium, one gave a dark brown solution with concentrated sulphuric acid, and the other a dirty blue. Both of these solutions were decomposed by water, furnishing in the former case a dark brown deposit, and in the latter a dirty green. In most of their other reactions, however, they presented the characters of indigo; and it is especially deserving of notice, that they were reduced by lime and sugar, giving a liquid from which hydrochloric acid threw down a greenish-blue precipitate.

The cause of concentrated sulphuric acid giving with one of these samples a brown solution, and with the other only a dirty blue, was, the author considers, mainly owing to the large quantity of animal matter with which the specimens were contaminated; the acid, from its charring effect on this, would produce a brown or blackish solution, thus obscuring the colour of the solution of indigo.

The brown extractive.—The brown extractive yielded nearly the same results as on its first analysis, and the aqueous solution furnished a few blue flocculi. A portion of the alcoholic extract was heated with Liq. Potassae for the purpose of ascertaining whether it contained leucine, and the product, on being treated with hydrochloric acid, gave off a powerful odour, which was somewhat like valerianic acid; but the result was too doubtful to be of much value. The author had already referred to the peculiar smell of Valerian emitted by the extractive of more than one of the samples. He considers that the clearest and most positive evidence was thus obtained that the blue colouring matter in this case was indigo.

It was not very long after the occurrence of the first case of blue urine that numerous other instances fell under the author's observation. The urines of all these cases underwent very nearly the same changes as in the first; in some, the quantity of blue colouring matter found was very considerable, in others less; and in the third class of cases the microscope was necessary for its discovery. In nearly all these cases the blue colouring matter was submitted to analysis, and ascertained, on the clearest evidence, to be indigo.

The author in the next place considers the question of the source and origin of indigo in the urine.

It appeared that in the cases related by the author, coloured indigo was not present in urine when first voided, but that it was gradually formed some time afterwards by a process of oxidation on exposure to the air, being in most of the cases probably derived from the brown extractive, which in its chemical manifestations so closely resembles haematin.

The author contrasts cyanourine with the indigo detected in urine. He observes that the most distinctive tests laid down for cyanourine are its solubility in boiling alcohol, and the action of sulphuric acid upon it, which give a reddish-brown solution; and states he had
ascertained that these tests are not to be relied upon, since indigo, when contaminated (as in the urine it frequently is) with a large quantity of animal matter, vibrones, &c., gives a reddish-brown solution with concentrated sulphuric acid, from the charring of the animal matter, and in many cases forms a bright blue solution with boiling absolute alcohol; hence he could not help suspecting that cyanourine and indigo are very closely connected with each other, if they be not identical. He observes, it is at least singular that while so many cases of indigo were met with, not a single instance of cyanourine presented itself. He also contrasts indigo with apo-glaucine, and remarks that this is acknowledged by Heller himself to be nothing more than cyanourine mixed with urrhodine.

Taking then into consideration the whole of the facts described in this communication, the following conclusions are deduced:—

1st. That blue indigo is frequently formed in human urine, the quantity being subject to the greatest variation; in some cases it is so considerable as to impart a deep green, or bluish-green colour to the whole urine; a pellicle of nearly pure indigo also extending over the entire surface of the liquid; while in others it is so small that it can only be detected by means of the microscope.

2nd. That for the formation of this indigo, it is in general necessary that the urine should be exposed to the air for some days in an open vessel, oxygen being absorbed and the blue indigo developed. Whatever facilitates therefore oxygenation, as free exposure to light and air, warmth and sunshine, hastens the appearance of the blue indigo; hence in summer the changes described take place much more quickly than in winter; on the contrary, these changes are retarded and even stayed by exclusion of the atmosphere. Blue indigo may even be deprived of its colour and reformed, alternately, according as air is excluded or admitted to urine containing it. From some of the cases recorded, it would appear, however, that blue indigo is occasionally formed in the system, and is voided as such in the urine.

3rd. That there is usually found with the blue indigo, where the amount of this is considerable, a brown extractive, sometimes in large quantity, the aqueous solution of which, by exposure to air, yields a further supply of coloured indigo, and which closely resembles hæmatine in its chemical manifestations and elementary composition. There is therefore great reason for believing that in the majority of the cases here recorded, the blue indigo was derived from altered hæmatine, although it is at the same time probable, that in some cases it is formed from modified urine pigment which is itself supposed to be a modification of hæmatine. Between the greater number of the animal colouring matters there is the closest relationship in chemical composition, so that the transformation of the one into the other would appear to be both easy and natural.

4thly. That the urines in which the coloured indigo occurs in the largest quantity, are usually of a pale straw colour, readily becoming turbid, alkaline, and of low specific gravity. Small quantities
of indigo are however frequently found in urines possessing characters the very reverse, that is, in such as are high-coloured, acid, and of high specific gravity; but, as a rule, in these urines the blue pigment is usually absent.

5thly. That as coloured indigo does not occur in healthy urine, and since where the amount of this is at all considerable it is accompanied with strongly-marked symptoms of deranged health, the formation of blue indigo in urine must be regarded as a strictly pathological phenomenon, apparently associated rather with some general morbid condition, than essentially with disease of any one organ; although there is reason for believing that the blue deposit is met with very frequently in Bright's disease, and in affections of the organs of respiration, it should however be remarked that none of the worst cases of indigo in the urine which the author met with were cases of Bright's disease.

The paper is illustrated by drawings, and a specimen of the indigo, as deposited from urine, was exhibited.


The authors had already proved by experiments conducted on a small scale, that when dry atmospheric air, exposed to pressure, is made to percolate a plug of non-conducting porous material, a depression of temperature takes place increasing in some proportion with the pressure of the air in the receiver. The numerous sources of error which were to be apprehended in experiments of this kind conducted on a small scale, induced the authors to apply for the means of executing them on a larger scale; and the present paper contains the introductory part of their researches with apparatus furnished by the Royal Society, comprising a force pump worked by a steam-engine and capable of propelling 250 cubic inches of air per second, and a series of tubes by which the elastic fluid is conveyed through a bath of water, by which its temperature is regulated, a flange at the terminal permitting the attachment of any nozzle which is desired.

Preliminary experiments were made in order to illustrate the thermal phenomena which result from the rush of air through a single aperture. Two effects were anticipated, one of heat arising from the vis viva of air in rapid motion, the other of cold arising from dilatation of the gas and the consequent conversion of heat into mechanical effect. The latter was exhibited by placing the bulb of a very small thermometer close to a small orifice through which dry atmospheric air, confined under a pressure of 8 atmospheres, was permitted to escape. In this case the thermometer was depressed 13° Cent. below the temperature of the bath. The former effect was exhibited by causing the stream of air as it issued from the orifice to pass in a very narrow stream between the bulb of the thermometer and a piece of gutta percha tube in which the latter was enclosed. In this experiment, with a pressure of 8 atmospheres, an elevation of temperature equal to 23° Cent. was observed. The same
phenomenon was even more strikingly exhibited by pinching the rushing stream with the finger and thumb, the heat resulting therefrom being insupportable.

The varied effects thus exhibited in the "rapids" neutralize one another at a short distance from the orifice, leaving however a small cooling effect, to ascertain the law of which and its amount for various gases, the present researches have principally been instituted. A plug of cotton wool was employed, for the purpose at once of preventing the escape of thermal effect in the rapids, and of mechanical effect in the shape of sound. With this arrangement a depression of 0°.31 Cent. was observed, the temperature of the dry atmospheric air in the receiver being 14°.5 Cent., and its pressure 34·4 lbs. on the square inch, and the pressure of the atmosphere being 14·7 lbs. per square inch.

ROYAL INSTITUTION OF GREAT BRITAIN.


In what is termed mineral chemistry, chemical substances are classified according to the different nature of the elements of which they consist. But in organic chemistry this distinction is no longer available. Organic substances were formerly defined as triple compounds of carbon, hydrogen and oxygen; and this, with the statement of the relative proportion of these elements in any given compound, was all that was attempted to be made out as to its constitution. But this class of bodies is more numerous possibly than all the other chemical substances taken together with which we are acquainted, and some further distinction was necessary for the purposes of science. The sagacity of certain chemists at length discovered a relation which was capable of becoming the basis of a truly rational and natural classification. It was perceived, that in the long series of chemical changes of which these bodies were susceptible, the whole of the substance did not change, and that in these combinations certain groups of elements had the same persistent character and fulfilled the same chemical function as the simple elements themselves in other bodies. These constant groups have been named radicals. Among these, those hydrocarbons termed the homologues of hydrogen are of special interest. Æthyle, a group consisting of 2 atoms of carbon and 5 of hydrogen, C₂H₅, is one of these bodies. Assuming water as 2 atoms of hydrogen and 1 of oxygen, HHO, alcohol is composed of 1 atom of æthyle, 1 of hydrogen, and 1 atom of oxygen, (C₂H₅)HO. Hydriodic acid, the iodide of hydrogen, consists of an atom of hydrogen combined with an atom of iodine, HI. The iodide of æthyle consists of an atom of æthyle combined with an atom of iodine, (C₂H₅)I. It is from these, and other like analogies between hydrogen and æthyle, that the idea arose of the similarity in their chemical function.

Certain chemists, however, conceived these views to be mere fan-
ciful speculations. Their principal objection, reasonable or not, was that this æthyle was a purely ideal substance. From hydrochloric acid, or from water, we readily procure hydrogen. We separate metals from their combinations; but æthyle could not thus be obtained, and there was a point where it seemed that this analogy failed. Frankland however has silenced this objection in the most satisfactory manner, namely by procuring and isolating this æthyle.

He prepared it by a modification of the form of experiment by which hydrogen itself is prepared. He placed together zinc and iodide of æthyle in tubes hermetically sealed, and heated them considerably above the boiling-point of water. On opening the tubes, the æthyle escapes as a colourless combustible gas. There is only one property of æthyle on which I need dwell—its specific weight; it is about twice the weight of air.

Æthyle, however, when procured, did not realize all the anticipations formed of it, and there was one very important difference between the actual and the anticipated æthyle. It was supposed that when zinc acts upon iodide of hydrogen, it takes away (so to say) the iodine, and the hydrogen becomes what is termed free, and the same with æthyle. On this view, æthyle would have a certain atomic constitution, C²H³. Now there is much reason to believe, that in the gaseous form the molecules of all bodies occupy the same space, whether this molecule consist of 2 only, or, as may be the case, of 100 atoms. Hence to ascertain of how many atoms the molecule of a substance consists, we have simply to compare its weight in the gaseous form with that of some other gas of which the molecule is already determined. When this experiment was made with æthyle, it was found to be just twice as heavy as it should be; that is to say, the space which should have contained 2 atoms of carbon and 5 of hydrogen was found to contain just twice that quantity, or C⁴H⁶.

Some chemists considered that æthyle was an exception to the general rule, and that the molecule of æthyle only occupied half the space of the molecule of other bodies, so that the same space which contained 1 molecule of water truly contained 2 molecules of æthyle. This however is evidently but an arbitrary assumption to meet the case. Others said that, after all, the true æthyle remained yet to be discovered, and that this body was not it, but a hydrocarbon isomeric with it, for that the real æthyle would have only half the density of this body.

There is however a third view, on which the æthyle of theory is also the æthyle of fact. On a former occasion, I showed reasons for believing that the elements are in a certain sense compound molecular groups, consisting of 2 or more atoms, which (in the present state of our knowledge) we must regard as similar, united to form a compound molecule. On this idea, the gas hydrogen is represented, not by the symbol H, but as HH; and æthyle, the analogue of hydrogen, would also consist of a double atom, and be represented, not as C²H³, but as C²H³ C²H³.

The old view, however, had always a certain advantage over this,
in the clear and consistent account which it gave of the mode of formation of hydrogen. How is it, it may be asked, and by what process, that this compound atom of hydrogen is formed? The answer is by no means obvious. Indeed the investigation of the nature of the process by which æthyle was formed alone gave the key to its solution.

Æthyle is not, in truth, made by the direct action of zinc upon the iodide of æthyle; but by the intervention of another body, which belongs to the class of what I may term fugitive or evanescent combinations, and which is made and decomposed again in the course of the experiment. This body is zincæthyle. The molecule of zinc, consisting of 2 atoms, ZnZn, splits into two parts. 1 atom, Zn, combines with the iodine of the iodide of æthyle, C₂H₃I, to form iodide of zinc, ZnI, while the other atom at the same moment combines with the æthyle, forming zincæthyle, Zn C₂H₃.

The mode of action of zincæthyle upon iodide of æthyle is perfectly analogous to its action upon water. In contact with water, HHO, it immediately decomposes, forming hydrated oxide of zinc, ZnHO, and hydride of æthyle, C₂H₃H. This hydride of æthyle has hardly more than half the density of the æthyle gas. In the same space in which, in the other case, are contained 2 heavy atoms of æthyle, are here contained 1 heavy atom of æthyle and 1 light atom of hydrogen. Now the zincæthyle with the iodide of æthyle decomposes in a perfectly similar manner, forming iodide of zinc, ZnI, and æthyle gas, C₂H₃ C₂H₅. That this is truly the mode of the formation of the æthyle is proved by the fact, that by careful modification of the experiment, it is possible to break up this process of the formation of the æthyle into the two factors (so to say) of which it consists*. At a low temperature the zincæthyle alone is formed. At a higher temperature the zincæthyle disappears, and the æthyle is produced.

It is evident that the formation of the compound molecule of hydrogen, HH, must be a very different physical event to the formation of the single atom of hydrogen, H, if such could exist. The ordinary hypothesis of the “liberation” of hydrogen gives us no conception of its nature; we need some other explanation.

It seems to me probable, that when zinc acts on hydrochloric acid and water, there are, as in the case of æthyle, two steps in the process,—the first, the formation of a zinchydrogen, ZnH; the second, the action of this zinchydrogen on the water, with the formation of hydrated oxide of zinc, ZnHO, and hydrogen gas, HH. There are various arguments in favour of this view. First, it explains the result, which the other hypothesis does not. Secondly, the analogy of æthyle compels us to it. It is not probable that bodies so similar in other respects are dissimilar in the mode of their formation. Thirdly, there is at least one experiment in which we are absolutely able to analyse the process of the formation of hydrogen, and to prove that it does take place in this manner.

This remarkable experiment is the formation of hydrogen by the

decomposition of hypophosphorous acid by copper salts*. Hypophosphorous acid is, like zinc, what is termed a reducing agent. It precipitates certain metals from their solutions, and by a process of deoxidation decomposes alcalies with the formation of phosphorous acid and hydrogen. If this hypophosphorous acid be boiled with the copper salt, nothing is perceived but the formation of metallic copper and hydrogen gas; but if the solution be gradually heated, and the action arrested at a certain point, it can be shown that this formation of hydrogen is preceded by the formation of a combination of hydrogen and copper, CuH, analogous to zincethyle. The part which this bears in the formation of the hydrogen is distinctly shown by the action of acids, hydrochloric acid, HCl, for example, upon it. This acid, which does not act upon metallic copper, immediately decomposes this body, forming protochloride of copper, CuCl, and hydrogen, HH.

This hydride of copper has only a very ephemeral existence. It is decomposed very nearly at the same temperature at which it is produced, and its formation for this reason had long been overlooked by chemists. We can hence readily comprehend that other combinations of this class may take place in the case of which the temperature of formation and of decomposition may either coincide, or so closely approximate to each other, that it may ever be impossible to isolate the substance produced. This is probably the case in the action of zinc.

XXXV. Intelligence and Miscellaneous Articles.

ON THE INFLUENCE OF THE EARTH'S ROTATION ON THE MOTION OF A CONICAL PENDULUM. BY M. A. BRAVAIS.

On the 10th of February, 1851, I had the honour to notify to the Academy, that the unequal duration of the vibrations of a right- and left-handed pendulum appeared to me to be a necessary consequence of the beautiful experiment of M. Foucault on the deviation of the plane of oscillation. (See p. 197 of the preceding volume of this Journal.)

M. Arago having had the goodness to place the great meridian-room of the observatory at my disposal, I was able to make the experiments necessary to the testing of this point, with a pendulum 10 metres long; and thus not only to confirm the predictions of theory, but to do it with a degree of accuracy which I had not previously ventured to hope for.

I applied two different methods, that of direct observation and that of coincidences. In the direct observations, I observed the duration of 900 to 1200 left-handed oscillations (from west through south to east), and immediately afterwards the deviation of an equal number of right-handed ones. The distance of the point of suspension from the centre of the weight amounted to from 10,187 to 10,197 millims. The observed duration was as follows:—

Intelligence and Miscellaneous Articles. 235

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<td>6°39823</td>
<td>0°00064</td>
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<tr>
<td>11th of May</td>
<td>6°39925</td>
<td>6°39849</td>
<td>0°00076</td>
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<tr>
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<td>6°39815</td>
<td>6°39751</td>
<td>0°00064</td>
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<td>18th of May, Wire A</td>
<td>6°39959</td>
<td>6°39863</td>
<td>0°00096</td>
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<tr>
<td>16th of May, Wire B</td>
<td>6°40106</td>
<td>6°40032</td>
<td>0°00074</td>
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<tr>
<td>18th of May, Wire B</td>
<td>6°40116</td>
<td>6°40044</td>
<td>0°00072</td>
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Mean difference... 0°00074

As the difference given by theory is 0·000716, the agreement may be regarded as satisfactory.

The second method admits of far greater exactitude. Two similar pendulums, but differing somewhat in length, were suspended in the plane of the meridian at 7 decims. distance from each other. The distance of the point of suspension from the centre of the weight amounted, in the long pendulum, to 10,216 millims., and in the short pendulum to 10,115. The conical rotations, one right and the other left, were impressed upon both pendulums simultaneously, and the simultaneous passage of their suspending wires across the meridian was observed by a telescope, the optic axis of which, when the pendulums were at rest, intersected both wires. The number of oscillations $n, n'$ of the two pendulums accomplished between two coincidences was noted; and we thus had $n' = (n + 1)$, where $n'$ refers to the shorter pendulum.

When the series was finished, the pendulums were arrested, and set in motion in the opposite directions; the coincidences were again observed, and the numbers $N$ and $N' = (N + 1)$ were determined.

Calling the latitude $\lambda$, the duration of a sidereal day $T$, the duration of an oscillation of the longer pendulum released from the action of the earth $t$, the same duration of the shorter pendulum $t'$, then if the theory be correct we must have

$$\frac{\sin \lambda}{T} = \frac{1}{t + t'} \left\{ \frac{1}{n + n'} - \frac{1}{N + N'} \right\}.$$  

The experiment on the 25th of May gives

$n = 207.86$ \hspace{1cm} $n' = 208.86$ \hspace{1cm} $N = 217.82$ \hspace{1cm} $N' = 218.82$

The experiment on the 10th of June gives

$n = 206.31$ \hspace{1cm} $n' = 207.31$ \hspace{1cm} $N = 215.96$ \hspace{1cm} $N' = 216.96$.

From these experiments we may deduce the difference in the time of rotation between a right- and left-handed motion; applying the necessary corrections, we find—

Difference on the 25th of May \ldots \ 0''.000725
Difference on the 10th of June \ldots \ 0''.000710.
Intelligence and Miscellaneous Articles.

I wished to ascertain whether the presence of iron exerted any influence upon the results, and therefore enclosed the mercury of one of my copper cylinders in a cylinder of iron which weighed 900 grms. For the difference of both oscillations we now obtained

\[ 0''000702; \]

hence the influence of the iron may be regarded as almost null.

From the whole number of my observations, I conclude that with a pendulum 10 metres long, when it rotates conically from west to east, the angular velocity at Paris is retarded 11''-4 per second of time; on the other hand, when the motion is from east to west, the velocity is increased by the same amount.

From these observations the length of the simple seconds pendulum may be deduced when the following corrections are taken into account:

1. The effect of temperature.
2. The loss of weight by the air; according to Dubuat, Bessel and Poisson, the motion of this fluid must also be taken into account.
3. The sinking of the centre of oscillation under the centre of gravity.
4. The diurnal course of the chronometer or of the clock.
5. The effect produced by the weight of the suspension-wire on raising the centre of oscillation, an effect which in my experiments amounted to 10'6 millims.
6. Finally, the bending of the wire to the form of a catenary by the simultaneous action of its weight and its centrifugal force. From this it follows that the lowest tangent to the wire, prolonged to the axis of rotation, would intersect the latter at a point which must be regarded as the true centre of the upper suspension of the wire. I find by calculation that a subtractive correction is thus rendered necessary, which is almost the same as the foregoing, and in my experiments rises to 10'5 millims.

Applying these corrections, I find the length of the simple seconds pendulum at Paris = 993'77 millims., which differs but little from 993'86 millims., the length assumed.

It is important to remark, that the correction, on account of the bending of the wire, as far as I know, has never been applied to the plane oscillations of a pendulum suspended by a wire; that made use of by Borda in 1792, for example, had no such correction applied to it. This correction, however, seems just as necessary in the case of the plane swinging pendulum as in that of the conical one. As the value of this correction may be very perceptible, the subject is well worthy the attention of the calculator and of the experimental physicist.—Comptes Rendus, vol. xxxiii. p. 195.

EXAMINATION OF A RED VARIETY OF MOLYBDATE OF LEAD FROM PHENIXVILLE, U.S. BY CHARLES M. WETHERILL, PH.D.

In the Proceedings for the 30th March of this year is a description of a red variety of molybdate of lead, examined qualitatively
by me, and in which it is stated that I could not detect chromium either by the moist way or before the blowpipe. In the July number of Silliman's Journal, page 105, among the mineralogical notices by Mr. W. T. Blake, he observes that the specimens from the Wheatley mine, which he mistook for chromate of lead, have been found to be the molybdate containing chromic acid. I find also in Dana's Mineralogy, under the head of this mineral, the observation that "a red variety contains a few per cent. of chromic acid." In my former analysis I looked particularly for this acid without finding it. Supposing that Mr. Blake had been more fortunate than myself, I repeated the examination on larger quantities of mineral, and with great care, but have no reason to change what was before written. About 1 decigrm. of the pulverized molybdate was dissolved in boiling concentrated hydrochloric acid. No chlorine could be detected, as would have been the case were chromic acid present. After boiling for some time to reduce the hypothetical acid to sesquioxide of chromium, enough water was added to dissolve the formed chloride of lead, and then sulphuric acid in excess. The solution was yellow, and when drops of the liquid had reached the hot sides of the capsule, the blue molybdenum compound was formed, which gave with the yellow solution a green tinge to those parts. The lead being thus separated in combination with sulphuric acid, to the filtrate was added hydro sulphuret of ammonium, which kept in solution the sulphuret of molybdenum thus formed, and precipitated sulphuret of iron, which should contain sesquioxide of chromium, if chromic acid were in the mineral. This sulphuret of iron was removed from the solution, oxidized by nitric acid, precipitated hot by ammonia, then washed, and the ashes of the filter fused with nitre and carbonate of soda. The result was the slightest shade greenish, as if from the presence of minute traces of manganese, but not in the least yellow, nor did it give a yellow solution with water; nor did the solution, neutralized exactly by ammonia and nitric acid, give, as would be the case with chromic acid, the purple-red precipitate of the chromate of silver on addition of the nitrate. The reactions of chromium are so decided, that the presence of this metal could hardly escape the notice of one looking for it. I have not yet been able to obtain a sufficient quantity of the mineral for a quantitative examination. This compound may possibly be an acid molybdate, analogous to many chromates, which when neutral are yellow and red when acid.—Proc. Franklin Institute, 1853.

ON THE PRETENDED OCCURRENCE OF IODINE IN MILK, EGGS, AND THE ATMOSPHERIC AIR. BY DR. LOHMEYER.

Some years since, Chatin instituted some investigations upon the distribution of iodine; he not only found it in all spring-waters, in fresh water and land plants, in the most various articles of food, in artificial beverages such as wine and beer, but, according to him,
it constantly occurs also as an essential constituent in the inhabitants of our rivers and in land animals.

As it was shown, about the same time, by Meyrac, Marchand and others, that the distribution of iodine is much more considerable than had hitherto been supposed, it could excite no surprise that it should be found in the animal organism; here, as everywhere in nature, it was to be regarded as the constant companion of the chlorine compounds.

Chatin, however, regards iodine not as an accidental, but as an essential constituent of the organs; and according to his statements with respect to the quantities of it contained in eggs, milk, &c., he appeared to be perfectly right in so doing. According to Chatin, a hen's egg contains more iodine than 1 litre of milk, whilst this again contains more than our usual articles of food and beverage. He also considers iodine to be of importance in the respiratory process. Normally the air contains 1·500 milligrm. of iodine in 4000 litres, and during respiration 4·5 are said to disappear. Fourcault, who has occupied himself with the study of the causes of goitre and cretinism, examined the air in those places where these diseases are indigeneous; in these localities he found the atmosphere free from iodine, and concluded accordingly that the deficiency of iodine was the principal cause of goitre and cretinism. Chatin immediately proved that the air of the Alps was much poorer in iodine than the atmosphere of Paris; he found less iodine in the valley of the Rhone than in that of the Seine, less still in the valley of the Isère, and a constant diminution towards the Alps. He only met with it again in the plain of Piedmont, after it had eluded his investigations on the French side of the Alps.

As therefore the influence of iodine appears to be of the greatest importance to the development and continuance of the animal organism, the author regarded the testing of Chatin's statements as of sufficient importance to undertake it himself with great care.

Goitre does not occur in Göttingen, whilst in some of the neighbouring villages, in Lengden, for instance, it is very frequently met with. If the want of iodine in the air were the cause of the evil, it might be expected that a difference would be presented between the quantity of iodine contained in the air of Göttingen and that of Lengden. The author therefore examined first the air of Göttingen; he allowed 4000 litres of air to pass in small bubbles through a stratum of solution of caustic soda 18 inches in thickness, adding quicklime to the solution from time to time to remove the carbonic acid which was taken up by it. The apparatus was so arranged that the passage of 4000 litres of air required nearly a whole week, so that no iodine could be lost. Nevertheless, on testing for iodine, a negative result was obtained, so that an investigation of the air of Lengden must appear perfectly unnecessary *.

The author has sought for iodine in cow's milk and hen's eggs

* The same result was obtained by Mr. S. Macadam of Edinburgh (see Chem. Gaz. for Aug. 2, 1852), with whose researches the author does not appear to be acquainted.—Ed.
with a similar result. Thus no trace of iodine was to be found in 12 eggs or in 600 or 6700 grms. of milk. The fluids in which the iodine must be concentrated amounted in all cases only to a few centimetres; for testing them, cold starch-paste and pure nitric acid into which nitrous acid had previously been passed were employed.

The author does not deny, that, by the employment of still larger quantities of milk and eggs, the exhibition of iodine may perhaps be possible, and that under certain circumstances iodine may also occur in the atmosphere, especially in the neighbourhood of manufactories and chemical laboratories; but from his experiments he considers that he may draw the conclusion, that Chatin's statements are to be considered as erroneous throughout, and that the absence of iodine in the air cannot be the cause of goitre and cretinism.—Nachrichten von der Gesellsch. der Wiss. zu Göttingen, 1853, p. 131.

METEOROLOGICAL OBSERVATIONS FOR JULY 1853.


Mean temperature of the month .................................. 61°-99
Mean temperature of July 1852 .................................. 67'61
Mean temperature of July for the last twenty-seven years 63'22
Average amount of rain in July ................................... 2-29 inches.


Sandwich Manse, Orkney.—July 1. Bright a.m.: drops p.m. 2. Showers a.m.: fair p.m. 3. Rain a.m.: fair p.m. 4, 5. Cloudy a.m.: rain p.m. 6. Bright a.m.: cloudy p.m. 7. Drops a.m.: cloudy p.m. 8. Bright a.m.: cloudy p.m. 9. Clear a.m.: cloudy p.m. 10, 11. Bright a.m.: cloudy p.m. 12. Bright a.m.: clear p.m. 13. Clear a.m.: cloudy p.m. 14. Clear a.m.: rain p.m. 15. Showers a.m.: cloudy p.m. 16—18. Bright a.m.: clear p.m. 19. Bright, fine a.m.: clear p.m. 20. Rain a.m. and p.m. 21. Damp a.m. and p.m. 22. Damp a.m.: clear p.m. 23. Clear, fine a.m.: cloudy p.m. 24. Rain a.m.: clear p.m. 25. Bright a.m.: clear p.m. 26. Showers a.m.: damp p.m. 27. Showers a.m. and p.m. 28. Fine a.m.: clear p.m. 29. Drizzle a.m.: clear p.m. 30. Bright a.m.: fair p.m. 31. Bright a.m.: fine p.m.

Mean temperature of July for twenty-six previous years .... 54°-93
Mean temperature of July 1852 .................................. 61·36
Mean temperature of this month .................................. 58·15
Average quantity of rain in July for seven previous years ... 2·69 inches.
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<td>29'60</td>
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| Mean.        | 29'891   | 29'780     | 29'33             | 29'650       | 29'664 |      |

|                | 71'67    | 52'22      | 64'4              | 60'35        | 55'95  |      |

|                | 4'31     | 3'35       | 1'57              |             |       |      |

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.
XXXVI. On the Electro-chemical Department of Oxygen.

By M. Viard.*

[With a Plate.]

IN 1801, a year only after the discovery of the battery, Pepys demonstrated that when a battery was placed in air, an absorption of oxygen took place, and that the current showed a tendency to cease when nitrogen alone remained.

Since then, Biot and Cuvier, De la Rive, Joule, and Adie have successively demonstrated this same influence of oxygen.

This class of experiments may be considered as having formed one of the principal bases of the electro-chemical theory; and for a long time oxygen by its presence was supposed to act on the positive plate, in that it there produced oxidation.

Nevertheless, Grove having shown that in a gas battery the platinum plate placed in the hydrogen could be replaced by a plate of zinc without thereby causing the absorption of oxygen on the other platinum plate to cease, it was necessary at once to admit that oxygen could play a different part to the one hitherto assigned it. In the Edinburgh New Philosophical Magazine, January and October 1848, and in the third volume of the Philosophical Magazine, (S. 4) Adie corroborated the same facts in a still more forcible manner, and endeavoured to show, that, contrary to the received opinion, oxygen by its presence acted only upon the negative plate. Amongst other experiments hereafter to be mentioned, he cites the remarkable one, that when a plate of zinc, entirely immersed in an electrolyte, is opposed to a plate of zinc in partial contact with oxygen, the latter, and not the former, is rendered negative.

* From the Annales de Chimie et de Physique, vol. xxxvi. p. 129.
These are the principal experiments known to me, which tend clearly to show the influence of oxygen in batteries.

Independently of the facts just cited, and of some others to be afterwards mentioned, many other phenomena, involving certainly the presence of oxygen, may be found in the memoirs of Porret, Marianini, CErsted, Sturgeon, De la Rive, Becquerel, Faraday, Vorsselman de Heer, Poggendorff, Ed. Becquerel, Schröder, Adie, Beetz, &c. I have made many experiments upon the effects produced by agitating the plates immersed in the liquids, by elevating the temperature of one of the plates, by the ebullition of the liquids, and by exposing the one, or the other, or both plates to the action of the atmosphere. Almost all these phenomena vary with the nature of the liquids and the plates, and consequently they did not appear to me at all capable of illustrating the electro-chemical deportment of oxygen.

Above all, if we reflect on the varied manners in which these phenomena present themselves, the insufficiency of the experiments, on which all explanations are sought to be based, will be evident; and then it will be understood why it has here been deemed necessary so often to repeat, upon the metals and different electrolytes, the fundamental experiments which show the mode of action of the gas upon the electrolytes.

As liquids, I have used water, the sulphates of potash, soda, magnesia, zinc and copper, chloride of sodium, sulphuric acid, and hydrochloric acid.

As metals,—zinc, iron, copper, silver, platinum, and the amalgam of potassium.

And as in all such experiments as are cited in the present memoir analogous results were obtained without encountering a single exception, I believe the suggested law to be a general one.

§ 1.—On the Currents which are produced when two metallic plates of the same nature are immersed in the same electrolyte, in presence of different quantities of oxygen.

The currents first examined were such as are produced when two plates of the same nature are immersed in two separate portions of the same electrolyte, which differ from each other only in the quantity of oxygen therein dissolved.

The apparatus used (Plate IV. fig. 1) consists of two tubes, each open at the top, but closed at the bottom by a piece of bladder, both being sustained in the interior of a glass vessel by means of the same cork. The vessel is filled with the electrolyte to be operated upon, the two tubes are also filled with the same liquid, but the portion poured into one tube differs from the portion poured into the other by the quantity of oxygen or atmos-
spheric air which it holds in solution. Two plates of the same nature, connected with a galvanometer, are plunged into the two tubes, and then the deviation of the needle observed.

Unfortunately, by employing the same electrolyte, deviations in the same direction are not always obtained with plates of the same nature. The experiments appeared generally to indicate that the presence of oxygen upon a plate renders it negative; but contradictory experiments, although few in number, throw some doubt upon the conclusions one is here tempted to draw.

Let us therefore examine the causes capable of producing such a variation in the results.

However carefully the plates may be prepared, whether by cleansing them chemically, or by exposing a fresh metallic surface by means of friction with glass paper, emery paper, silica, or lamp-black, all which methods were successively tested, it appeared to me impossible to obtain plates which, when plunged simultaneously into the same liquid, would produce no movement of the needle in the galvanometer. By using pure acids, and by purifying the materials from which the plates are mechanically prepared, this desideratum may be sometimes approached; but there are many cases where the plates are strongly heterogeneous.

Poggendorff, Lenz, Saweljew, Schröder, Du Bois Reymond, and many others have encountered a like difficulty.

This heterogeneity, almost always present, was evidently one of the most disturbing causes in my first experiments. They were eliminated from all such, however, by uniting to the first apparatus a second one, of as similar a construction as possible, and containing in its two tubes the same electrolyte, but at the same degree of oxidation; for by this means it is evident that the heterogeneity of the plates could at any time be measured (fig. 2).

Let A represent the apparatus whose two tubes contain the same liquid at the same degree of oxidation, for example, boiled water; and B the apparatus whose two tubes contain the same liquid at two different degrees of oxidation, for instance, boiled water and aerated water.

If now, by plunging two plates of copper into the apparatus A, the needle of the galvanometer deviate 10° to the right, thereby announcing that the plate to the right is negative with respect to the plate to the left; if, by taking the two plates from the apparatus A, and plunging them into the apparatus B, which differs from the former in no other respect than that one tube now contains aerated water instead of boiled, a deviation of 56° again to the right be obtained; if, by alternately repeating these two experiments, the deviation always decrease when passing from the apparatus B to the apparatus A, and always increase
when the contrary is the case, we may safely conclude that, independently of the primitive heterogeneity of the plates, and the alterations which the liquids have in the mean time sustained, the presence of oxygen upon a plate renders it negative. This result will be still more evident if, by turning the apparatus B so that the aërated water be to the left and the boiled water to the right, a deviation of, for example, 40° to the left be observed; the deviation with the apparatus A and boiled water being, as before, from 10° to 12° to the right.

In the case of one of the most imperfect preparations, a deviation of 50° to the right was obtained with the apparatus A; with the apparatus B, when the aërated water was to the right, a deviation of 80° in the same direction; and with the apparatus B, when the aërated water was to the left, a deviation of 10° still to the right; the right plate being thus always negative, whatever the position of the aërated water with respect to the boiled. At first sight, or in an isolated series of experiments, this might appear to throw some incertitude on the results of the first and second experiments, for here we find a plate of copper in aërated water positive with respect to another plate of the same nature in boiled water; the apparent contradiction is, however, readily explained by the heterogeneity of the plates; and in fact, by comparing the deviations, it will at once be seen that these experiments confirm the general principle, of which I have here undertaken the demonstration; that is to say, that when any two plates are simultaneously immersed in the same electrolyte, an increase of the quantity of oxygen on the side of one of them tends to render that plate more negative or less positive.

The above, however, is not the only disturbing cause to be guarded against; for when two metallic plates have been plunged into either apparatus, the part of the plate which has not been wet, and the part situated at the junction between air and liquid, are not in the same condition as the parts which have been immersed in the liquids.

Experiment proves that in passing from one operation to another, very different deviations may be obtained, according as these parts are above or beneath the surface of the liquid; for we are never certain of operating always under the same circumstances, even when employing tubes of exactly the same length.

This cause of error, encountered also by Du Bois Reymond, disappears in a great measure by covering a certain length of the wires or the plates with a non-conducting substance. I have generally employed glue or gum-lac; and experiment having proved that a copper wire entirely covered with glue or gum-lac, and a zinc wire, when plunged together into acidulated water, caused no deviation of the needles of one of my most sensitive.
galvanometers, I think that the presence of these substances, on a certain length of the wires or stems of the plates employed, could scarcely have given rise to new causes of current.

At first, simple glass tubes, 12 millimetres in diameter and 1 decimetre long, were operated with, but afterwards some experiments were repeated with two series of four new porous cells, such as are used in Bunsen's battery; before using the latter they were boiled for a long time in pure water.

As electrodes, wires, plates of 6 square centimetres, and lastly, plates of from 30 to 40 square centimetres were used.

In the course of the investigations, where it was necessary to observe currents of very different intensities, various galvanometers were employed*. Their sensitiveness was made to vary very much; sometimes by means of a simple magnet, according to Melloni's method, and sometimes by means of a small magnetic compass with which Ruhmkorff provides his galvanometers. Occasionally the needles were rendered sufficiently astatic as to require 120 seconds to make a complete oscillation.

When the plates are withdrawn from one apparatus in order to plunge them into the other, it is necessary to operate quickly, and to avoid as much as possible all agitations which could modify the phænomena under consideration. With a little practice, and by always employing very fine and well-annealed wires to connect the electrodes with the galvanometer, this source of error may to a great extent be avoided; and the danger will be further diminished, if, instead of the initial deviations, those which exist two or three minutes after immersion be noted.

In certain cases, in order to compare the results, it is necessary to employ great regularity in experimenting, to observe equal intervals of time, dating from each immersion. In the present investigations a new experiment was generally made every four minutes, and the deviations noted at the end of the twelfth, thirteenth, and fourteenth. The time was indicated by a clock beating seconds.

Lastly, it is advisable to introduce the same liquids into each apparatus on commencing each series of experiments, so as to ensure like deviations under like circumstances.

In order to obtain solutions exactly at the same point of saturation, and at different states of oxidation, the liquid was first boiled, then a portion enclosed in a vessel with a ground stopper, and the rest exposed to the action of the air for one or two days.

In the tables showing the results of my experiments, the letter b indicates that the tubes of the apparatus contained boiled liquid, the letter a that they contained aërated liquid, ba that

* One of the most sensitive had 1800 coils of wire, the other 3000.
the boiled liquid was to the left and the aerated to the right, and \( ab \) the contrary disposition.

The sign + expresses that the plate to the right is negative, and the sign — that it is positive.

<table>
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<td>( ba )</td>
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<td>+24.0</td>
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<tr>
<td>b</td>
</tr>
<tr>
<td>+0.2</td>
</tr>
<tr>
<td>0</td>
</tr>
<tr>
<td>0</td>
</tr>
<tr>
<td>( ba )</td>
</tr>
<tr>
<td>+24.5</td>
</tr>
<tr>
<td>+23.0</td>
</tr>
<tr>
<td>+23.0</td>
</tr>
<tr>
<td>b</td>
</tr>
<tr>
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</tr>
<tr>
<td>+0.2</td>
</tr>
<tr>
<td>+0.2</td>
</tr>
<tr>
<td>( ba )</td>
</tr>
<tr>
<td>+24.0</td>
</tr>
<tr>
<td>+23.0</td>
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<td>b</td>
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<tr>
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<tr>
<td>+0.8</td>
</tr>
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<td>+0.5</td>
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<th>Solution of common salt.</th>
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<tr>
<td>( ba )</td>
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<tr>
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<td>+30.5</td>
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<td>a</td>
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<td>+1.5</td>
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<tr>
<td>( ab )</td>
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<tr>
<td>-30.0</td>
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<tr>
<td>-30.0</td>
</tr>
<tr>
<td>-30.0</td>
</tr>
</tbody>
</table>
M. Viard on the Electro-chemical Deportment of Oxygen. 247

Many other series were obtained, but as they presented exactly the same form as the above, the latter may suffice; the following table merely indicates the several experiments made:

<table>
<thead>
<tr>
<th>Metals employed.</th>
<th>Electrolytes.</th>
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<tr>
<td>Zinc</td>
<td>OH SO₄K SO₄N SO₄Mg SO₄Zn SO₄H Cl Na Cl Ba</td>
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<tr>
<td>Iron</td>
<td>OH SO₄K SO₄Mg ClE Cl Ba</td>
</tr>
<tr>
<td>Copper</td>
<td>OH SO₄K SO₄N SO₄Mg SO₄Zn SO₄H SO₄Cu ClNa ClBa</td>
</tr>
<tr>
<td>Silver</td>
<td>OH SO₄K SO₄H SO₄Cu Cl Na Cl Ba</td>
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</table>

The experiments with zinc were made either with distilled zinc or with amalgamated zinc.

For some time the experiments with iron failed; for when ordinary iron wire, cleaned with emery paper, was used, the needle often made sudden leaps, thus rendering the observations extremely irregular. It was only by employing perfectly polished steel knitting needles that regular results could be obtained.

Sometimes copper wires were used, but oftener, plates of copper beaten flat with the hammer.

The silver plates had been reduced from chloride of silver by means of potash, and may be regarded as very pure. The copper as well as the silver plates were well cleaned with emery paper.

The above tables show clearly the deportment of oxygen; for example, from the results obtained with zinc and water, we see that when the wires were plunged into boiled water, the deviations were almost null; that when the boiled water on the right was replaced by aërated water, the deviation announced that the right wire had become negative; and that when the aërated water was to the left, the direction of the deviation was changed.

When the two wires were plunged into aërated water, the deviation was again almost null; but when the aërated water on the right or on the left was replaced by boiled water, the right or left plate became positive.

The other tables but extend the same results to other metals and other electrolytes.

Instead of operating as above explained, another method was often employed, which, though perhaps less sure, yet when employed with care, leaves no doubt as to the influence of oxygen.

Two plates, as identical as possible, were covered with test-tubes (fig. 3); these test-tubes and the vessel which served as their recipient were filled with the same electrolyte, and subsequently oxygen was passed into the one tube and the current observed. The results of these experiments confirmed those already given, but, I must repeat, they require great care in
their execution. It is necessary, in order to avoid oxidation at
the exterior surface of the electrolyte, to cover the exterior parts
of the stems to the plates in both glass tubes with gum-lac; first,
to fill the apparatus with the liquid deprived of air by ebullition,
in order to be assured of the equality of the plates before admit-
ting oxygen; and lastly, in all observations to avoid the effects
of agitation as much as possible.

In order that my researches might include all the metals, I
wished to examine the influence of oxygen on the one hand upon
potassium, and on the other upon platinum.

As potassium possesses the property of decomposing water, it
was of course impossible to employ it in its pure state: the
amalgam of potassium alone could be operated upon.

To the bottom of two broad glass tubes, and where the glass
was thin, two platinum wires were soldered, the upper parts of
which were curved back in a circle. An amalgam of potassium
was formed with distilled mercury and a very small quantity of
potassium; one-half of the amalgam was then poured into each
tube. The two tubes were first filled with boiling water, the
two wires were joined to the galvanometer, and the two liquids
were united by means of a kind of siphon, full of water, but
closed at its two extremities by membranes. At first only a
feeble current was observable; but when the one tube contained
boiled water and the other aerated, it could again be recognized
that the presence of oxygen rendered the amalgam negative.
The experiment also succeeded when the water was replaced by
marine salt.

Platinum led to the same conclusion; but the first experi-
ments made with the same apparatus which had served for zinc,
iron, and copper, having caused some incertitude, much larger
plates of platinum were operated with, and, at the same time, all
contact between the platinum plates and the membranes was
avoided. The surface of these plates contained 20 square centi-
metres, and they were connected with the galvanometer by means
of extremely fine platinum wires. The plates and the wires were
submitted to the action of heated nitric acid, of concentrated
sulphuric acid, and of dilute sulphuric acid, and lastly, they were
washed several times in water. Two pairs of bottles were used
large enough completely to enclose the plates. In the first pair,
both glasses contained the same electrolyte at the same degree
of oxidation, boiled or aerated; in the second pair, the one ves-
sel contained boiled liquid, the other aerated. When the two
plates were plunged into the first, and a communication was
established between the two liquids by means of a large siphon
terminated by membranes, or rather by means of a small basin,
bearing two vertical tubes closed by membranes (fig. 4), the
needle rested almost stationary. Passing from the first to the second pair caused a strong deviation of the needle; and this initial deviation, although it grew rapidly more feeble, indicated again that the presence of oxygen rendered the plate negative. These experiments succeeded easily with marine salt and with sulphuric acid.

It is probable that by operating with platinized platinum, more powerful currents, and always in the same direction, might be obtained; for with sulphuric acid and two plates of platinized platinum, one of which was placed in oxygen, Beetz could not only observe a current, but could even deduce therefrom the electromotor force*.

All the currents of which we have hitherto spoken may be very well explained according to the ideas of Faraday. If, for example, we immerse in water, on the one hand, a naked copper plate, on the other a copper plate covered over with oxygen, and connect both by a conducting wire; then, under the united influence of the first and the oxygen which adheres to the second, certain particles of the electrolyte will be polarized (fig. 5), that is to say, will take up a certain direction, and present at their extremities contrary electricities; afterwards the several polarized molecules will react upon themselves and upon the other molecules of the circuit, and produce a general state of polarization which will continue to increase. This is exactly the same as would occur in a complete circuit formed by two small magnets $a$ and $a'$, and two pieces of soft iron $b$ and $b'$ (fig. 6). At length, when the polarization has attained a certain degree of completeness, electricity passes from each molecule to the next following throughout the circuit, and at the same time the copper combines with the oxygen of the first molecule of water, the hydrogen of the first molecule with the oxygen of the second, and so on, until finally, the hydrogen of the last molecule of water combines with a free molecule of oxygen. We shall here insist upon the hypotheses, that polarization is anterior to the current and to all chemical decomposition; that it is the result of chemical affinity, and subsequently of the mutual reaction of the polarized particles; that certain effects of tension are produced before all decomposition and before all chemical combination.

I am consequently of opinion that oxygen acts in the battery by its affinity for the hydrogen of water; that alone, it would produce no action, but that the separation of the elements of water is effected under the simultaneous influence of the free oxygen upon the hydrogen of the water, and of the metal upon the oxygen of the water.

* Poggendorff's Annalen, vol. lxx.
I believe we should form a false notion of the phenomena which now occupy our attention, if, considering entirely the deportment of the oxygen adhering to the aërated plate, in relation to the other plate, we neglected to examine the deportment of this oxygen in relation to the plate to which it adheres.

We must take well into consideration that the oxygen does not form a continuous stratum upon the aërated plate; consequently, every point of the surface of this plate, covered with oxygen, tends to form a current, not only with the second plate, but also with all points of the first plate where the copper is exposed. At one and the same time, therefore, there are currents traversing the liquid itself (these are indicated by the galvanometer), as well as currents traversing the liquid strata in contact with the aërated plate. Both kinds of currents have the same electromotor force, but the latter have incontestably the advantage of a much feebler resistance to overcome in the circuit which they traverse; their intensity should therefore exceed that of the former. The oxidation of the non-aërated plate is not the sole effect produced, which, by absorption of oxygen, produces a current; it is accompanied by a still more powerful oxidation of the aërated plate by means of its adhering oxygen and a series of superficial currents. It is not here asserted that all the oxidation is thus produced, for such an assertion would be in advance of facts; but it will scarcely be disputed, that at least a part of the oxidation is produced by means of the electric current.

It might be thought, according to the ideas of Faraday himself, that the action of oxygen in all the cases just cited is but a secondary one; that the oxygen acts upon the metal, forming oxide, and that a current is thus produced between the molecules of the metal and those of the oxide at the other extremity of the current, accompanied with reduction of this oxide.

This appears to me less probable; for on the one hand, with metals less oxidable than hydrogen (such as copper and silver), I do not understand how the united actions of metal and oxide could determine the decomposition of water; and on the other hand, with metals such as iron and zinc, more oxidable than hydrogen, I do not see how the oxide could be reduced. It may be further remarked, that if the presence of a molecule of metal and a molecule of oxide in water, united by a metallic circuit, had the property of producing a current accompanied with reduction of the oxide, then the molecule of metal would be transformed into oxide, and the molecule of oxide into metal, so that a series of alternating currents should be the result.

According to the present hypothesis, it is only in the case where the oxide formed in presence of the oxygen is superior
to that formed electro-chemically, that the oxide could intervene by its reduction, which even then would be only partial.

But, although denying this mode of action of the oxide, I am prepared to admit a certain action. I am aware that, according to the experiments of Davy, Becquerel and Marianini, the oxide is negative with respect to the metal; but I maintain still, that the oxide acts only as a less oxidable body. A battery of copper and oxide of copper, of zinc and oxide of zinc, is more or less analogous to a battery of copper and platinum, of zinc and platinum, which we shall presently examine.

With copper alone, free oxygen is essential to the current; to replace the metal by the oxide would be to accelerate the effect.

With zinc and iron, in absence of free oxygen, a current would be produced accompanied by a disengagement of hydrogen upon the oxide without causing its decomposition. The presence of free oxygen would determine a new kind of current, where the affinity of free oxygen for hydrogen would join itself to that of zinc for oxygen.

When the water, instead of being pure, contains sulphate of potash, sulphate of zinc, sulphuric acid, chlorides or hydrochloric acid in solution, the actions are analogous. An observable current corresponds to the formation of sulphate and oxide, or of chloride and oxide upon the non-aërated plate; and of potash, soda, oxide of zinc, or water upon the aërated plate (fig. 5).

At the same time, the aërated plate is the seat of superficial currents of the same nature.

All bodies thus formed have a tendency to react upon each other; nevertheless it is still possible to examine the formation, as will be seen in another memoir.

§ II.—On the influence exercised by oxygen in batteries composed of two metals.

In examining this subject, we shall first demonstrate, that in a simple element the intensity of the current increases with the quantity of oxygen dissolved.

Experiments were made with aërated liquids, and with liquids deprived of air by ebullition. The arrangement of the experiments was the same as before, with the exception, that, instead of bladders being attached to the tubes, the plates rested immediately on the bottom of the vessels. One apparatus contained boiled water, the other aërated water.

Only a few of the series of results which were obtained will be here cited; they are those which correspond to marine salt.
M. Viard on the Electro-chemical Department of Oxygen.

<table>
<thead>
<tr>
<th>Zinc and iron</th>
<th>Iron and copper</th>
<th>Copper and silver</th>
<th>Silver and platinum</th>
</tr>
</thead>
<tbody>
<tr>
<td>a</td>
<td>a</td>
<td>b</td>
<td>a</td>
</tr>
<tr>
<td>40.9</td>
<td>28.0</td>
<td>30.0</td>
<td>39.0</td>
</tr>
<tr>
<td>40.4</td>
<td>27.0</td>
<td>28.5</td>
<td>37.0</td>
</tr>
<tr>
<td>40.0</td>
<td>26.5</td>
<td>28.0</td>
<td>36.1</td>
</tr>
<tr>
<td>b</td>
<td>b</td>
<td>a</td>
<td>b</td>
</tr>
<tr>
<td>32.7</td>
<td>19.9</td>
<td>45.0</td>
<td>10.2</td>
</tr>
<tr>
<td>32.3</td>
<td>19.3</td>
<td>44.5</td>
<td>10.0</td>
</tr>
<tr>
<td>31.9</td>
<td>18.7</td>
<td>44.0</td>
<td>9.8</td>
</tr>
<tr>
<td>a</td>
<td>a</td>
<td>b</td>
<td>a</td>
</tr>
<tr>
<td>40.5</td>
<td>27.5</td>
<td>30.2</td>
<td>39.3</td>
</tr>
<tr>
<td>40.1</td>
<td>26.8</td>
<td>29.0</td>
<td>37.4</td>
</tr>
<tr>
<td>39.8</td>
<td>26.2</td>
<td>28.0</td>
<td>36.5</td>
</tr>
<tr>
<td>b</td>
<td>b</td>
<td>a</td>
<td>b</td>
</tr>
<tr>
<td>32.5</td>
<td>22.5</td>
<td>44.5</td>
<td>10.0</td>
</tr>
<tr>
<td>32.5</td>
<td>22.1</td>
<td>44.0</td>
<td>10.0</td>
</tr>
<tr>
<td>31.6</td>
<td>21.4</td>
<td>43.2</td>
<td>9.9</td>
</tr>
</tbody>
</table>

This table evidently shows that, for the four elements therein contained, the intensity of the current increases with the quantity of oxygen dissolved.

The plates and liquids were varied in many ways, as may be seen from the following table; the results, however, were always similar. No exception occurred, except with a battery of zinc and iron in presence of dilute sulphuric acid, which, on account of the abundant disengagement of hydrogen which then always takes place, might have been easily foreseen. We shall afterwards see how it is possible, even in this case, to verify the influence of oxygen.

General Table of Experiments.

<table>
<thead>
<tr>
<th>Metals.</th>
<th>Electrolytes.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Zinc and platinum</td>
<td>HO SO₄K</td>
</tr>
<tr>
<td>Zinc and silver</td>
<td>HO SO₄K SO₄Mg SO₄Zn</td>
</tr>
<tr>
<td>Zinc and copper</td>
<td>HO SO₄K SO₄Mg</td>
</tr>
<tr>
<td>Zinc and iron</td>
<td>HO SO₄K SO₄Mg</td>
</tr>
<tr>
<td>Iron and platinum</td>
<td>HO SO₄K</td>
</tr>
<tr>
<td>Iron and silver</td>
<td>HO SO₄K SO₄Mg</td>
</tr>
<tr>
<td>Iron and copper</td>
<td>HO SO₄K SO₄Mg</td>
</tr>
<tr>
<td>Copper and platinum</td>
<td>HO SO₄Zn SO₄H SO₄Cu</td>
</tr>
<tr>
<td>Copper and silver</td>
<td>HO SO₄K SO₄Mg SO₄Zn SO₄H SO₄Cu</td>
</tr>
<tr>
<td>Silver and platinum</td>
<td>HO SO₄K SO₄Mg SO₄Zn SO₄H</td>
</tr>
</tbody>
</table>

It was found impossible entirely to expel atmospheric air from the liquids used in these experiments; consequently, other experiments were made, in order to ascertain to what degree the current could be enfeebled by expelling as much oxygen as possible.

The plates of the elements were introduced into glass bells
closed by a stratum of mercury (fig. 7). The plates were terminated by tongues of metal, which traversed glass tubes partially filled with gum-lac, or rather with mercury, when the latter did not attack the plates. The electrolytes employed had been deprived of air by ebullition. When the elements were thus arranged, the circuit was closed, and allowed to remain so until, by the action of the plates, the intensity of the current was made very feeble. The glass bells were then replaced by glass tubes of the same diameter, but open at their upper extremities, and the electrolytes were replaced by aërated liquids of the same nature. Plates of zinc, iron, and copper were successively employed, and opposed to platinum in water, sulphate of potash, and marine salt; in all cases it was found that the presence of oxygen increased the intensity of the current considerably.

But the difference is still more visible when, according to Adie's method, the electrolytes are hermetically enclosed in glass tubes, bearing at their extremities wires of iron or copper, of silver or of platinum, affixed by means of the blowpipe. The apparatus, figs. 8 and 9, is filled by means of a lateral aperture, which can be subsequently closed before the lamp. By allowing the circuit to remain closed for fifteen days or a month, the intensity of the current may be rendered very feeble; it increases considerably, however, on the admission of oxygen. The latter may be effected by breaking the lateral tube, and allowing the apparatus thus to remain for some hours. Adie thus obtained a deviation of from $\frac{1}{2}$ to $3\frac{1}{2}$ degrees in the needle of his galvanometer. I have not obtained so great a deviation, and have found it preferable completely to replace the electrolyte by a liquid freshly aërated, which could be effected by means of two lateral apertures affixed to the tube, for then the whole instrument could be used as a siphon (fig. 9). Experiments have been made with iron and platinum, in presence of water, the sulphates of potash and magnesia, and the chlorides of sodium and barium, as well as with iron and silver, and copper and platinum, in presence of water, sulphate of potash and marine salt; and in fifteen experiments, permanent deviations at least a hundred times greater than those with the primitive electrolytes, were always observed after the introduction of the aërated liquids.

In place of employing the galvanometer to verify the influence of oxygen, chemical decompositions may be effected by operating upon iodide of potassium and starch, or upon dilute sulphuric acid. The inconvenience most often encountered here is, that a certain number of elements are necessary in most cases. Whatever may be the method employed, the effect of oxygen may be always increased by allowing a part of the plates to remain exposed to the air, instead of immersing them entirely in the oxidized liquid.
All these experiments, therefore, confirm and generalize the results before obtained with a zinc battery, by Adie, Gassiot, Joule, De la Rive, and still earlier by Cuvier and Biot, under very different circumstances. 

Now that we have verified the influence of oxygen in the batteries, we have to examine its special influence upon each of the plates.

This question is easily solved experimentally. It suffices in the apparatus with tubes, before used (fig. 1), to close the tubes by bladders; in one apparatus to fill the two with the same electrolyte at the same degree of oxidation; in the other apparatus to fill them with the same electrolyte, but at different states of oxidation; and lastly, to observe the currents which are produced by changing the plates from one apparatus to the other, and thus successively, on each plate, replacing the aerated liquid by the boiled liquid, and reciprocally.

Some of the results obtained are shown by the following table, where, for greater simplicity, the least oxidable plate is supposed always to be on the right, so that ba indicates that the aerated liquid is on the side of the least oxidable plate, and ab the contrary disposition.

<table>
<thead>
<tr>
<th>Zinc and iron.</th>
<th>Iron and copper.</th>
<th>Copper and silver.</th>
<th>Silver and platinum.</th>
</tr>
</thead>
<tbody>
<tr>
<td>HO.</td>
<td>Cl Na.</td>
<td>HO.</td>
<td>Cl Na.</td>
</tr>
<tr>
<td>b R</td>
<td>a R</td>
<td>b R</td>
<td>ba R</td>
</tr>
<tr>
<td>46'7</td>
<td>55'0</td>
<td>19'8</td>
<td>13'7</td>
</tr>
<tr>
<td>44'7</td>
<td>54'0</td>
<td>18'8</td>
<td>13'5</td>
</tr>
<tr>
<td>43'6</td>
<td>53'5</td>
<td>18'0</td>
<td>12'3</td>
</tr>
<tr>
<td>ab R</td>
<td>ba R</td>
<td>ab R</td>
<td>b R</td>
</tr>
<tr>
<td>46'0</td>
<td>56'0</td>
<td>18'0</td>
<td>3'5</td>
</tr>
<tr>
<td>44'0</td>
<td>55'1</td>
<td>16'3</td>
<td>3'0</td>
</tr>
<tr>
<td>42'8</td>
<td>54'3</td>
<td>16'0</td>
<td>2'7</td>
</tr>
<tr>
<td>b</td>
<td>a</td>
<td>b</td>
<td>ba</td>
</tr>
<tr>
<td>46'7</td>
<td>55'0</td>
<td>20'0</td>
<td>13'7</td>
</tr>
<tr>
<td>44'8</td>
<td>53'8</td>
<td>19'0</td>
<td>13'5</td>
</tr>
<tr>
<td>43'4</td>
<td>53'2</td>
<td>18'0</td>
<td>12'3</td>
</tr>
<tr>
<td>ab</td>
<td>ba</td>
<td>ba</td>
<td>b</td>
</tr>
<tr>
<td>45'8</td>
<td>56'0</td>
<td>37'5</td>
<td>3'7</td>
</tr>
<tr>
<td>43'3</td>
<td>55'0</td>
<td>37'0</td>
<td>3'5</td>
</tr>
<tr>
<td>42'4</td>
<td>54'1</td>
<td>36'3</td>
<td>3'2</td>
</tr>
<tr>
<td>b</td>
<td>a</td>
<td>b</td>
<td>ab R</td>
</tr>
<tr>
<td>46'4</td>
<td>54'8</td>
<td>20'3</td>
<td>0'7</td>
</tr>
<tr>
<td>44'6</td>
<td>53'7</td>
<td>19'0</td>
<td>0'5</td>
</tr>
<tr>
<td>43'5</td>
<td>52'9</td>
<td>18'0</td>
<td>0'2</td>
</tr>
<tr>
<td>ba R</td>
<td>ba</td>
<td>ba</td>
<td>b R</td>
</tr>
<tr>
<td>54'5</td>
<td>56'0</td>
<td>37'0</td>
<td>3'7</td>
</tr>
<tr>
<td>53'0</td>
<td>55'0</td>
<td>36'7</td>
<td>3'5</td>
</tr>
<tr>
<td>52'8</td>
<td>53'8</td>
<td>36'7</td>
<td>3'2</td>
</tr>
<tr>
<td>b R</td>
<td>a</td>
<td>b R</td>
<td>ab</td>
</tr>
<tr>
<td>46'3</td>
<td>54'6</td>
<td>23'0</td>
<td>0'5</td>
</tr>
<tr>
<td>45'0</td>
<td>53'7</td>
<td>22'1</td>
<td>0'3</td>
</tr>
<tr>
<td>43'8</td>
<td>52'7</td>
<td>21'6</td>
<td>0'2</td>
</tr>
</tbody>
</table>

Some of the results obtained are shown by the following table, where, for greater simplicity, the least oxidable plate is supposed always to be on the right, so that ba indicates that the aerated liquid is on the side of the least oxidable plate, and ab the contrary disposition.
The results contained in the foregoing table clearly indicate that the presence of oxygen upon the negative plate augments the current, and that its presence upon the positive plate diminishes the same.

For example, let us consider the currents produced by copper and silver in water. The first column shows us that in boiled water the deviation was about 3°5, that it was increased to 13° when the oxygen was on the side of the silver plate, and that it was reduced to 0°5 when the oxygen was on the side of the copper plate.

The second column shows that the deviation, which in aërated water amounted to 10°, decreased to 0°5 when the silver plate was immersed in boiled water, but that it increased 1° when the copper plate was withdrawn from the influence of oxygen.

All other results in the same table may be interpreted in a similar manner.

The following table shows the whole series of experiments which were made by varying the plates and the electrolytes. The results accorded perfectly with the above:

<table>
<thead>
<tr>
<th>Metals</th>
<th>Electrolytes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Zinc and iron</td>
<td>HO Cl Na</td>
</tr>
<tr>
<td>Zinc and copper</td>
<td>HO Cl Na</td>
</tr>
<tr>
<td>Zinc and silver</td>
<td>HO SO₄ K SO₄ Zn Cl Na</td>
</tr>
<tr>
<td>Zinc and platinum</td>
<td>HO</td>
</tr>
<tr>
<td>Iron and copper</td>
<td>HO SO₄ K Cl Na</td>
</tr>
<tr>
<td>Iron and silver</td>
<td>HO Cl Na</td>
</tr>
<tr>
<td>Copper and silver</td>
<td>HO SO₄ K SO₄ Mg SO₄ Zn SO₄ H SO₄ Cu Cl Na</td>
</tr>
<tr>
<td>Copper and platinum</td>
<td>Cl Na</td>
</tr>
<tr>
<td>Silver and platinum</td>
<td>SO₄ K SO₄ H Cl Na</td>
</tr>
</tbody>
</table>
Experimentally, it is easy to prove that the presence of oxygen upon the negative plate augments the current; but when it is required to show that its presence upon the positive plate diminishes the same, the operation becomes very delicate. In most of the experiments here made, the intensity of the current could only be reduced from 1 to 2 degrees, and sometimes only the fraction of a degree; and then, in order to distinguish the direction of the current, it was often necessary, on account of the slow modifications of the plates, to compare the deviations of each experiment with the mean of the deviations in the foregoing and in the following experiments.

As the chemical actions at the contact of the metals with the liquids change the composition and aeration of the latter, it is always necessary, after verifying the influence of oxygen upon one plate, to renew these liquids in each apparatus before seeking to verify its influence upon the other. This renewal is indicated in the table by the letter R.

Lastly, we may remark, that, when passing from one series of experiments to another upon different metals or electrolytes, a change of glasses and corks is indispensable; in short, the apparatus must at such times be reconstructed.

The theory of the phenomena just examined may be deduced from the one before given for currents produced by two plates of the same nature when placed in liquids differently aerated.

Let us consider a plate of copper and a plate of silver in non-aerated water. Under the influence of the copper, the liquid will polarize itself, but there will be no decomposition and no current; but if the silver be covered with oxygen, this oxygen will assist the copper in the polarization of the liquid; and under the united influence of copper and oxygen, a current will be produced accompanied with a formation of oxide of copper and water. The introduction of oxygen upon the copper would evidently tend to produce a contrary state of polarization to the one which copper alone tends to produce; hence the current would be thereby diminished.

The theory of a zinc and silver battery in water would again be nearly the same; with this difference, however, that without oxygen a current would be produced.

Some have maintained, that in these experiments, or in analogous ones with a gaseous battery, an oxidation of the positive plate at first takes place, and at the same time a deposit of hydrogen upon the negative plate; that free oxygen does not interfere except to depolarize the negative plate.

Others, that the negative plate oxidizes under the influence of the free oxygen present, and that a current is thus produced between the positive plate and the oxide of the negative plate.
Neither of these explanations can be general. Why is it necessary to admit, in batteries where silver and copper are the most oxidable metals, that the effect is due to depolarization, when there are no facts supporting this hypothetical disengagement of hydrogen?

How is it possible to admit, in batteries where silver and platinum form the negative plates, that the oxidation of these metals can produce the current, when this oxidation in itself is so little known as to be altogether disputed by many philosophers? How can we conceive that platinum, the least oxidable of metals, can, by its oxidation and deoxidation, make, at the same time, most oxygen disappear?

There is, however, little doubt that in batteries where a current can be produced without the intervention of oxygen, in batteries of zinc and platinum, for example, a deposit of hydrogen may in some points take place, and that the subsequent presence of some molecules of oxygen may serve to depolarize the plates; experience, too, has long since proved, that if some oxide has been formed upon the negative plate, a current may take place between the positive plate and this oxide, accompanied with reduction of the latter.

But, inasmuch as there are no grounds for denying that gases absolutely possess the property of conducting electricity, of acting as electromotors, without which properties, indeed, they would constitute a class distinct from all other bodies, whereas a crowd of experiments, and particularly those of Faraday, tend, on the contrary, to attribute the same properties in different degrees to all bodies, whatever their state,—I think that when the negative plate is covered with oxygen, we must in general admit that a great part of the current, and often the whole, is produced under the influence of the positive plate and the free oxygen.

We may here remark, too, that in the batteries we have examined, the oxygen of the aërated liquids not only ought to disappear by means of the current between the two plates, but that a certain quantity of it would be absorbed at the surface of the plates, either directly, or by the artificial currents. And when we consider that the oxygen of the negative plate can, at the same time, produce currents with the metallic parts of this plate, and currents with the more oxidable plate; that if the former enjoy a less resistance, the latter exceed in electromotor force, we shall conceive that, according to circumstances, these two kinds of currents would surpass each other alternately, and that in all cases the presence of the most oxidable plate protects the less oxidable one united to it.

It may be perceived, that in the above tables batteries formed by zinc and sulphuric acid are not mentioned. In reality, in these batteries, as far as could be recognized in a number of experiments in other respects very regular, the influence of oxygen apparently disappears. Nevertheless, the fact cannot be regarded as contrary to the theory above exposed; for these batteries possess a considerable electromotor force, and disengage hydrogen in abundance, so that almost all the oxygen, which could act by its presence upon the negative plate, is destroyed or dispelled before the needle of the galvanometer has ceased to oscillate.

Such an inconvenience may be evidently remedied by introducing contrary electromotor forces into the circuit, so as to diminish the circuit's total electromotor force. A convenient voltameter introduced into the circuit effects this.

From experiments made by Grove, it results that an element of zinc, sulphuric acid, and platinized platinum, which alone cannot decompose the acidulated water between two platinum plates, decomposes it when the platinum plate of the element is in contact with oxygen gas.

Analogous results would probably be obtained, by this method, with all kinds of batteries, in which a too great electromotor force has a tendency to hide the action of oxygen.

In conclusion, I may add, that all that has hitherto been said of the elements of a battery, may with equal justice be repeated of any decomposing apparatus placed in the circuit of a battery; that is to say, that the presence of oxygen upon all the negative plates in the circuit has always the effect of increasing the current by augmenting the electromotor force, unless the current be already very energetic in the absence of oxygen.

The following fact is also demonstrated by an experiment of Grove's.

It was before said, that a pile of zinc, sulphuric acid, and platinized platinum, which alone could not decompose the water between two platinum plates, decomposed the same when a part of the negative pole of the element was covered with oxygen. Grove has shown that decomposition also takes place when oxygen is passed up the side of the negative plate of the voltameter: this fact ought evidently to receive a similar explanation.

In a future memoir I propose to examine the chemical phenomena, which, according to theory, should accompany the currents whose existence has been verified in this memoir.
XXXVII. Note on a Question in the Theory of Probabilities.
By A. Cayley*.

The following question was suggested to me, either by some of Prof. Boole’s memoirs on the subject of probabilities, or in conversation with him, I forget which; it seems to me a good instance of the class of questions to which it belongs.

Given the probability $\alpha$ that a cause $A$ will act, and the probability $p$ that $A$ acting the effect will happen; also the probability $\beta$ that a cause $B$ will act, and the probability $q$ that $B$ acting the effect will happen; required the total probability of the effect.

As an instance of the precise case contemplated, take the following: say a day is called windy if there is at least $w$ of wind, and a day is called stormy if there is at least $r$ of rain, and a day is called rainy if there is at least $R$ of wind, or if there is at least $r$ of rain. The day may therefore be stormy because of there being at least $W$ of wind, or because of there being at least $R$ of rain, or on both accounts; but if there is less than $W$ of wind and less than $R$ of rain, the day will not be stormy. Then $\alpha$ is the probability that a day chosen at random will be windy, $p$ the probability that a windy day chosen at random will be stormy, $\beta$ the probability that a day chosen at random will be rainy, $q$ the probability that a rainy day chosen at random will be stormy. The quantities $\lambda, \mu$ introduced in the solution of the question mean in this particular instance, $\lambda$ the probability that a windy day chosen at random will be stormy by reason of the quantity of wind, or in other words, that there will be at least $W$ of wind, $\mu$ the probability that a rainy day chosen at random will be stormy by reason of the quantity of rain, or in other words, that there will be at least $R$ of rain.

The sense of the terms being clearly understood, the problem presents of course no difficulty. Let $\lambda$ be the probability that the cause $A$ acting will act efficaciously; $\mu$ the probability that the cause $B$ acting will act efficaciously; then

$$p = \lambda + (1 - \lambda)\mu\beta$$
$$q = \mu + (1 - \mu)\alpha\lambda,$$

which determine $\lambda, \mu$; and the total probability $\rho$ of the effect is given by

$$\rho = \lambda\alpha + \mu\beta - \lambda\mu\alpha\beta,$$

suppose, for instance, $\alpha = 1$, then

$$p = \lambda + 1 - \lambda\mu\beta, \quad q = \mu + \lambda - \lambda\mu, \quad \rho = \lambda + \mu\beta - \lambda\mu\beta,$$

that is, $\rho = p$, for $p$ is in this case the probability that (acting a cause which is certain to act) the effect will happen, or what is the same thing, $p$ is the probability that the effect will happen.

Machynlleth, August 16, 1853.

* Communicated by the Author.
S 2

The influence of compression and dilatation in producing the doubly refracting structure in solids of all kinds, whether crystallized or uncrystallized, which do not possess it, and in modifying that structure in all crystals which do possess it, has been long known; but with this class of phenomena, those which I am about to describe have no connexion whatever.

In the course of experiments on the double reflexion and polarization of light which I discovered in the chrysammates of potash and magnesia, murexide, and other crystals, I was surprised to find that these substances could be spread out upon glass by hard pressure, like grease or soft wax; and that in the case of chrysammate of potash and other bodies, when the powder could scarcely be distinguished from snuff, I obtained a transparent film, exhibiting the phenomena of double reflexion and polarization from its surface as perfectly as if I had been using a large crystal.

In subsequently repeating these experiments, and examining under polarized light the film thus produced by compression and traction, I was surprised to observe that the streaks and separate lines of the film, as well as the film itself, had regular axes of double refraction, as if they were regularly crystallized portions of the substance under examination. These streaks and capillary lines, which were often of extreme minuteness, did not appear to consist of insulated particles merely dragged into a line; but when the substance possessed the new property in perfection, the lines of polarized light were continuous, and the crystallographic as well as the optical axes of the particles were placed in that line. In other cases, where the experiment was less successful, the insulation of the particles was easily recognised, though the general mass of them was crystallographically arranged.

In making these experiments, the natural crystalline powder, or the particles of the crushed crystal, may be placed either upon a polished glass surface or upon a piece of glass ground on one side. In those cases where the substance is soft, the polished surface is preferable; but when the powder is hard, and considerable pressure necessary, it is better to place it upon the ground surface of a piece of glass, as the particles are detained between its minute elevations, and submit more readily to the

combined force of pressure and traction. When the powder is thus placed, I take a polished and elastic knife, and with its broad point I compress and drag the powder in a given direction till there is the appearance of a polished surface on the compressed substance. In general, I have used both the smooth and the rough glass, and have frequently obtained results with the one which were not given by the other.

If we now place the plate of glass in a polarizing microscope with the field dark, we shall find that the streaks and lines produced by traction have, in certain substances, regular neutral and depolarizing axes, as if they were prismatic crystals of the substance under examination. With the chrysammate of magnesia, a red powder with specks of yellow reflected light, the phenomena are peculiarly splendid; the natural colours of the substance, which vary greatly with the thickness of the streaks and films, being combined with the different tints which they polarize. As the crystals of this substance possess unusual reflexion, this property is displayed in the crystallized streaks produced by traction; and the superficial colours which they reflect vary with the azimuth which the plane of incidence forms with the plane passing through the axis of the prism.

The remarkable property which I have now described I have found, in a greater or a less degree, in the following crystals:

<table>
<thead>
<tr>
<th>Chrysammate of magnesia.</th>
<th>Platina and magnesia, cyanuret of.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydro-chrysammid.</td>
<td>... and barytes, cyanuret of.</td>
</tr>
<tr>
<td>Murexide.</td>
<td>... potassium, cyanuret of.</td>
</tr>
<tr>
<td>Aloe titinate of potash.</td>
<td>... ammonia, chloride of.</td>
</tr>
<tr>
<td>Aloe tinic acid.</td>
<td>Potash, oxymuriate of.</td>
</tr>
<tr>
<td>Oxamide.</td>
<td>... chromate of.</td>
</tr>
<tr>
<td>Palmine.</td>
<td>Urea, nitrate of.</td>
</tr>
<tr>
<td>Amygdaline.</td>
<td>Camphor.</td>
</tr>
<tr>
<td>Tannin, pure.</td>
<td>Cinchonine.</td>
</tr>
<tr>
<td>Quinine, pure.</td>
<td>... sulphate of.</td>
</tr>
<tr>
<td>... acetate of.</td>
<td>Meconic acid.</td>
</tr>
<tr>
<td>... sulphate of.</td>
<td>Brucine, sulphate of.</td>
</tr>
<tr>
<td>... muriate of.</td>
<td>Morphia, acetate of.</td>
</tr>
<tr>
<td>... phosphate of.</td>
<td>Tin, iodide of.</td>
</tr>
<tr>
<td>... citrate of.</td>
<td>Cerium, oxide of.</td>
</tr>
<tr>
<td>Cacao butter.</td>
<td>Parmeline.</td>
</tr>
<tr>
<td>Veratric acid.</td>
<td>Lecanorine.</td>
</tr>
<tr>
<td>Esculine.</td>
<td>Indigo red.</td>
</tr>
<tr>
<td>Theine.</td>
<td>Ammonia, oxalate of.</td>
</tr>
<tr>
<td>Silver, cyanide of.</td>
<td>... sulphate of.</td>
</tr>
<tr>
<td>... acetate of.</td>
<td>Soda, chromate of.</td>
</tr>
<tr>
<td></td>
<td>Lead, iodide of.</td>
</tr>
</tbody>
</table>
Strychnine, sulphate of.   | Mercury, oxymuriate of.
... acetate of.           | Isatine.
Soda, native nitrate of.  | Alizarine.
Berberine.               | Manganese, sesquioxide of.
Mucic acid.              | Lead, protoxide of.
Solanine.                | Tungstic acid.
Asparagine.              | Chromo-oxalate of potash.

In submitting other crystals to the influence of compression and traction, I have found great numbers which do not exhibit the least trace of transparent streaks and lines, the separate particles being merely dragged into lines, and exhibiting only a quaquaversal polarization. On the other hand, there is another class of crystals whose powders or particles are forced into distinct and transparent streaks and lines, in which the individual particles have a quaquaversal polarization and no trace of a prismatic arrangement. As these crystals have a peculiar relation to those in the preceding list, I shall enumerate the most important of them in the following table; that is, those in which the powder has been dragged into transparent and continuous streaks and lines, resembling externally portions of a solid body; for it is only by a comparison of the physical, or perhaps the chemical qualities of the two classes of bodies, that we can expect to explain the new property which is possessed only by one of them.

<table>
<thead>
<tr>
<th>Hydrate of potash, pure.</th>
<th>Soda, acetate of.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Indigotic acid.</td>
<td>Mercury, prussiate of.</td>
</tr>
<tr>
<td>Urea.</td>
<td>... muriate of.</td>
</tr>
<tr>
<td>Citric acid.</td>
<td>... sulphuret of.</td>
</tr>
<tr>
<td>Silver, nitrate of.</td>
<td>Barytes, acetate of.</td>
</tr>
<tr>
<td>Meconine.</td>
<td>Zinc, chromate of.</td>
</tr>
<tr>
<td>Naphthaline.</td>
<td>... sulphate of.</td>
</tr>
<tr>
<td>Soda, nitrate of.</td>
<td>Cobalt, sulphate of.</td>
</tr>
<tr>
<td>Potash and copper, sulphate of.</td>
<td>Magnesia and soda, sulphate of.</td>
</tr>
</tbody>
</table>

As both compression and traction are necessary in producing the transparent streaks and lines in both classes of the substances I have enumerated, it became interesting to ascertain what effect was produced by each of these forces acting separately, and which of them was chiefly influential in developing the doubly refracting arrangement exhibited by the substances that possessed it.

The force of compression was undoubtedly the agent in forcing the separate particles into optical contact, while that of traction drew them into a line, and tended to dilate the film in the direction of that line, and to draw its particles from each other; or overcome their attraction of aggregation in that direction. It
is quite possible, too, that these forces may have exercised some influence in modifying the doubly refracting structure of the substance under examination; but as such a question has no bearing upon our present subject, I have not attempted its solution.

Without expecting any very interesting result, I submitted to examination several of the soft solids which possess double refraction, such as bees' wax, oil of mace, tallow, and almond soap. The last of these substances, though in common use, is a very remarkable one. Owing to its particles not being in optical contact, it has a fine pearly lustre, and may be drawn out into long and slender strings. Upon laying a portion of it on glass, it has a quaquaversus polarizing structure, with a tendency to form circular crystals; but when it is drawn out into strings, and laid upon glass, these strings have neutral and depolarizing axes, like the streaks formed by compression and traction. In the present case it is by traction alone that this crystalline arrangement of the particles is produced.

In oil of mace and tallow a similar effect is produced by compression and traction. With bees' wax the depolarizing lines are still better displayed, and the effect is considerably increased by mixing the bees' wax with a small quantity of rosin.

As the preceding experiments place it beyond a doubt that the optical or crystallographic axes of a number of minute particles are dragged by pressure and traction into the same direction, so as to act upon light like regular crystals, it became interesting to discover the cause of phenomena which certainly could not have been anticipated from any theoretical principle with which we are acquainted. The primary force, and indeed the only apparent one exerted in these experiments, is a mechanical force; but it is not improbable that a secondary force, namely, that of electricity, may be generated by the friction which accompanies the forces of pressure and traction. That such a force is excited with certain crystals will not admit of a doubt; but even if it were developed in every case, this would not prove that electricity was the agent in producing the phenomena under consideration. In subjecting asparagine to compression and traction, I observed, upon placing it in the polarizing microscope, that its particles were moving about under an electrical influence, but in no other case did the same phenomenon present itself to me.

The experiments with soft solids, but especially those made with the almond soap, exclude the supposition that the electricity of friction is the cause of the crystalline arrangement of its particles; though it is not improbable that the sliding of the particles upon one another, as produced by traction, and their mutual separation, as in the case of tearing asunder mica or
paper, may produce enough of electricity to have some share in
giving the same direction to the axes of the particles.

When a portion of almond soap is placed upon glass, the axes
of its particles lie in every direction, and have no tendency to
assume the crystalline arrangement. The forces of aggregation
eemanating from three rectangular axes, are not strong enough
to overcome the inertia, as we may call it, arising from the
natural quaquaversus adhesiveness of the substance, and from the
water interposed between its particles; but when the portion of
soap is drawn out into a thread, these resistances to crystalline
arrangement are diminished; elementary prisms, or crystals
whose length is greater than their breadth, will have a tendency
to place their greatest length in the line of traction; and all
lateral obstruction to the play of its natural polarities being to a
great extent removed, when the substance is drawn into a capil-
larv thread the molecules will have free scope to assume their
natural crystalline arrangement.

The application of these views to the powders and particles of
hard crystals is not so readily apprehended; but when we con-
sider that the pressure brings the molecules of the substance
within the sphere of their polarities, and that the force of trac-
tion reduces the compressed film into separate streaks and lines,
like the threads of the almond soap, we have reason to conclude,
that, even in hard substances, the atoms, when released from
their lateral adhesions and brought into narrow lines, will assume
the crystalline arrangement.

In the course of these experiments, I have observed in some
cases where the crystalline arrangement was very imperfectly
effected, a tendency in the atoms to quit their position, as if
they were in a state of unnatural constraint, like the particles of
silex and manganese in certain kinds of glass which experience
a slow decomposition. If this should prove to be the case, either
partially or generally, which time only can show, it will doubt-
less arise from the non-homologous sides of the elementary
atoms having come into contact; a condition of the crystalline
lines perfectly compatible with the existence of neutral and de-
polarizing axes, and of the colours of polarized light, provided
that the non-homologous sides in contact deviate from their
proper position, either 90° or 180°. If we cut a plate of mica,
for example, into two pieces, and combine them by turning one
of them round 90° or 180°, polarized light transmitted through
them perpendicularly will exhibit the same colours as when they
were in their natural position, and also the same neutral and
depolarizing axes. If the polarized light is transmitted obliquely,
the hemitropism of the combination, as we may call it, will be
instantly discovered by the difference of colour of the two plates.

St. Leonard's College, St. Andrews,
February 25, 1853.
XXXIX. *On the Optical Phenomena and Crystallization of Tourmaline, Titanium, and Quartz, within Mica, Amethyst, and Topaz.* By Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.*

[With a Plate.]

The existence of certain minerals imbedded in others,—the optical phenomena which they exhibit,—their form and mode of distribution, and the mechanical influence which has been exerted during their formation on the mineral that contains them, are among the most curious and instructive facts in physical science.

The dissemination of perfectly-formed crystals of titanium, both in the form of titanite and anatase, in Brazilian crystals of quartz, is a fact so well known that I shall take no further notice of it, but shall proceed to give an account of a series of facts of a much more general and interesting character, which I have had occasion to observe during an extensive examination of minerals, undertaken with a different object.

1. *On the Distribution of Tourmaline in Mica.*

When fluids and condensed gases are imprisoned in the cavities of topaz and other hard minerals, they retain their place till some powerful agent releases them from confinement, or till heat gives them such an expansive force as to burst the mineral. In mica, however, where the laminae of which it is composed are held together by a very feeble cohesive force, the fluids in their cavities, and the extraneous materials which were present at their formation, have experienced no difficulty in quitting their place and spreading themselves between the plates of the mineral.

Tourmaline and quartz, though thus distributed between the laminae of mica subsequent to its crystallization, have yet found a place in it contemporaneously with the crystallization of the mica itself. In this case they are large crystals, equivalent in thickness to many laminae, and may be taken out and subjected to examination. Some of the crystals of tourmaline are so large, indeed, that I have used them with their own natural faces as analysing prisms; and the quartz crystals, which are amorphous and very irregularly formed, occupy a still greater space. In both cases, however, the tourmaline and the quartz when taken out leave large openings in the laminae, and have greatly disturbed the structure of the mica around them.

The crystals of tourmaline thus formed in the mica have almost always the faces of the flattened hexagonal prism parallel

to the laminae of the mica. I have found, however, a few cases in which the flat summit of the hexagonal prism is parallel to the laminae. The crystallizations of quartz have also the axis of the prism, or its hexagonal faces parallel to the laminae.

The other crystals of tourmaline which I have discovered in mica have a very different character; they have been formed subsequently to the crystallization of the mica, and exist only between its laminae. I have not been able to discover any cavities in mica containing fluids or gases, but I have found thousands from which the fluids and gases have escaped,—the one crystallizing into hexagonal plates of tourmaline, and the other separating the laminae, or running between them, and carrying along with it minute portions of crystallizable matter.

The hexagonal crystals thus formed have their faces perpendicular to the axis of double refraction, which is the axis of the prism; and what is peculiarly interesting, the fluid from which they were formed has insinuated itself between several of the laminae, and the different plates of tourmaline which they formed have, of course, the sides of the hexagon coincident. Sometimes these crystals extend to different distances from the centre of the original cavity, and are occasionally formed round it in a circular group. See Plate III. fig. 1.

The centre of the cavity from which these crystals have been projected is occupied by a spherical group of granular or capillary crystals, which is generally very opaque, though such groups sometimes exhibit, in particular spots, double refraction, and a speck of light is occasionally seen through the centre of the group. In some cases I have observed these very thin hexagonal plates without this opaque centre, and they have probably been formed by a portion of the fluid projected to a distance between faces of easy cleavage. The black spherical group already mentioned has its outward surface bristled with points, which are the extremities of the crystals radiating from its centre; and in one fine specimen to be further described, it is surrounded with a ring of less opacity than the nucleus, and analogous to what is common in circular crystals. See fig. 1.

The thin plates thus formed between the laminae, whether hexagonal or prismatic, are always of a faint brownish-yellow, which at an increased thickness becomes green; and so exceedingly thin are these plates, especially those furthest from the nucleus, that with a power of 400 it is often very difficult to see their terminal lines.

In order to convey an idea of these phenomena, I have given a drawing in fig. 1 of a very interesting one, where the prismatic crystal nearest the black central group is a bright green in all azimuths with polarized light, surrounded with three or four
larger prismatic yellowish plates, growing fainter both in tint and outline as they recede. In some cases the crystals are brown, and in others beautifully dichroic, being bright green and pink in the different azimuths of polarized light.

As considerable forces must have been in operation during the production of these phenomena, we may expect to see the effects of them upon the surrounding mica. We accordingly observe the polarization produced by pressure round almost all of these crystalline groups. Rents and other marks of violence are distinctly seen in the mica, and cracks or luminous streaks often occur in the tourmaline plates themselves. I have observed, too, in portions of the mica where I cannot find any cavities or crystals, distinct luminous sectors of polarized light, which could only be produced by a force emanating from their centre. This force may have been that of gas discharged from some neighbouring cavity, and driven by change of temperature to some other part of the mica plate; and in the following remarkable phenomenon we may perhaps find some evidence in favour of this opinion.

Plates of mica contain many beautiful systems of Newton's rings, occupying a circular space where the laminae have been separated by some cause or other, and where of course there must be either air or some gaseous body. The colours of the first order are at the circumference of the circular space where the laminae are in optical contact, and the higher orders of colour extend towards and often to the centre of the space. Now it is a curious fact, that wherever there is a cavity which has projected its fluid and probably gaseous contents, it is situated in the circumference of one of these circular spaces. When two cavities have been near each other, the circular spaces unite and lose their form; and when the cavities have been more numerous, the circular spaces unite into very irregular shapes. That these circular hollows or spaces between the laminae have been produced by something which has issued from the cavity to which they are so constantly related, cannot admit of a doubt. That it has not been a fluid is evident, and therefore it must have been a gas, which is either there still, or has escaped through some minute openings between the laminae, where optical contact has been restored*.

There are some specimens of mica in which the crystals of tourmaline are large and opake, and exhibit phenomena which I believe have not been recognised in any other mineral. The

* A fluid even may have thus escaped, and the circular hollow remained as before. In support of this opinion, see Edinburgh Transactions, vol. x. p. 11, but especially vol. xvi. p. 13; or Phil. Mag. vol. xxxi. p. 101, August 1847.
most interesting specimen of this kind I owe to Professor Fleming, who pointed out to me one of the peculiarities which it contains. This specimen is accurately represented, of the natural size, in fig. 2. The largest of the five crystals is 0·28 of an inch broad, and the smallest 0·08 of an inch. Their thickness cannot greatly exceed the thousandth of an inch, and yet it is with difficulty that the strongest sun-light can be seen through them. The form of the smallest is a perfect hexagon, and in the rest the same form is more or less distinct. In the oval crystal there are numerous holes, and in all of them there are numbers of rectilineal cracks parallel to the sides of the hexagon, and some of them so narrow that light can scarcely pass through them. When we look at the sun through one of these crystals, a curious optical phenomenon is seen; a luminous hexagonal surface, composed of lines of light, parallel to the sides of the hexagon, and six beautiful radiations, like those of the asterial sapphire, perpendicular to the sides of the hexagon.

The existence of these rectilineal fissures is an important fact in crystallography. It proves that the crystals were in a soft state after they had attained their present form; and that, in the process of induration, the fissures were produced by the shrinking of the tourmaline, in the same manner as similar fissures are produced during the induration of clay. In the mica which surrounds some of the crystals there is the appearance of considerable disturbance, but I can find no trace of any cavity from which the tourmaline may have been ejected in a fluid state. The faces of these crystals are not everywhere in optical contact with the mica, and it is very probable that they could be removed without any adhering mica, as I have occasionally found crystals of tourmaline that were moveable between the laminae.

In the same specimen which contains these tourmalines, and in others, I have found, what I believe has never before been observed, the woolly filaments of the Penicillium glaucum of Link, with its sporules scattered about between the laminae, and sometimes beautifully moniliform, as in the Penicillium glaucum obtained from milk by M. Turpin*.

2. On the Distribution of Titanium in Mica.

In examining a remarkable specimen of mica from Irkutsk in Siberia, I found titanium between the laminae in various forms, sometimes in amorphous plates, sometimes in a powdery state adhering to the mica, and most frequently in beautiful dendritic forms of various degrees of thickness. At a thickness of about the hundredth of an inch, the titanium in all these forms

Tourmaline, &c. within Mica and other Minerals.

is opake; but at less thicknesses it has a brownish transparency, becoming almost perfectly transparent at thicknesses which do not seem to exceed the 2000th part of an inch. In fig. 3 I have given a drawing of an opake group executed for me with minute accuracy by my celebrated friend M. Haidinger of Vienna, during his residence in Edinburgh. The transparent groups are much more beautiful than the opake ones, the crystalline ramifications having the most diversified forms, resembling often regular organizations.

When the mica is removed from above the titanium, so that only an exceedingly thin film of it is left, the reflected light is extremely brilliant, and consists of the most splendid colours. These colours, which have always the form of the titanium, are those which are produced by the thin film of mica which covers the titanium, and are not produced, as has been supposed, by a vacuity in the mica.

In some specimens of mica from Bengal, the imbedded titanium is spread out in a very irregular manner from a nucleus, sometimes having the form of a thin film; sometimes of oriental characters; and sometimes it is disseminated in grains so extremely minute, that the flame of a candle seen through it is surrounded with a halo of five or six perfectly-formed coloured rings.

3. Distribution of Quartz in Mica.

In mica from various localities I have found large crystallizations of quartz, the quartz replacing the mica. I have never even once met with a regular crystal of quartz; and what is curious, all the crystalline masses of it which I have examined have their axis of double refraction in the plane of the laminae of mica. In some very large specimens of Bengal mica given to me by Mr. Swinton, I have found layers of quartz, several inches in area, and about the 200th of an inch thick. The two surfaces of the plates are exceedingly unequal and corrugated, owing to the circumstances under which they were formed, but they possessed regular double refraction, and gave the colours of polarized light.

4. Distribution of Titanium in Amethyst.

While examining many years ago, along with the late Marquis of Northampton, several bags of amethyst which had been imported into Scotland from the Brazils, we were surprised to observe a number of fine pyramidal crystals, which seemed to have a powdery matter distributed through their mass. Upon more narrowly examining these crystals, I found that this dust formed an inner pyramid, all the faces of which were parallel to the faces of the pyramid of amethyst. When two parallel faces were ground upon the pyramid, and perpendicular to its axis,
the particles of dust were seen by the microscope to consist each of several spicular crystals of titanium, crossing one another at angles of $60^\circ$ and $30^\circ$, and forming distinct groups. In one crystal there were two interior pyramids composed of these groups; and it will be seen, from the explanation which I shall presently give of this phenomenon, that there may be any number of such pyramids.

As the crystals of amethyst are supposed to have been produced by the gradual enlargement of a small crystal placed in an amethystine solution, we have only to assume that a solution containing titanium has been introduced into the amethystine solution at different times during the growth of the crystal. The small crystals of titanium will deposit themselves on each of the surfaces of the pyramid; and when the whole of the introduced titanium has been thus deposited, the enlargement of the amethyst will go on, leaving a pyramid of titanium crystals in its interior. If a second solution of titanium is introduced, a second pyramid of its particles will be formed in the same manner; and this process may be repeated any number of times.

If we now suppose that the amethystine solution is exhausted just when the titanium solution has deposited all its crystals, the completed crystal of amethyst will have its outer surfaces covered with spicular crystals of titanium, or the pyramid of titanium groups will be on the very outside of the pyramid of amethyst. I had the good fortune to find such a crystal, in which the coat containing the titanium is laid like varnish on all the faces of the pyramid, but only on the upper end of three of them, the lower end of these three faces having lain on the solution protected from the deposition of the titanium. This crystal is, I believe, unique, and possesses the great interest of exhibiting the very process by which it was formed.

The two phenomena which I have just described are shown in figs. 4 and 5.

5. Distribution of Titanium in Brazil Topaz.

In examining a great number of very imperfect crystals of Brazil topaz, I found many which contained crystals of titanium of a brilliant scarlet colour, with a tinge of yellow. These crystals were perfectly transparent, and occurred in seven different forms.

1. In flat amorphous plates, which were highly transparent.
2. In hexagonal plates, lying in different planes.
3. In transparent lines running in different directions, and, though continuous, lying in different planes.
4. In lines running inwards from the margin of the specimen, and terminating in small flat plates. See fig. 6.
5. In the most remarkable symmetrical forms like sceptres or maces, resembling some of those symmetrical cavities which I had previously found in the white topazes of New Holland*. See fig. 7.

6. In some specimens the plates of titanium are actually bent, as in fig. 8.

7. In little groups of transparent circular plates of a scarlet colour, and having concentric rings. When light is reflected from the separating faces of the titanium and topaz, it is almost completely polarized; and at greater angles than that of maximum polarization, colours of singular brilliancy cross the reflected images. These colours are doubtless connected with the fact, that at some of these faces there are three images of a luminous object seen by reflexion, one of the two outer ones being polarized oppositely to one of the double middle images, as in the case of the multiplication of images in composite crystals of calcareous spar†.


In the greater number of the crystals of garnet which I have had occasion to examine, I have found many crystals and cavities, and much amorphous matter. In one specimen, in particular, the included crystals form a larger mass than the garnet, which is merely a cement for holding them together. These crystals have various crystalline forms, while some are amorphous, though regularly crystallized in their interior. All these crystals are doubly refracting, and give the colours of polarized light from their small size.

In another specimen, many of the crystals, in the form of hexagons and rhombic plates, are opake, and exhibit by polarized light the remarkable phenomenon, which I had never before seen, of having luminous edges; so that when the rest of the crystal and all the field of view is dark, we observe hexagons and rhombs, and other geometrical figures, depicted in lines of red light. It is not easy to ascertain the cause of this singular appearance, because we cannot see the form of the crystals where the light exists; but I have no doubt that the luminous lines consist of light depolarized by reflexion from the sides of the hexagonal and rhombic plates, because the illuminating pencil is much larger than the crystals, and the crystals much smaller than the pupil of the eye, so that light must be reflected from the prismatic faces of the hexagons and rhombic plates if they have sufficiently broad faces, and that light so reflected must enter the pupil of the eye.

† See Phil. Trans. 1815, plate 15. fig. 2.
On Selenide of Mercury from the Hartz.

In this specimen and in others there are many spherical cavities, surrounded with sectors of polarized light, and also several amorphous masses of matter, round which there is also polarized light, indicating, as all the phenomena of the crystals do, that the matter of the garnet must have been in a soft state, and compressed by some force emanating from these cavities.

In another specimen of garnet, a large fissure in its interior is occupied with granular matter, which must have issued either from a burst cavity containing a fluid or a gas, or both; but what is very interesting, and what I have never observed in any other mineral, the matter has in several places formed circular crystals of singular beauty, some being very simple, and others very composite.

St. Leonard's College, St. Andrews,
December 11, 1852.

XL. On Selenide of Mercury from the Hartz.
By Professor Rammelsberg*.

Pure selenide of mercury was first met with by W. Tiemann, some considerable time since, in an abandoned mine in the neighbourhood of Zorge in the Hartz. He considered it to be native selenium, but it was shown by Marx to contain mercury. (See Schweigger's Journal, vol. liv. p. 223.) The other minerals with which we are acquainted, in which both the substances in question are met with, contain also sulphur, for instance, Onofrite from San Onofre in Mexico, or lead and copper, as is the case with the other seleniferous species of the Hartz.

Some little time since I received from M. Roemer, assessor of mines in Clausthal, a mineral from the Charlotte mine at that place, which he considered to be selenide of mercury, with the request that I would submit it to examination. I have done so, and the result completely bears out M. Roemer's supposition. It is massive, fine granular, blackish-gray, mixed with quartz, and interspersed here and there with red oxide of iron.

Heated in an open tube it gives off a strong odour of selenium, and is completely volatilized, with the exception of a residue of quartz, the interior of the tube being invested with a brownish-red and a white sublimate, and also with a deposition of metallic quicksilver. It is soluble only in aqua regia, and the solution, when the substance operated upon is pure, is perfectly free from sulphuric acid.

The result obtained from an analysis by means of chlorine,

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after deducting 31·8 per cent. of quartz and 2·5 per cent. of red oxide of iron, gave—

<table>
<thead>
<tr>
<th>Element</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Selenium</td>
<td>25·5</td>
</tr>
<tr>
<td>Mercury</td>
<td>74·5</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100·0</strong></td>
</tr>
</tbody>
</table>

The combination Hg Se would consist of 28·38 selenium, and 71·62 mercury, so that the above analysis gives rather a ratio which is more closely represented by the formula Hg³ Se.".

XLI. Laplace’s Equation, its Analogues, and the Calculus of Imaginaries. By Robert Carmichael, A.M., Fellow of Trinity College, Dublin*

It is the object of the following paper to exhibit the applicability of imaginary symbols of operation to integration. It will be evident that the employment of such symbols is in many cases indispensable if we would arrive at the most general results; that where the equations to be solved possess a symmetrical character, both the methods of deduction and the solutions thus derived are symmetrical, and not devoid of elegance; and finally, that the process of verification is in all cases simple and obvious.

The first four articles of the paper were published in the Cambridge and Dublin Mathematical Journal for May 1852, and are occupied with the discussion of Laplace’s equation and its more immediate analogues; the remainder of the paper treats of the more remote analogues. In reference to the former, it may be observed that it has been long recognized by mathematicians that the arbitrary portions of the solutions of partial differential equations can in general be expressed under three distinct forms. The first gives the arbitrary portion of the general solution as the sum of an infinite number of particular expressions, and though open to objection, is recommended by the circumstances that it is unaffected by any signs of integration, and is wholly free from arbitrary functions. The second, which is due to Laplace, expresses the arbitrary portion by means of Definite Integrals, under the signs of which arbitrary functions occur; and while it is, in the existing state of science, generally unattainable, yet when once arrived at, it has the advantage of enabling us to determine the arbitrary functions with considerable facility. The third and most common form is that which exhibits the same arbitrary portion in terms of arbitrary functions without any signs of integration; and, in contrast with the last, while in most cases it can be obtained by the aid of the Calculus of Opera-

* Communicated by the Author.


T
tions, it is objectionable from the difficulty of determining the arbitrary functions. It may be observed that this last form is unique, whereas in each of the two preceding cases we have obviously as many general solutions as we can obtain particular solutions of distinct types. The order in which these forms have been stated seems to be that of their chronological employment, although the reverse of this order is that of their logical filiation.

It is proposed in the following paper to exhibit general solutions of the class of partial differential equations to which the Laplacian equation belongs, under these three several forms. It is obvious that, as this class of equations contains no term in its right-hand member involving only the independent variables, the general solutions will contain no determinate expression, but will reduce themselves to the arbitrary portions solely. It will be found that the particular solutions employed in the first and second forms are duplicate, each indicating a second in close correlation with itself. In the case of the equation of the simplest type, the duplicate solutions are omitted as being evident.

The instrument employed in arriving at these solutions is known by the name of the Calculus of Imaginaries*, under which are included the symbols known as Duplets, Triplets, Quaternions, &c., which have of late occupied so considerable a share of the attention of the mathematical world. The laws of the system of Triplets here employed are analogous to those which govern Quaternions. The writer is not aware that any use has as yet been made of these instruments in connexion with the subject of the integration of partial differential equations, and believes the forms of the solutions themselves, as found by them, to be new. It will be seen that he has commenced with the equation of the simplest character, the solution of which is familiar to the reader, and by successively engaging the equations as they rise in order, has endeavoured to show the identity of the method applied to all.

1. It is known that a general solution of the equation
\[
\frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} = 0 \quad \ldots \quad (I)
\]
is given by
\[
U = \Sigma A e^{m_1x + m_2y},
\]
where \(m_1, m_2\) are connected by the relation
\[
m_1^2 + m_2^2 = 0.
\]
Now, by the ordinary Calculus of Imaginaries, or Duplets, this

relation is equivalent to

\[(m_2 + im_1)(m_2 - im_1) = 0,\]

where \(i\) is such an operation that

\[i^2 = -1.\]

Thus \(m_2\) has either of values \(\pm im_1\), and upon the introduction of these, the value of \(U\) becomes

\[\Sigma \Delta e^{m_1 x + i m_1 y} + \Sigma B e^{m_1 x - i m_1 y},\]

or

\[U = \left\{\begin{array}{c}
\Sigma \Delta e^{m_1 x} (\cos m_1 y + i \sin m_1 y) \\
+ \\
\Sigma B e^{m_1 x} (\cos m_1 y - i \sin m_1 y)
\end{array}\right\}, \quad (a_1)\]

which is a general solution of \((I.)\) in the first form. It is evident that this solution may be condensed, but we shall retain it in its present shape for the sake of symmetry.

Now since the arbitrary constants \(A\), \(B\), and \(m_1\) are independent of each other, we may obviously substitute for \(A\) and \(B\) arbitrary functions of \(m_1\), and the solution just found may then be put under the shape

\[U = \left\{\begin{array}{c}
\int \Phi (m_1) e^{m_1 x} \cos m_1 y \ dm_1 \\
+ \\
\int \Psi (m_1) e^{m_1 x} \sin m_1 y \ dm_1
\end{array}\right\}, \quad (a_1)\]

the limits of the integrals being supposed independent of \(x\) and \(y\). At first sight this might be regarded as a solution in the second form, that namely of a definite integral. It seems, however, more just to consider it as merely another shape of the first form, since the limits are indeterminate.

The general solution in the third form is given by

\[U = \left(\frac{d}{dy} - i \frac{d}{dx}\right)^{-1} \left(\frac{d}{dy} + i \frac{d}{dx}\right)^{-1} 0,\]

or is

\[U = \phi (x + iy) + \psi (x - iy), \quad (a_1)\]

and the coincidence of the previous with this is evident.

2. Similarly, a general solution of the higher equation, rendered famous by its connexion with the name of Laplace,

\[\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = 0 \quad \text{(II.)}\]

is given by

\[V = \Sigma \Delta e^{m_1 x + m_2 y + m_3 z},\]

where \(m_1\), \(m_2\), \(m_3\) are connected by the corresponding relation

\[m_1^2 + m_2^2 + m_3^2 = 0.\]
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Now, by the Calculus of Triplets, this relation is equivalent to

\[(m_3 + im_1 + jm_2)(m_3 - im_1 - jm_2) = 0,\]

where \(i\) and \(j\) are such operations that

\[i^2 = -1, \quad j^2 = -1, \quad ij = -ji.\]

Thus \(m_3\) has either of values \(\pm (im_1 + jm_2)\), and the solution assumes the form

\[V = \left\{ \sum A e^{m_1 x + m_2 y + (im_1 + jm_2)z} + \sum B e^{m_1 x + m_2 y - (im_1 + jm_2)z} \right\},\]

To obtain the form corresponding to \((a_1)\), we assume

\[m_1 = r \cos \alpha, \quad m_2 = r \sin \alpha,\]

and the value of \(V\) becomes

\[\sum A e^{m_1 x + m_2 y + i_p \cdot rz} + \sum B e^{m_1 x + m_2 y - i_p \cdot rz},\]

where

\[i_p = i \cos \alpha + j \sin \alpha; \quad r^2 = m_1^2 + m_2^2;\]

and a general solution of (II.) in the first form is

\[V = \left\{ \sum A e^{\sqrt{(m_1^2 + m_2^2)}z} \{ \cos (m_1 x + m_2 y) + i_p \cdot \sin (m_1 x + m_2 y) \} + \sum B e^{\sqrt{(m_1^2 + m_2^2)}z} \{ \cos (m_1 x + m_2 y) - i_p \cdot \sin (m_1 x + m_2 y) \} \right\}, \quad (a_2)\]

the duplicate of which, namely,

\[V = \left\{ \sum A e^{\sqrt{(m_1^2 + m_2^2)}z} \{ \cos (m_1 x + m_2 y) + i_p \cdot \sin (m_1 x + m_2 y) \} + \sum B e^{\sqrt{(m_1^2 + m_2^2)}z} \{ \cos (m_1 x + m_2 y) - i_p \cdot \sin (m_1 x + m_2 y) \} \right\}, \quad (a'_2)\]

is had by an obvious modification.

It may be well to preserve the solutions in these shapes, as it is probable that the quantity \(i_p\) and those corresponding to it bear some relation to the character of the problems whose law is expressed by the equation of which this is the solution.

As before, substituting for \(A\) and \(B\) arbitrary functions of \(m_1\) and \(m_2\), we can throw these solutions into the following shapes:

\[V = \left\{ \iint \Phi (m_1, m_2) e^{m_1 x + m_2 y} \cdot \cos \sqrt{(m_1^2 + m_2^2)}z \cdot dm_1 dm_2 \right\}, \quad (a_2)'\]

with its duplicate

\[V = \left\{ \iint \Psi (m_1, m_2) \cos (m_1 x + m_2 y) e^{\sqrt{(m_1^2 + m_2^2)}z} \cdot dm_1 dm_2 \right\}, \quad (a_2)''\]

the limits of the integrals in both cases being supposed indepen-
dent of the quantities \(x, y\) and \(z\), and their order being equal to
the number of the quantities \(m_1, m_2\). In this latter respect it
will be found that these solutions are only particular cases of a
general law.

The same remark applies to the solutions of (II.) just found
as to the solution of (I.) represented by the formula \((a_1)'\). They
are not to be regarded as solutions of (II.) in the second form,
but only as other shapes of the solutions in the first form, since
the limits of the integrals are indeterminate.

Poisson has furnished an integral of the Laplacian equation
strictly in the second form\(^*\), namely,

\[
4\pi V = \left\{ \begin{array}{l}
\int_0^{2\pi} \int_0^{2\pi} x\Phi(y + x\sin u \cos v \sqrt{-1}, \sin u \sin v \sqrt{-1}) \, du \, dv \\
\int_0^{2\pi} \int_0^{2\pi} x\Psi(y + x\sin u \cos v \sqrt{-1}, \sin u \sin v \sqrt{-1}) \, du \, dv \\
\end{array} \right\}
\]

\( \text{(b}_2 \text{)} \)

but he considers its value lessened by the circumstance of its
containing imaginaries under the signs of the arbitrary functions,
more especially since, in the application of these solutions, the
arbitrary functions ought to be discontinuous. The integral of
equation (I.) might have been expressed in a form similar to
this, but upon examination it will be seen that it is reducible to
the third given form \((c)_1\).

It now remains only to find the solution of the Laplacian
equation in the third or simple functional form, and this is
given by

\[
V = \left( \frac{d}{dz} + i \frac{d}{dx} - j \frac{d}{dy} \right)^{-1} \cdot \left( \frac{d}{dz} + i \frac{d}{dx} + j \frac{d}{dy} \right)^{-1} \cdot 0,
\]

or is

\[
V = \phi(x + iz, y + jz) + \psi(x - iz, y - jz), \quad \text{ \ \ (c}_3 \text{)}
\]

the coincidence of which with the first form is obvious. The
necessity of interpreting all the results of this article, which are
analytically complete, is sufficiently apparent, and the practical
value of the results mainly depends on their susceptibility of
such interpretation. The first form of solution is easily inter-
pretable by the ordinary principles of triplets. As regards the
two latter, the writer regrets his inability hitherto to satisfy the
same demand, and would solicit the attention of those who may
favour the present paper with a perusal, to this point.

It is evident that the solution of the polar form of Laplace's
equation is had by substituting in that just exhibited the polar
values for \(x, y\) and \(z\).

\* Mémoires de l'Institut, 1818.
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It may be well to add, that the solution of such an equation as
\[
\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = \Lambda e^{mx} \cos ny \sin pz
\]
is
\[
V = \frac{\Lambda e^{mx} \cos ny \cdot \sin pz}{m^2 - n^2 - p^2} + \phi + \Psi.
\]

3. Again, a general solution of the still higher equation of the same type
\[
\frac{d^2 W}{dx^2} + \frac{d^2 W}{dy^2} + \frac{d^2 W}{dz^2} + \frac{d^2 W}{dw^2} = 0 \quad \ldots \quad (\text{III.})
\]
is given by
\[
W = \Sigma \Lambda e^{m_1 x + m_2 y + m_3 z + m_4 w} + \sum _{i=1}^{n} \Lambda e^{m_1 x + m_2 y + m_3 z + m_4 w},
\]
where \(m_1, m_2, m_3, m_4\) are connected by the relation
\[
m_1^2 + m_2^2 + m_3^2 + m_4^2 = 0.
\]
Now, by the Calculus of Quaternions, this is equivalent to
\[
(m_4 + im_1 + jm_2 + km_3)(m_4 - im_1 - jm_2 - km_3) = 0,
\]
where \(i, j, k\) are such operations that
\[
i^2 = j^2 = k^2 = -1, \quad ij = -ji, \quad jk = -kj, \quad ki = -ik,
\]
and the solution becomes
\[
W = \left\{ \begin{array}{c}
\Sigma \Lambda e^{m_1 x + m_2 y + m_3 z + (im_1 + jm_2 + km_3)w} \\
\Sigma B e^{m_1 x + m_2 y + m_3 z - (im_1 + jm_2 + km_3)w}
\end{array} \right\},
\]
To obtain the form of this corresponding to \((a_1)\) and \((a_2)\), we assume
\[
m_1 = r \cos \alpha, \quad m_2 = r \cos \beta, \quad m_3 = r \cos \gamma,
\]
and we have
\[
W = \left\{ \begin{array}{c}
\Sigma \Lambda e^{m_1 x + m_2 y + m_3 z + i_R \cdot rw}, \\
\Sigma B e^{m_1 x + m_2 y + m_3 z - i_R \cdot rw},
\end{array} \right\},
\]
where
\[
i_R = i \cos \alpha + j \cos \beta + k \cos \gamma, \quad r^2 = m_1^2 + m_2^2 + m_3^2,
\]
and a general solution of \((\text{III.})\) in the first form is
\[
W = \left\{ \begin{array}{c}
\Sigma \Lambda e^{m_1 x + m_2 y + m_3 z \{ \cos \sqrt{(m_1^2 + m_2^2 + m_3^2)}w + i_R \cdot \sin \sqrt{(m_1^2 + m_2^2 + m_3^2)}w} \\
\Sigma B e^{m_1 x + m_2 y + m_3 z \{ \cos \sqrt{(m_1^2 + m_2^2 + m_3^2)}w - i_R \cdot \sin \sqrt{(m_1^2 + m_2^2 + m_3^2)}w}
\end{array} \right\}, \quad (a_3)
\]
and its duplicate

\[
W = \begin{cases}
\sum \Lambda e^{\sqrt{(m_1^2+m_2^2+m_3^2)}w} \left\{ \cos(m_1x + m_2y + m_3z) \\
+ i_n \cdot \sin(m_1x + m_2y + m_3z) \right\} \\
\sum B e^{\sqrt{(m_1^2+m_2^2+m_3^2)}w} \left\{ \cos(m_1x + m_2y + m_3z) \\
- i_n \cdot \sin(m_1x + m_2y + m_3z) \right\}
\end{cases} \quad \ldots (a_3')
\]

Again, as before, regarding \( \Lambda \) and \( B \) as arbitrary functions of \( m_1, m_2, m_3 \), we can throw these solutions into the following shapes:

\[
W = \begin{cases}
\iiint \Phi(m_1, m_2, m_3) e^{m_1x + m_2y + m_3z} \\
\cos \sqrt{(m_1^2+m_2^2+m_3^2)}w \cdot dm_1dm_2dm_3 + \\
\iiint \Psi(m_1, m_2, m_3) e^{m_1x + m_2y + m_3z} \\
\sin \sqrt{(m_1^2+m_2^2+m_3^2)}w \cdot dm_1dm_2dm_3
\end{cases} \quad \ldots (a_3')
\]

with its duplicate

\[
W = \begin{cases}
\iiint \Phi(m_1, m_2, m_3) \cos(m_1x + m_2y + m_3z) \\
e^{\sqrt{(m_1^2+m_2^2+m_3^2)}w} \cdot dm_1dm_2dm_3 + \\
\iiint \Psi(m_1, m_2, m_3) \sin(m_1x + m_2y + m_3z) \\
e^{\sqrt{(m_1^2+m_2^2+m_3^2)}w} \cdot dm_1dm_2dm_3
\end{cases} \quad \ldots (a_3'')
\]

the limits of the integrals in both cases being supposed independent of the quantities \( x, y, z \) and \( w \). The same remark which has been made upon the similar solutions of (I.) and (II.) will apply here with equal force.

By a modification of Poisson’s solution of the equation of oscillatory motion in an unlimited gas, we should obtain a solution of equation (III.) strictly in the second form, viz.

\[
4\pi W* = \begin{cases}
\pi \int_0^{2\pi} \int_0^{2\pi} w\Phi(x + w\cos u \sqrt{1}, y + w\sin u\cos v \sqrt{1}, z + w\sin u\sin v \sqrt{1}) \sin u\sin v \sqrt{1} du dv \\
\frac{d}{dw} \int_0^{2\pi} \int_0^{2\pi} w\Psi(x + w\cos u \sqrt{1}, y + w\sin u\cos v \sqrt{1}, z + w\sin u\sin v \sqrt{1}) \sin u\sin v \sqrt{1} du dv
\end{cases} \quad \ldots (b_3)
\]

* By a regular deduction of the integral from the equation

\[
W = \left( \frac{d}{dw} - i \frac{d}{dx} - j \frac{d}{dy} - k \frac{d}{dz} \right)^{-1} \cdot \left( \frac{d}{dw} + i \frac{d}{dx} + j \frac{d}{dy} + k \frac{d}{dz} \right)^{-1} \cdot 0,
\]

or, putting

\[
i \frac{d}{dx} + j \frac{d}{dy} + k \frac{d}{dz} = D,
\]
To complete the discussion of equation (III.), it now only
remains to find its solution in the third or simple functional
form, which is
\[ W = \phi(x + iw, y + jw, z + kw) + \psi(x - iw, y - jw, z - kw), \quad (3) \]
and whose correspondence with the third forms of the solutions
of equations (I.) and (II.), as well as whose coincidence with its
own first form, are both obvious.

4. It is evident, that, by the adoption of an extended system of
operations, regulated by laws similar to those already employed,
the same methods of solution may be applied to the general
equation containing \( n \) independent variables. However, when
the number of operations indicated by the letters \( i, j, k, \) &c. ex-
ceeds three, we can no longer attach to them distinct geometrical
conceptions. Thus the signification of \( i \), is purely analytical,
and on that account it seems unlikely that the examination of
the general equation would lead to any practical result.

To adopt the language of Sir William Hamilton, the quanti-
ties \( i, i_r, i_n, i_s, \) &c. are, severally, imaginary units; that is, their
moduli are positive unity and their squares negative unity. If
for each of these quantities we had substituted the root of nega-
tive unity, the solutions in this form could have been obtained
at once. Since, however, as has been before remarked, it is prob-
able that these several units bear some relation to the characters
from the equation
\[ 2W = (e^{uW} - e^{-uW})\phi(xyz) + (e^{uW} + e^{-uW})\psi(xyz), \]
a form can be obtained, which seems to the writer to possess somewhat
greater generality, viz. \( 4\pi W = \)
\[
\begin{align*}
&\left\{ \int_0^{2\pi} \int_0^{2\pi} w\Phi(x + iw \cos u, y + jw \sin u \cos v, z + kw \sin u \sin v) \sin u \sin v du dv \\
&+ \int_0^{2\pi} \int_0^{2\pi} w\Psi(x + iw \cos u, y + jw \sin u \cos v, z + kw \sin u \sin v) \sin u \sin v du dv, \right. \\
&\left. \int_0^{2\pi} \int_0^{2\pi} z\Phi(x + iz \cos u, y + jz \sin u \cos v) \sin u \sin v du dv \\
&+ \int_0^{2\pi} \int_0^{2\pi} z\Psi(x + iz \cos u, y + jz \sin u \cos v) \sin u \sin v du dv. \right. \\
\end{align*}
\]

From the distinct geometrical characters which we are able to assign to
the several symbols \( i, j, k, \) it would seem that their occurrence, so far from
being matter of objection, will yet be found to possess some important
bearing upon the problems, in whose solutions such symbols are exhibited.
of the problems, whose laws are represented, respectively, by the equations (I.), (II.), (III.), it has been thought better to state the solutions in their present form. It will be noticed that the imaginary unit in the solution of (II.) has the same reference to an unit circle, as that in the solution of (III.) has to an unit sphere.

Again, it is to be observed that we are not at liberty to write the second form of the solution of (II.) in the shape

\[ V = \sum A e^{m_x x + m_y y} (\cos m_1 z + i \sin m_1 z)(\cos m_2 z + j \sin m_2 z) \]

+ \[ \sum B e^{m_x x + m_y y} (\cos m_1 z - i \sin m_1 z)(\cos m_2 z - j \sin m_2 z) \],

as at first sight it might be supposed; and a similar remark will apply to the second forms of the general solution of (III.). In fact, neither in the Calculus of Triplets nor in that of Quaternions does the ordinary property of exponential functions, namely,

\[ f(T) \cdot f(T') = f(T + T'), \]

\[ f(Q) \cdot f(Q') = f(Q + Q') \],

hold good, unless \( T \) and \( T' \), \( Q \) and \( Q' \), be respectively co-directional*. In general, it will be seen that the statement of the solution in the above form is equivalent to the breaking up of the imaginary unit into its constituents, and to the illegitimacy of this process the fact now mentioned reduces itself.

5. As additional examples of the application of these imaginary symbols to integration, it is proposed to show that by their use we can obtain, very simply, integrals of several well-known differential equations, and that they may be employed with advantage generally when the equations to be solved are symmetrical.

(I.) As a first example, let the solution of the familiar equation

\[ ds^2 + dy^2 + dz^2 = ds^2, \]

which has recently occupied so much attention with French geometers, be sought.

It is evidently equivalent to

\[ (dz + idx + jdy)(dz - idx - jdy) = ds^2, \]

and the solution is given by the system

\[ \begin{align*}
    z + ix + jy &= \phi(s + \gamma_1) \\
    z - ix - jy &= \psi(s + \gamma_2)
\end{align*} \]

\[ \phi'(s + \gamma_1) \psi'(s + \gamma_2) = 1 \]

where \( \gamma_1 \) and \( \gamma_2 \) are arbitrary vectors.

(II.) The general solution of the equation

\[ \left( \frac{d^2 x}{ds^2} \right)^2 + \left( \frac{d^2 y}{ds^2} \right)^2 + \left( \frac{d^2 z}{ds^2} \right)^2 = \frac{1}{m^2} \]

* Proceedings of the Royal Irish Academy, p. 433.
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is, by an exactly similar process, given by the system
\[
\begin{align*}
    z + ix + jy &= \frac{1}{m} \Phi(s + \gamma_1) + a_1 s + b_1 \\
    z - ix - jy &= \frac{1}{m} \Psi(s + \gamma_2) + a_2 s + b_2 \\
    \Phi''(s + \gamma_1)\Psi''(s + \gamma_2) &= 1
\end{align*}
\]
where, as before, \(a_1, b_1, a_2, b_2\) are arbitrary constants.

Thus, by the combination of the system of the previous example with that just stated, we obtain the general representation of the curve of double curvature, whose curvature is constant.

(III.) To find the integral of the equation of vibratory motion of thin plates, namely,
\[
\frac{d^2 z}{dt^2} + b^2 \left( \frac{d^4 z}{dx^4} + 2 \frac{d^4 z}{dy^2} \frac{dy^2}{dy^2} + \frac{d^4 z}{dy^4} \right) = 0.
\]
Let
\[
i \frac{d}{dx} + j \frac{d}{dy} = D,
\]
and the equation becomes
\[
\frac{d^2 z}{dt^2} + b^2 D^4 z = 0,
\]
or
\[
z = e^{i \sqrt{b^2} t} \phi(xy) + e^{-i \sqrt{b^2} t} \psi(xy).
\]
Now
\[
\int_{-\infty}^{\infty} dw \cdot e^{-\sqrt{\pi} \cdot w^2} = \sqrt{\pi} ;
\]
and putting alternately
\[
\alpha = \sqrt{(ir \cdot bt)} \left( i \frac{d}{dx} + j \frac{d}{dy} \right),
\]
\[
\alpha = \sqrt{(-ir \cdot bt)} \left( i \frac{d}{dx} + j \frac{d}{dy} \right),
\]
we get for the required solution,
\[
\sqrt{\pi} \cdot z = \left\{ \begin{array}{l}
\int_{-\infty}^{\infty} dw \cdot e^{-\sqrt{\pi} \cdot w^2} \phi \{x + 2iw \sqrt{(ir \cdot bt)}, y + 2jw \sqrt{(-ir \cdot bt)}\}, \\
\int_{-\infty}^{\infty} dw \cdot e^{-\sqrt{\pi} \cdot w^2} \psi \{x + 2iw \sqrt{(-ir \cdot bt)}, y + 2jw \sqrt{(ir \cdot bt)}\}.
\end{array} \right\}
\]
(IV.) By a similar process the integral of the equation
\[
\frac{d^2 v}{dt^2} + b^2 \left( \frac{d^4 v}{dx^4} + \frac{d^4 v}{dy^4} + 2 \frac{d^4 v}{dy^2 dx^2} + 2 \frac{d^4 v}{dx^2 dy^2} + 2 \frac{d^4 v}{dx^4} \right) = 0
\]
would be $\sqrt{\pi v} = \int_{-\infty}^{\infty} dw \cdot e^{-w^2} \phi \{ x + 2iw \sqrt{(i_\nu \cdot bt)}, \ y + 2jw \sqrt{(i_\nu \cdot bt)}, \ z + 2kw \sqrt{(i_\nu \cdot bt)} \}$,

$$+ \int_{-\infty}^{\infty} dw \cdot e^{-w^2} \psi \{ x + 2iw \sqrt{(-i_\nu \cdot bt)}, \ y + 2jw \sqrt{(-i_\nu \cdot bt)}, \ z + 2kw \sqrt{(-i_\nu \cdot bt)} \}$$

As regards the last two results, it may be again observed that they too demand interpretation, and that upon their susceptibility of such their practical value will depend.

6. It is obvious that a method of integration similar to that just exhibited will apply to the equations

$$\left( \frac{dU_1}{dx} \right)^2 + \left( \frac{dV_1}{dy} \right)^2 = 0,$$

$$\left( \frac{dV_1}{dx} \right)^2 + \left( \frac{dV_1}{dy} \right)^2 + \left( \frac{dW_1}{dz} \right)^2 = 0,$$

$$\left( \frac{dW_1}{dx} \right)^2 + \left( \frac{dW_1}{dy} \right)^2 + \left( \frac{dW_1}{dz} \right)^2 + \left( \frac{dW_1}{dw} \right)^2 = 0.$$
M. W. Haidinger on the Iodo-quinine Compound,
is given by
\[ z = \left( \frac{d}{dt} + ah \left( i \frac{d}{dx} + j \frac{d}{dy} \right) \right)^{-1} \left( \frac{d}{dt} + ah \left( i \frac{d}{dx} + j \frac{d}{dy} \right) \right)^{-1}. 0, \]
or is
\[ z = \Phi(x - hiat, y - hjat) + \Psi(x + hiat, y + hjat). \]

Similarly, the solution of the equation representing vibratory motion in an unlimited gas, namely,
\[ \frac{d^2 u}{dt^2} = a^2 \left( \frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2} + \frac{d^2 u}{dz^2} \right), \]
is
\[ u = \Phi(x - hiat, y - hjat, z - hkat) + \Psi(x + hiat, y + hjat, z + hkat). \]

The process of verification of this solution is so simple and obvious, and that of derivation so easy, that they would seem to recommend the form in comparison with that given by Poisson.

Again, the following equation occurs in the Theory of the Tides:
\[ \frac{d^2 u}{dt^2} - a^2 \left( \frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2} + \frac{d^2 u}{dz^2} \right) = \Lambda \sin \frac{2\pi}{\lambda} (lx + my + nz - vt). \]

Its integral is at once
\[ u = \frac{2\pi a^2}{4\pi^2 \lambda^2} \sin \frac{2\pi}{\lambda} (lx + my + nz - vt) + \Phi + \Psi. \]

It may be observed, that the unit \( h \) must obviously be regarded as extra-spatial. In fact, any directive unit, such as \( i \cos \alpha + j \cos \beta + k \cos \gamma \),
cannot be commutative with \( i, j, k \).

Trinity College, Dublin,
July 1853.

XLII. On the Iodo-quinine Compound, investigated in its Optical Relations by Dr. Herapath and Professor Stokes. By W. Haidinger*.

The phænomena of pleochroism of crystals, and those of the occurrence of surface- and substance-colours (Oberflächen- und Körperfarbe) in one and the same individual, have often formed the subject of communications which I have had the honour to lay before the Academy.

It is crystals possessing both these peculiarities that today's discourse will treat of; but for the greatest part not from investigations which I was the first to make, but according to the results obtained by Dr. W. Bird Herapath in Bristol, and Prof. G. G. Stokes in Cambridge; still I have myself repeated and extended them, in order to show the connexion which exists between them and many of my former observations and views.

* From Poggendorff's Annalen, June 1853.
The crystals were discovered, exhibited, optically investigated, and described by Herapath; and indeed it was their having, by transmitted light, a greater polarizing power than the best tournamaline, which principally claimed his attention. The crystals can easily be procured in the following manner.

The disulphate of quinine of commerce is dissolved in hot acetic acid, in the proportion of about ten grains to one or two cubic inches, and when the solution is cooled somewhat, a few drops of a spirituous solution of iodine are added drop by drop; it is then allowed to remain some hours to get quite cold and to crystallize. The crystals are then transferred in some of the fluid upon glass, and are thus ready for examination as soon as they are dried. In a later communication, Herapath gives the formula \((C^{20}H^{12}NO^{2} \cdot I) + SO^{3} + 6HO\).

The results of two analyses A and B, and of the calculation C, were —

<table>
<thead>
<tr>
<th></th>
<th>A</th>
<th>B</th>
<th>C</th>
<th>Atom.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodine</td>
<td>32·6092</td>
<td>31·453</td>
<td>124</td>
<td>32·63</td>
</tr>
<tr>
<td>Sulphuric acid</td>
<td>10·612</td>
<td>10·844</td>
<td>40</td>
<td>10·52</td>
</tr>
<tr>
<td>Alkaloid</td>
<td>42·692</td>
<td>...</td>
<td>162 quinine 42·63</td>
<td>1</td>
</tr>
<tr>
<td>Water</td>
<td>14·1764</td>
<td>...</td>
<td>54</td>
<td>14·2152</td>
</tr>
<tr>
<td></td>
<td>100·0896</td>
<td>380</td>
<td></td>
<td>99·9952</td>
</tr>
</tbody>
</table>

As, however, the disulphate of the alkaloid obtained from the dichromatic crystals by a later process had not quite the same properties as the originally employed disulphate of quinine, Herapath considers the basis not directly as true quinine, nor yet \(\beta\)-quinine or quinidine, since it approaches nearer to what has been lately called \(\gamma\)-quinine; three bodies, of which \(\alpha\)-quinine is a trihydrate, \(\beta\)-quinine a bihydrate, and \(\gamma\)-quinine a monohydrate of the radical \(C^{20}H^{12}NO^{2}\).

As a somewhat complex expression would be needful for a perfectly clear verbal description of the crystals of the new compound of iodo-quinine now under discussion, I propose as a short, easily used expression, the name Herapathite, in remembrance of the discoverer, who also was the first to give an optical description of them. I follow in this the principle of specific nomenclature, which slowly but certainly will be extended and established. In the present case, a simple specific name, and indeed this very one, is still more desirable, since Dr. Herapath has succeeded in applying crystal plates of the new body as "artificial tournamalines" between the eye and the eye-piece of the microscope, an application which must certainly be called a real addition to optical apparatus.

In its optical relations, though mention is made of the metallic green surface colour, like the clytra of cantharides, or murexide, yet it was chiefly the phenomena of the dichroism by transmitted light, or of the different absorptions, which were investi-
gated in the four or six-sided tabular crystals in two positions, at right angles to each other. There follows as the result—

1. The substance-colour.—In common light, pale olive-green; in very thin plates, up to \( \frac{1}{0.01} \) th of an inch, colourless. In polarized light, if the light is polarized in the long direction of the crystal, transparent and colourless, at least with thin crystals; if the light is polarized in the transverse direction of the crystal, opaque and black, however thin the plates may be.

An increased interest in the crystals was excited by the following optical investigations of the metallic surface-colours which Professor Stokes made known at the meeting of the British Association in Belfast, September 1852. Abbé Moigno's *Cosmos* contains a report of the communication.

Professor Stokes found that in the same direction of polarization of the reflected ray a vitreous lustre answered to the nearly colourless tint, and the green metallic lustre to the dark or black tint. It follows, therefore,—

2. The surface-colour.—When polarized in the transverse direction of the crystals, with a tolerably perpendicular incident ray gives a metallic yellowish-green; with greater angles it varies to a dark steel blue.

As Professor Stokes discovered in the crystals here mentioned the property of polarized surface-colour*, independently of my observations of analogous crystals, and without having any knowledge of them, so in consequence of the great interest of the phenomenon, he has given in detail the various modifications of the observations and illustrative remarks respecting their nature.

When I read the report, the circumstance struck me particularly, that with similar polarizations a green metallic surface-colour should correspond to a complete black in the substance-colour. The black was in opposition to the perfect transparency of the light polarized perpendicularly to the preceding colours. I had thought, by observations on a tolerable number of bodies, to have satisfactorily established as a law, "that the surface- and substance-colours stand in a complementary relation to each other." To the "green" of the surface a "red" in the colour of the substance should have corresponded. It was now my earnest wish to examine the crystals myself. According to the method given in the *Cosmos*, I could not succeed in forming them; for through an unfortunate misprint, instead of "solution d'iode dans l'alcool," there were the words "solution d'acide dans l'alcool." I had quite forgotten, that even in May my honoured friend Wöhler had written to me about

* Dr. Herapath noticed that the metallic green reflected ray was a polarized beam, and assigned 41° as the angle necessary to produce polarization by reflexion.—Ed.
the substance, and even communicated a letter from M. Kindt in Bremen respecting it; also that an experiment was made, which, however, but imperfectly succeeded, and produced crystals far too small. Wöhler had also sent me some very small crystals, but I neglected to examine them with a high magnifying power. I now wrote afresh to Stokes and Wöhler. The first kindly sent me some crystals on a leaf of mica, and again communicated the process of their formation, as given above, and as it now has yielded distinct crystals to Dr. Ragsky likewise in our chemical laboratory. I afterwards compared with them Dr. Herapath's treatises.

Through the dichroscopic lens I now saw the dichroism, but for an accurate examination the crystals were quite too small. But when I brought the Herapathite crystals on the stage of the microscope with a magnifying power of ninety, everything was clear at a glance. The different pale olive-green, pale red, deep blood-red, black colours lying among one another were distinguishable in their true character, as well in ordinary as in polarized light, which with a compound microscope is very easily produced by simply laying a rhomboid of doubly refracting spar on the eye-piece; you have then the two images oppositely polarized close to each other, as in the dichroscopic lens. In order to have perfectly smooth surfaces to the spar, pieces of (thin) plate-glass are cemented on the faces of the rhomboid in order completely to restore the polish, which from the softness of the spar is so soon injured.

Now every single crystal was distinguished; you saw how some of them in different places were unequally thick; how the image in the direction in which the light is more absorbed was for the thicker parts quite black; "black as midnight," says Herapath, "even when the thickness of the crystal does not amount to \( \frac{1}{50} \) of an inch. But it was also seen that thinner places of the same crystal were dark blood-red; and that consequently the black itself showed this absence of all colour only because even the last red ray is absorbed by the thick crystal. For the effect on the surface, therefore, the colour of the substance must always be considered as red, and to such the metallic green polarized perpendicularly to the axis observed by Professor Stokes is its true complementary colour. The crystals of the iodo-quinine compound form, therefore, a fresh confirmation of the validity of the position (which is favoured by all the before-mentioned ones), that the surface-colour is connected with the substance-colour as its complement.

You could not easily have two kinds of crystals, which, in relation to the substance-colour, would be more similar to each other than the Herapathite of which we are now speaking, and the transparent Brazilian Andalusite,—the same pale olive-green polarized in the direction of the axis of the crystal, the same
hyacinth-red, dark blood-red, or black, according to the thickness of the plates or crystals polarized perpendicularly to the axis.

In the one, as with the other species, perfectly transparent pale greenish crystals or plates polarize, when crossed, to a complete black. If the crystals are thin, the crossing produces only red, just like the colour in the examination through the calcareous spar of the dichroscopic lens. Very thin plates of andalusite have a pale but evident red colour. If two such plates are laid in a parallel position on each other, the red vanishes, it is absorbed, and the thicker pale green aggregate is in a surprising degree lighter coloured than each single plate. Just so, also, the exceedingly thin crystal plates of the Herapathite arc evidently pale red; indeed there are many crystals, which, being unequally thick, exhibit both red and green parts. The more evident the red, so much the more in the examination by polarized light does the tint polarized in the direction of the axis approach to a perfect absence of colour, so that the crystals are not at all seen, while the image polarized perpendicularly to it appears more or less deep red.

The shades of colour of the "Herapathite" may be represented in the following form conformably to the above observations.

1. *Substance-colour.*—In common light in very thin crystal plates, pale red, between rose and brick-red; in thicker crystals, pale olive-green or greenish-gray, which with increased thickness is sometimes yellowish.

In polarized light, according to the thickness, the image polarized in the direction of the axis, colourless to a pale green; the image polarized perpendicularly to the axis, blood-red to black; black even with a thickness of \( \frac{1}{100} \)th of an inch.

2. *Surface-colour.*—Polarized perpendicularly to the axis, with an almost perpendicular incidence of the light grass-green, with a greater angle of incidence in the plane of the axis, verdigris, "drake" blue; dark steel-blue.

When polished on glass (*auf Glas aufpolirt*) the substance-colour is a reddish-brown, there appears no metallic green; but in all azimuths polarized perpendicularly to the plane of incidence, a not very bright surface blue.

Wöhler's green hydrochinon, which I examined some time since, gives when polished in all azimuths from a metallic pinchbeck-brown to a brass-yellow, in connexion with a violet-blue substance-colour.

In crystals, the pinchbeck-brown is polarized perpendicularly to the axis, corresponding to the tint of least transparency in the substance-colour. With greater inclinations polarized perpendicularly to the plane of incidence, blue is added, so that the colour in the lower image of the dichroscopic lens changes eventually into dark steel-blue.
If the colours of the two quinine combinations are compared, the following result is obtained:

<table>
<thead>
<tr>
<th>Substance-colour, polarized in the direction of axis</th>
<th>Hydrochinon.</th>
<th>Herapathite.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ditto, polarized perpendicularly to axis</td>
<td>Obscure violet-blue.</td>
<td>Greenish-white.</td>
</tr>
<tr>
<td>Surface-colour, polarized perpendicularly to the axis</td>
<td>Very dark violet-blue.</td>
<td>Very dark red.</td>
</tr>
<tr>
<td></td>
<td>Pinchbeck-brown.</td>
<td>Grass-green.</td>
</tr>
</tbody>
</table>

Now the substance-colour of pure iodine is yellow in different tints, the surface-colour blue. I examined the substance-colour lately for the purpose of comparison with some small crystals, which Dr. Ragsky had obtained by evaporation from a solution in bisulphuret of carbon. The long six-sided plates of about 128°, and four angles of 116°, were opaque in the thickest parts, at the edges changing through reddish-brown, dark honey-yellow into paler tints. There was, besides, evidently a difference in the intensity; the colour in the direction of the small diagonal of the rhomb of 128° polarizes paler, the colour in the direction of the long diagonal polarizes darker.

But during the observation through the microscope with a magnifying power of 90 diameters, I saw the volatilization was even progressing, and in a remarkable manner the crystal plates became thinner and thinner towards the centre of the hexagon, and of a lighter honey-yellow; then appeared a hole that grew larger and larger, while small particles were again deposited like a beard, especially on the edge parallel to the small diagonal of the rhomb of 128°.

It requires certainly no very violent supposition to detect the colour of the hydrochinon and of the iodine in the Herapathite. The less dark violet-blue is neutralized by the less deep yellow into the almost colourless weak greenish tint of the substance colour when polarized in the direction of the axis. The very dark violet, with the somewhat stronger yellow, leaves red behind; the metallic yellow with the blue gives metallic green. Certainly a comparison of this kind deserves still further attention, although for the present occasion it is only approximately ventured as a first attempt.

XLIII. *On a new Method for the Determination of Urea.* By
JOHN W. DRAFER, M.D., Professor of Chemistry and Physiology in the University of New York*.

This method depends on the fact, that urea in solution or in urine is immediately decomposed by nitroso-nitric acid, carbonic acid gas escaping with a brisk effervescence.

In practice, therefore, the determination of urea is reduced to the determination of carbonic acid, one of the simplest operations of chemistry.

It is to be recollected, that the carbonic acid thus set free will always be contaminated with a certain quantity of nitrous acid vapour; provision must therefore be made to exclude this source of error. In a series of experiments which I have been recently making on the constitution of urine, I have resorted to several different methods, but upon the whole have preferred to determine the urea as carbonate of baryta.

As at the present time the attention of physicians is closely directed to variations in the composition of the urine, and new methods have been published by Liebig and others, the details of this one may perhaps be acceptable. To the chemist, what I have already stated has made it intelligible.

Preparation of the Decomposing Liquid.—The substance employed for the decomposition of the urea is nitroso-nitric acid. It may be made by passing into strong nitric acid, kept cold by immersion in ice-water, nitrous acid arising from the decomposition of nitrate of lead by heat. The liquid assumes after a while a green colour, and evolves dense red fumes; it is then fit for use. But as this process is somewhat troublesome, I prefer the following.

Take a single cell of Grove’s voltaic battery, placing as usual dilute sulphuric acid in contact with the amalgamated zinc, and strong nitric acid in the porous cup in contact with the platinum. Connect the zinc and platinum together. The nitric acid soon begins to change colour by dissolving nitrous acid, and in a few minutes becomes green. It is then decanted for use.

Determination of the Urea in Urine.—Filter a portion of urine from the mucus it contains. The object of this is to prevent subsequent frothing.

In a bottle, a, fig. 1, of about 50 cubic centimetres capacity, place 5 grammes of filtered urine. Through the cork which closes the mouth of the bottle a small funnel tube, b, passes down to the urine. Through this, when everything is ready, the nitroso-nitric acid is poured. There passes likewise through

* Communicated by the Author.
On a new Method for the Determination of Urea.

Fig. 1.

the cork a second tube, \(c\), to convey away the carbonic acid which arises.

Attached to the last-mentioned tube by means of caoutchouc connecters are two little Wouffe's bottles, \(d\) and \(e\), each of about 50 cubic centimetres capacity. These are to be half-filled with a solution composed of a saturated solution of baryta water, diluted with its volume of water. The exit tube, \(f\), of the last Wouffe's bottle is connected with an aspirator, \(g\).

The aspirator is set in action, and the tightness of the joints proved by the passage of air-bubbles at the proper places. Then through the funnel \(b\), the nitroso-nitric acid is poured. A brisk effervescence instantly takes place in the urine, and the flow of water from the aspirator is so regulated that there is a constant succession of air-bubbles escaping from the end of the funnel-tube, \(b\), through the urine, and no carbonic acid gets out through that tube into the atmosphere. The decomposition should not be permitted to go on too fast. As the effervescence declines, more nitroso-nitric acid is added, and a slight gyrating motion given to the flask, \(a\), by the hand, the flexible connecter at \(c\) permitting this to be done.

This motion being persisted in for a few minutes, the air which is continually entering through the funnel-tube and passing in bubbles through the urine, finally removes from that liquid every vestige of carbonic acid. We know that this has taken place by its ceasing to sparkle on a momentary cessation of the motion, and that enough acid has been added by no minute bubbles being generated on the addition of a small quantity more.

The carbonic acid arising from the destruction of the urea is drawn by the aspirator through the baryta water, and the carbonate of baryta subsides. The vapours of nitrous or nitric acid drawn over form soluble salts. At the close of the decomposition 150 cubic centimetres of air are rapidly run through by the aspirator, to remove any remaining carbonic acid from the bottle \(a\).

The carbonate of baryta is next to be washed and weighed. The details are as follows:

A tin-plate cup, \(3\frac{1}{2}\) inches high and \(2\frac{1}{2}\) in diameter, \(aa\), fig. 2,
has two tubes, $b$ and $c$, opening into it. The tube $b$, 3 inches long and $\frac{3}{4}$ diameter, is closed at its outer extremity; and the tube $c$, 4 inches long and $\frac{4}{4}$ diameter, is open. The cup has three wire feet attached to it for supports. A glass funnel of such a size as to fit the cup in the way shown in the figure, is fixed therein water-tight by means of a cork at $d$. Water is now poured through the tube $c$ into the cup. It fills the tube $b$, and is allowed to reach the level, $aa$. A spirit-lamp is applied at $b$; when boiling takes place, the waste steam escapes through $c$, and the funnel is thus kept at 212° F.

On a filter in this funnel the contents of the two Woulfe's bottles are poured, and the mouth of the funnel loosely closed by a glass plate. The steam arising from the filter as it warms excludes the air for the most part; the advantage of this being, that it prevents the absorption of carbonic acid from the air.

The filtrate runs off quickly. The Woulfe's bottles are then carefully rinsed out with boiled water; this is put on the filter, and when it has gone through the carbonate of baryta, is thoroughly washed with water to which some ammonia has been added. When the washing is completed, the drop of water at the tip of the funnel is removed, and the glass plate taken from its mouth. The warm air now rises through the neck of the funnel, and the precipitate rapidly dries. By this manipulation the funnel serves the double purpose of a hot filter and a drying chamber.

The drying is continued until the carbonate has detached itself in flakes from the filter. The carbonate is then transferred to a platinum crucible, and the filter burnt in a loop of platinum foil, the incineration taking place without any difficulty. The weight is then determined, deducting the filter-ash.

A correction is then made, depending on the quantity of air which has been run through by the aspirator, and which introduced some carbonic acid. I have commonly employed about 1000 cubic centimetres, and assumed the quantity of carbonic acid at $\frac{1}{1000}$th of that volume. This gives the weight of carbonate of baryta which has arisen from the urea. Then,

As 198 : 60 : : weight of carbonate of baryta obtained : weight of urea.

It takes about two hours to make one determination in this way. If solutions containing known weights of urea dried at 212° F. are employed for testing the value of this process, the results will be found very accurate.

University, New York,
September 1, 1853.
XLIV. On the explicit Values of Sturm's Quotients.
By J. J. Sylvester, F.R.S.*

By Sturm's quotients is of course meant to be understood the quotients which result from applying the process for the discovery of the greatest common measure between \(fx\) (an algebraical function of the \(n\)th degree in \(x\), and whose first coefficient is unity) and \(f'x\) its first derivative, as in Sturm's theorem; or which is the same thing in effect, supposing \(\frac{f'x}{fx}\) to be represented by

\[
\frac{1}{Q_1} - \frac{1}{Q_2} + \frac{1}{Q_3} - \cdots - \frac{1}{Q_n}
\]

(where \(Q_1, Q_2, \ldots, Q_n\) are all linear functions of \(x\)), the quotients in question are \(Q_1, Q_2, \ldots, Q_n\). Before proceeding to discuss these quotients, it will be well to state the form under which the other quantities which appear in the course of the application of the Sturmian process admit of being represented. First, then, it will be remembered that the residues with the signs changed are all of the form

\[
R_i = M_i \sum \{\zeta(h_1, h_2, \ldots, h_i) (x - h_{i+1}) (x - h_{i+2}) \cdots (x - h_n)\},
\]

where \(\zeta(h_1, h_2, \ldots, h_i)\) indicates the squared differences between every two of the quantities \(h_1, h_2, \ldots, h_i\) and \(h_1, h_2, \ldots, h_n\) are supposed to be the \(n\) roots of \(fx\); and where using \(\xi_i\) to denote \(\sum \zeta(h_1, h_2, \ldots, h_i)\), with the convention that \(\xi_0 = 1, \xi_1 = n\), and understanding by \((i), \{1 + (-)^i\}\) \(\frac{1}{2}\),

\[
M_i = \frac{\xi_{i-2}^2 \cdot \xi_{i-4}^2 \cdots \xi_{(i)+1}^2}{\xi_{i-1}^2 \cdot \xi_{i-3}^2 \cdots \xi_{(i)}^2}.
\]

Here it will be observed that the only quantities appearing are the factors and the differences of the roots of \(fx\); and since these latter are the same as the differences between the corresponding factors (for \((x - h) - (x - h') = h' - h\), the entire quantity which expresses any residue \(R_i\) may be considered as a function of the factors of \(fx\) exclusively.

Again, if we solve the syzygetic equation

\[
N_i f'x + D_i f'x = R_p
\]

I have published many years ago in this Magazine the value of \(D_i\) and subsequently in a paper read before the Royal Society on the 16th of June last the value of \(N_p\) both which values are also functions.

* Communicated by the Author.
of the factors of $fx$ exclusively. $\frac{N_i}{D_i}$ it is easily seen, represents
the successive convergents to the continued fraction by which $\frac{f^i}{x}$ is supposed to be expressed, and $R_i$ (to a constant factor
près) is the denominator of the reverse convergents of the same
continued fraction. To the completion of this part of the theory
it evidently therefore becomes necessary to express the quotients
$Q_1, Q_2, Q_3, \ldots Q_{n-1} Q_n$ (of which the first $(n-1)$ are those
which appear in Sturm’s process, and the last is simply the
penultimate Sturmian residue divided by the ultimate residue)
under a similar form, i.e. as functions exclusively of the factors
of $fx$, or, which comes to the same thing, of the factors and the
differences of the roots. Guided by an instinctive sense of the
beautiful and fitting, in a happy moment I have succeeded in
grasping this much wished for representation, with which I pro-
pose now and for ever to take my farewell of this long and
deePLY excogitated theorem.

If we write

$$R_{i-1} = M_{i-1} \{ A_{i-1} x^{n-i+1} - B_{i-1} x^{n-i} + \&c. \},$$

and

$$R_i = M_i \{ A_i x^{n-i} - B_i x^{n-i-1} + \&c. \},$$

we have

$$A_{i-1} = \Sigma \zeta(h_1 h_2 \ldots h_{i-1}) B_{i-1} = \Sigma (h_i + h_{i+1} + \ldots + h_n) \zeta(h_1 h_2 \ldots h_{i-1})$$

$$A_i = \Sigma \zeta(h_1 h_2 \ldots h_i) B_i = \Sigma (h_{i+1} + h_{i+2} + \ldots + h_n) \zeta(h_1 h_2 \ldots h_i),$$

and the $i$th quotient is evidently

$$\frac{M_{i-1}}{M_i} \cdot A_i x + (A_{i-1} B_i - A_i B_{i-1});$$

and this is the quantity (unpromising enough in aspect) to be
transformed in the manner prescribed.

$M_{i-1}, M_i, A_i$ are already given under that form, and I
find that, putting

$$T_i = A_{i-1} \cdot A_i x + (A_{i-1} \cdot B_i - A_i B_{i-1}),$$

$T_i$ may be represented by the double sum

$$\Sigma \{ \Sigma \{ \zeta(h_{i_1} h_{i_2} \ldots h_{i_i}) (h_1 - h_{i_1}) (h_1 - h_{i_2}) \ldots (h_1 - h_{i_i}) \} \} (x - h_1).$$

This of course implies the truth of the identity

$$\Sigma \{ \Sigma \zeta(h_{i_1} h_{i_2} \ldots h_{i_i}) (h_1 - h_{i_1}) (h_1 - h_{i_2}) \ldots (h_1 - h_{i_i}) \}^2$$

$$= A_{i-1} \cdot A_i = \zeta_{i-1} \cdot \zeta_i,$$
in itself a truly remarkable equation, which it will be seen is of
2(i-1)^2 dimensions in respect of the roots*. When
\( i=1 \),
\[ T_1 = \Sigma (x - h_i); \]
and when \( i=2 \),
\[ T_2 = \Sigma \left\{ (\Sigma (h_i - h_\theta))^2 \right\} (x - h_1), \]
i. e.
\[ = \Sigma \left\{ ((n-1)h_1 - (h_2 + h_3 + \ldots + h_n))^2(x - h_1) \right\}. \]
When \( i=n \), \( T_n \) becomes
\[ \Sigma \{ \xi(h_1 h_2 \ldots h_n) \times \xi(h_2 h_3 \ldots h_n)(x - h_1) \}
\[ = \xi_n \times \Sigma \{ \xi(h_2 h_3 \ldots h_n)(x - h_1) \}, \]
as it evidently ought to do. Substituting for \( T_{i-1}, T_i \) and \( A_\theta \), their values, we have as the complete general expression of the
ith Sturmian quotient the following expression, in which, agree-
table to a notation which I have previously used and explained,
\[
\begin{bmatrix}
h_1 \\
h_\theta_1 \\
h_\theta_2 \\
\vdots \\
h_{\theta_{i-1}}
\end{bmatrix}
\]
means \((h_1-h_\theta_1)(h_1-h_\theta_2)\ldots(h_1-h_{\theta_{i-1}})\),
viz.
\[ Q_i = \frac{\xi_{i-1}^2}{\xi_i^2} \cdot \frac{\xi_{i-3}^4 \cdot \xi_{i-5}^4 \ldots \xi_{(i)}^4}{\xi_{i-2}^4 \cdot \xi_{i-4}^4 \ldots \xi_{(i)+1}^4} \times \]
\[ \Sigma \left\{ \left\{ \Sigma (\xi(h_\theta_1 h_\theta_2 \ldots h_{\theta_{i-1}}) \begin{bmatrix} h_1 \\ h_\theta_1 \\ h_\theta_2 \\ \vdots \\ h_{\theta_{i-1}} \end{bmatrix}) \right\}^2 \right\} (x - h_1) \}
\]
It ought not to be passed over in silence, that if we write
\[ \frac{1}{Q_1} - \frac{1}{Q_2} - \frac{1}{Q_3} - \ldots - \frac{1}{Q_i} = D_i(x)^j \]
and if we suppose \( N_i(x) \) and \( D_i(x) \) to be expressed integrally, and to be algebraically prime to one another, then
\[ D_{i-1}(x) = \Sigma \left\{ \xi(h_\theta_1 h_\theta_2 \ldots h_{\theta_{i-1}}) \begin{bmatrix} x \\ h_\theta_1 \\ h_\theta_2 \\ \vdots \\ h_{\theta_{i-1}} \end{bmatrix} \right\}. \]

* Thus if \( n=4 \) and \( i=2 \) \( \xi_{i-1}=4 \) \( \xi_2=\Sigma(h_1-h_\theta)^2 \),
and we have
\[ 4 \{ (h_1-h_2)^2+(h_1-h_3)^2+(h_1-h_4)^2+(h_2-h_3)^2+(h_2-h_4)^2+(h_3-h_4)^2 \}
\[ =(3h_1-h_2-h_3-h_4)^2+(3h_2-h_1-h_3-h_4)^2+(3h_3-h_1-h_2-h_4)^2 \]
\[ +(3h_4-h_1-h_2-h_3)^2, \]
and so in general \( \xi_{i-1} \cdot \xi_i \), which is the product of two sums of variable
numbers of squares, is expressible rationally as the sum of a constant num-
ber \((n)\) of squares for all values of \( i \).

† \((i)\) denotes \( \frac{1}{2}\{(-1)^i+1\} \).
On the explicit Values of Sturm's Quotients.

Hence $Q$, is contained as a factor in

$$(D_{i-1}, h_1)^2(x - h_1) + (D_{i-1}, h_2)^2(x - h_2) + \ldots + (D_{i-1}, h_n)^2(x - h_n).$$

It may be observed also, that for all values of $i$ between 1 and $n$ inclusively,

$$D_i h_1 + D_i h_2 + D_i h_3 + \ldots + D_i h_n = 0,$$

and also that the determinant

$$\begin{vmatrix} 1 & 1 & 1 & \ldots & 1 \\ (D_1 h_1)^2 & (D_1 h_2)^2 & (D_1 h_3)^2 & \ldots & (D_1 h_n)^2 \\ (D_2 h_1)^2 & (D_2 h_2)^2 & (D_2 h_3)^2 & \ldots & (D_2 h_n)^2 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ (D_{n-1} h_1)^2 & (D_{n-1} h_2)^2 & (D_{n-1} h_3)^2 & \ldots & (D_{n-1} h_n)^2 \end{vmatrix}$$

is always zero. To complete the theory, I subjoin the value of $N_b$, the simplified numerator of the $i$th convergent to $\frac{f'x}{fx}$, expressed as an improper numerator continued fraction.

Let the sum of the products of $x - h, x - k, \ldots x - l$ combined $i$ and $i$ together be denoted by $S_i(h, k, \ldots l)$, and the sum of the $i$th powers of the same by $\sigma_i(h, k, \ldots l)$, then

$$N_i = \sum \{ \xi(h_{o_1}, h_{o_2}, \ldots h_{o_i}) \times \sigma_{i-1}(h_{o_1}, h_{o_2}, \ldots h_{o_i}) - \sigma_{i-2}(h_{o_1}, h_{o_2}, \ldots h_{o_i}) \times S_1(h_{o_1}, h_{o_2}, \ldots h_{o_i}) + \sigma_{i-3}(h_{o_1}, h_{o_2}, \ldots h_{o_i}), S_2(h_{o_1}, h_{o_2}, \ldots h_{o_i}) \ldots \}.$$

The anomaly of the last term being of the form $(1 + \sigma_0) \cdot S_i$, instead of being $\sigma_0 \cdot S_i$, is not a little remarkable.

Of the four sets of Sturmian quantities, viz. the residues, the quotients, and the denominators and numerators of the convergents to $\frac{f'x}{fx}$, it will have been seen that the first and third are expressible in terms of the roots and factors by single summations of equal simplicity, the second and fourth by double summations, whereof that which corresponds to the numerators is much the more complicated of the two.

7 New Square, Lincoln's Inn,
September 3, 1853.
LET \( \frac{1}{a_1} + \frac{1}{a_2} + \frac{1}{a_3} + \) &c. be any continued fraction, and let the successive convergents \( \frac{1}{a_1}, \frac{1}{a_1 + a_2}, \) &c. be called \( \frac{N_1}{D_1}, \frac{N_2}{D_2}, \) &c., and let \( D_i \) be denoted by \( (a_1, a_2, \ldots, a_i) \), then the following identity obtains which I regard as the fundamental theorem in the theory of continued fractions, but which I have never seen stated in any work where this subject is treated.

Theorem.

\[
(a_1 \ldots a_m) \times (a_{m+1} \ldots a_{m+n}) + (a_1 \ldots a_{m-1}) \times (a_{m+2} \ldots a_{m+n}) = (a_1 \ldots a_m a_{m+1} \ldots a_{m+n}).
\]

Corollary 1.

\[
(a_1 a_2 \ldots a_m) \times (a_2 a_3 \ldots a_{m+1}) - (a_2 a_3 \ldots a_m) \times (a_1 a_2 \ldots a_{m+1}) = (-)^m \cdot 1.
\]

This is the well-known theorem

\[
D_i \cdot N_{i+1} - D_{i+1} \cdot N_i = \pm 1,
\]

which, however, is only a case of a much more general theorem easily deduced from the fundamental theorem given above. In fact, we may derive immediately from the latter, the equation

\[
(a_1 a_2 \ldots a_m) (a_2 a_3 \ldots a_{m+i}) - (a_2 a_3 \ldots a_m) (a_1 a_2 \ldots a_{m+i}) = (-)^m (a_{m+i} a_{m+i-1} \ldots) \text{ to } i-1 \text{ terms}.
\]

Hence

\[
D_{m-1} \cdot N_m - D_m \cdot N_{m-1} = (-)^m \cdot 1
\]

\[
D_{m-2} \cdot N_m - D_m \cdot N_{m-2} = (-)^m \cdot a_m
\]

\[
D_{m-3} \cdot N_m - D_m \cdot N_{m-3} = (-)^m (a_m a_{m-1} + 1)
\]

\[
D_{m-4} \cdot N_4 - D_4 \cdot N_{m-4} = (-)^m (a_m a_{m-1} a_{m-2} + a_m + a_{m-2}),
\]

&c.

Corollary 2.

\[
(a_1 \ldots a_\rho a_{\rho+1} \ldots a_{\rho+f}) (a_1 \ldots a_\rho a_{\rho+1} \ldots a_{\rho+h}) - (a_1 \ldots a_\rho a_{\rho+1} \ldots a_{\rho+g}) (a_1 \ldots a_\rho a_{\rho+1} \ldots a_{\rho+h}) = (-)^\rho \{(a_{\rho+1} \ldots a_{\rho+f}) (a_{\rho+1} \ldots a_{\rho+h}) - (a_{\rho+1} \ldots a_{\rho+g}) (a_{\rho+1} \ldots a_{\rho+h})\}.
\]

Sub-corollary.—If all the several quantities \( a_1, a_2, a_3 \) are equal to one another, the quantity \( D_f \cdot D_k - D_g \cdot D_h \) is constant

* Communicated by the Author.
† It is essential to notice that \( (a_1 a_2 \ldots a_i) = (a_1 a_{i-1} \ldots a_1) \).
in magnitude, but alternating in sign, so long as the differences of the indices $f, g, h, k$ are constant; and as an easy deduction from this sub-corollary, if

$$T_{n+1} = aT_n - bT_{n-1}$$

be the characteristic equation of a recurrent series, and if $f + k = g + h$, $\frac{T_f \cdot T_k - T_g \cdot T_h}{b^2}$ will be constant; and as a particular case of this deduction from the sub-corollary to the second corollary of the fundamental theorem, we have

$$\frac{T_{n+1}^2 - T_{n-1} \cdot T_{n+1}}{b^n} = \text{a constant},$$

i.e.

$$\frac{T_{n+1}^2 - aT_n \cdot T_{n+1} + bT_n^2}{b^n} = \text{a constant},$$

which is Euler's theorem. See Terquem's *Nouvelles Annales*, vol. x. p. 357, and November 1852.

I was led up to a knowledge of the fundamental theorem (be it new or old) by some recent researches connected with my new Rule of Limits, considered with reference to the conditions which must be satisfied when one of the limits found by the rule comes into actual contact with a root; a contact which I can demonstrate is always possible, as well for the superior as for the inferior limits, and with so much the fewer equations (as distinguished from inequations) of condition between the coefficients of the assumed auxiliary function which the application of the rule of limits requires, as there are fewer pairs of imaginary roots in the function whose roots are to be limited.

I may add that the fundamental theorem is an immediate result of the representation of the terms of the convergents to a continued fraction under the form of determinants. Thus, *ex. gr.*

the determinant

$$\begin{vmatrix}
  a & 1 \\
-1 & b & 1 \\
-1 & c & 1 \\
-1 & d & 1 \\
-1 & e & 1 \\
-1 & f
\end{vmatrix}$$

is obviously decomposable into

$$\begin{vmatrix}
  a & 1 \\
-1 & b & 1 \\
-1 & c
\end{vmatrix} \times \begin{vmatrix}
  d & 1 \\
-1 & e & 1 \\
-1 & f
\end{vmatrix} + \begin{vmatrix}
  a & 1 \\
-1 & b & 1 \\
-1 & c
\end{vmatrix} \times \begin{vmatrix}
  e & 1 \\
-1 & b & 1 \\
-1 & f
\end{vmatrix}$$
or into

\[
\begin{align*}
&\begin{bmatrix}
a & 1 \\
b & -1 \\
\end{bmatrix} \times \begin{bmatrix}
e & 1 \\
d & -1 \\
\end{bmatrix} + a \times \begin{bmatrix}
d & -1 \\
e & -1 \\
\end{bmatrix} \\
&\begin{bmatrix}
e & 1 \\
f & -1 \\
\end{bmatrix},
\end{align*}
\]

or into

\[
\begin{align*}
&\begin{bmatrix}
a & b \\
c & -1 \\
\end{bmatrix} + \begin{bmatrix}
e & 1 \\
d & -1 \\
\end{bmatrix} \\
&\begin{bmatrix}
e & 1 \\
f & -1 \\
\end{bmatrix},
\end{align*}
\]

\[i.e.\]

\[
\begin{align*}
(abcd) &= (abc) \cdot (def) + (ab) \cdot (cdef) \\
&= (ab) \cdot (cdef) + a \cdot (bcdef) \\
&= a \cdot (bcdef) + (cdef).
\end{align*}
\]

Thus the whole of the properties of continued fractions are deduced without algebraical calculation from a theorem which itself springs immediately by inspection from the well-known simple rule for the decomposition of determinants.

If instead of a simple set a triple set of quantities be taken, as

\[
\begin{bmatrix}
l_1 & l_2 & \ldots & l_{i-1} \\
m_1 & m_2 & \ldots & m_i \\
n_1 & n_2 & \ldots & n_{i-1} \\
\end{bmatrix},
\]

which, when \(i=1, i=2, i=3, i=4, \&c.\) is to be interpreted to mean

\[
m_1; \quad m_1 \quad l_1; \quad m_1 \quad l_1; \quad m_1 \quad l_1; \quad m_1 \quad l_1; \\
- n_1 \quad m_2; \quad - n_1 \quad m_2 \quad l_2; \quad - n_1 \quad m_2 \quad l_2; \\
- n_2 \quad m_3; \quad - n_2 \quad m_3 \quad l_3; \quad - n_3 \quad m_4; \\
\&c.\]

respectively, the value of the determinant represented by any such set being called \(T_i\), we have in general

\[
T_i = m_i \cdot T_{i-1} + (l_{i-1} \cdot n_{i-1}) \cdot T_{i-2}
\]

(which, when \(m_i\) and \(l_i \cdot n_i\) are constant, becomes the characteristic equation to an ordinary recurring series). The theorem corresponding to the fundamental theorem for such triple sets will be

\[
\begin{align*}
\left\{ \begin{array}{ccc}
l_1 & l_2 & \ldots & l_{i+v} \\
m_1 & m_2 & \ldots & m_{i+v+1} \\
n_1 & n_2 & \ldots & n_{i+v}
\end{array} \right\} & \times \left\{ \begin{array}{cccc}
l_{i+1} & l_{i+2} & \ldots & l_{i+v} \\
m_{i+1} & m_{i+2} & \ldots & m_{i+v+1} \\
n_{i+1} & n_{i+2} & \ldots & n_{i+v}
\end{array} \right\} \\
+l_l \times n_l \left\{ \begin{array}{ccc}
l_1 & l_2 & \ldots & l_{i-2} \\
m_1 & m_2 & \ldots & m_{i-1} \\
n_1 & n_2 & \ldots & n_{i-2}
\end{array} \right\} & \times \left\{ \begin{array}{cc}
l_{i+2} & l_{i+v} \\
m_{i+2} & m_{i+1} \\
n_{i+2} & n_{i+1}
\end{array} \right\}.
\end{align*}
\]

Lincoln's Inn, Sept. 15, 1853.
XLVI. Proceedings of Learned Societies.

ROYAL SOCIETY.

[Continued from p. 231.]

June 16, "ON a Theory of the conjugate relations of two rational integral functions, comprising an application to the Theory of Sturm's Functions, and that of the greatest Algebraical Common Measure." By J. J. Sylvester, Esq., M.A., F.R.S., Barrister at Law.

The memoir consists of four sections. In the first section, the theory of the residues obtained by applying the process of the common measure to two algebraical functions is discussed. It is shown that a certain superfluous or allotrious factor enters into each, the value of which, in terms of the leading coefficients of the residues in their simplified form, is determined; and the simplified residues themselves are subsequently obtained from the given functions by a direct method.

In the case where the two functions are of the same degree \( m \) in \( x, m \) functions of the degree \( m - 1 \) in \( x \) are formed, which, being identical with those employed in the process which goes by the name of Bezout's abridged method, the author terms the Bezoutics or Bezoutic primaries. By linear elimination performed between these, a second system of functions, whose degrees in \( x \) extend from \( m - 1 \) to 0, are formed, which he terms the Bezoutic secondaries: these Bezoutic secondaries are proved to be identical with the simplified residues. A similar theory is shown to be applicable in the general case of the functions being of unlike degrees. Other modes of obtaining the simplified residues by a direct method are also given. The coefficients of the primary system of Bezoutics form a square symmetrical about one axis, to which (as to every symmetrical matrix) a certain homogeneous quadratic function of \( m \) variables is appurtenant. This quadratic function is termed the Bezoutiant, the properties of which are discussed in the fourth section.

Every residue is what may be termed a syzygetic function or conjunctive of the two given functions; these being respectively multiplied by certain appropriate rational integral functions, their sum may be made to represent a residue. These multipliers are termed the syzygetic multipliers; and they form two series, one corresponding to the successive numerators, the other to the successive denominators of the convergents to the algebraical continued fraction which expresses the ratio of the two given functions. The residues are obviously a particular class of the conjunctives that can be formed from the given functions; every conjunctive has the property of vanishing when the two functions to which it is appurtenant vanish simultaneously; and in general, for any given degree in \( x \), an infinite number of such conjunctives can be formed.

In the second section, the author commences with obtaining in terms of the roots and factors of the two given functions, a variety of forms, all containing arbitrary forms of function in their several terms, and representing a conjunctive of any degree not exceeding
the sum of the degrees of the two given functions in its most general form. The author then reverts to the Bezoutic system of the first section, and obtains the general solution for the conjunctive of any given degree in $x$ in terms of the coefficients of the given function; by aid of this general solution he demonstrates that the residues obtained by the common measure process (divested of their allotrious factors), are the conjunctives of the lowest weight in the roots of the given functions for their several degrees; and obtains the value of this weight. He then demonstrates that certain rational but fractional forms ascribed to the arbitrary functions in the general expressions for a conjunctive in terms of the roots, will make these expressions integral and of the minimum weight; they will all be consequently identical (save as to a numerical factor) with one another, and with the simplified residues. The formulæ thus obtained for the simplified residues deserve particular attention on their own account, being double sums of terms, any single series of which is made up of fractions whose denominators are the products of the differences between a certain number of the roots of each one of the functions and a certain other number of the same combined in every possible manner, thus containing a vast extension of the ordinary theory of partial fractions. The author subsequently determines under a similar form, the value of each of the multipliers which connects the given functions syzygetically with the simplified residues, and establishes a general theorem of reciprocity, by aid of certain general properties of continued fractions, between the series of residues and either series of syzygetic multipliers.

The third section is divided into two parts. The first part is devoted to a determination of the values of the preceding formulæ in the case to which Sturm's theorem refers, where one of the given functions is the first differential derivative of the other; when this is the case the roots and factors of the second function are functions of those of the first, and it will be found that one of the polymorphic representations for the residue of any given degree will consist of terms, each of which is convertible into an integral function of the roots and factors of the given primitive function; in this way are obtained the author's well-known formulæ for Sturm's auxiliary functions. In like manner, the multiplier which affects the derivative function in the syzygy between the primitive, the derivative and any simplified residue, may also be expressed immediately as a sum of integral functions of the roots and factors of the primitive, complementary in some sort to the formulæ for the residues. The formula for the remaining syzygetic multiplier, (that which attaches to the primitive itself,) cannot be obtained directly by a similar method, but it is deduced by aid of the syzygetic equation itself, all the other of the five terms of which are known, or have been previously determined. The process of obtaining this last-named multiplier is one of great peculiarity and interest, and results in a form far more complex than that for the residues or for the othersyzygetic multiplier.

In the second part of the third section are contained some curious and valuable expressions for the residues and multipliers, communi-
cated to the author by M. Hermite; and an instantaneous demonstration is given of the properties of the author's formulae for Sturm's auxiliary functions in determining the real roots of an equation by a method quite irrespective of the theory of the common measure, and depending upon a certain extremely simple but unobserved law of quadratic forms, which he terms the law of inertia. In place of these formulae it is shown that others greatly more general, and possessing the same properties as regards the determination of the real roots, may be substituted; the known formulae are, however, the most simple that can be employed. The author then proceeds to inquire as to the nature of the indications afforded by the signs of a series of successive simplified residues, taken between any two functions independent of one another, instead of standing in the relation of primitive and derivative, as in Sturm's theorem; this leads to the theory of interpositions, of which it is shown that the Sturmian theorem may be treated (not so much as a particular case) as an easy corollary. In this part, the author obtains an entirely new rule for determining, in an infinite variety of ways, a superior and inferior limit to the real roots of any algebraical equation, whether numerical or literal.

The fourth section is also divided into two parts. In the first part, the index of interposition for two functions of the same degree is shown to be determinable by means of the quadratic form, previously termed the Bezoutiant; and as a corollary, it follows that the number of real roots of an equation of the degree \( m \) depends in a direct manner on the number of positive roots in another equation of the degree \( m-1 \), all of whose roots are real, and the coefficients of which are quadratic combinations of the coefficients of the given equation.

In the second part of this section, the Bezoutiant is considered under a purely morphological point of view. It is shown to be a combinative invariant of the two given functions (each treated as homogeneous functions of two variables), remaining unaltered when any linear combination of the two given functions is substituted for the functions themselves, and also when any linear substitutions are impressed upon the variables of the given functions, provided that certain corresponding substitutions are impressed upon the variables of the Bezoutiant. The family of forms to which the Bezoutiant belongs is ascertained, and a method given for finding the constituent forms of this family (one less in number than the number of odd integers not exceeding in magnitude the degree of either of the given functions which, throughout this section, are supposed to be of equal dimensions in \( x \), of which all other forms of the family will be numero-linear functions. The numerical coefficients connecting the Bezoutiant with this constituent group, are calculated for the cases corresponding to any index from 1 to 6 inclusive. Finally, the author remarks upon the different directions in which the subject matter of the ideas involved in Sturm's justly celebrated theorem admits of being expanded, and of which the most promising is, in his opinion, that which leads through the
theory of interpositions. Several of the theorems in this memoir have been previously published by the author, but they are here given along with a great deal of new matter in a connected form, and with the demonstrations annexed, for the first time.


The author begins his investigations by proving the existence of principal axes for any point of a body, which he makes to depend on the existence of principal axes of an auxiliary ellipsoid (Poinset's central one) having its centre at the given point, and such that any semidiameter of it is reciprocally proportional to the radius of gyration of the body about that semidiameter.

He afterwards employs another ellipsoid (called M'Cullagh's ellipsoid of inertia) concentric to the former and reciprocal to it, which admirably suits and facilitates the remainder of his investigations, and whose characteristic property is this, that it gives the radius of gyration itself (and not its reciprocal, as in Poinset's) about any semidiameter of it, the radius of gyration being in fact equal to the portion of that semidiameter between the centre and a tangent plane perpendicular to it.

He then proves that the attraction of a body of any shape, whose centre of gravity is O and mass is \( \mu \), on a very remote point \( P \) along \( OP = d \), is \( \frac{\mu}{d^2} + \frac{3}{2d^4}(A + B + C - 3M) \), \( A, B, C \) being the three principal moments of inertia of the body, and \( M \) its moment about \( OP \). And if M'Cullagh's ellipsoid of inertia be taken having O its centre, and its principal axes coinciding in direction with the principal axes of the body at O; and if a tangent plane to this ellipsoid perpendicular to \( OP \) at \( P' \) touch it in \( R \), it is shown that the component of the attraction of the body \( \mu \) on \( P \) in a direction perpendicular to \( OP \) is parallel to \( RP' \), and equal to \( \frac{3\mu}{d^4} \times OP' \times P'R \).

Next comes the proposition, "if two confocal ellipsoids attract an external point, their two resultants are coincident in direction and proportional to their masses," the truth of which is very easily inferred from Ivory's theorem. This proposition is then employed in proving that the expressions already found for the attractions of a body of any shape on a very remote point hold true likewise for the attractions of an ellipsoid (whether it be homogeneous, or only composed of concentric ellipsoidal strata having the same principal axes, and any variable but small excentricities) on any external point, whether near or remote.

To apply these reasonings to the case of the earth, the ellipsoid is then supposed to become a spheroid, and the attracted point \( P \) is supposed on its surface; then \( C = B \) and \( M = B \cos^2 \lambda + A \sin^2 \lambda \), \( \lambda \) being the angle \( OP = d \) makes with the equator; and so the central attraction along \( PO \), viz. \( \frac{\mu}{d^2} + \frac{3}{2d^4}(A + B + C - 3M) \), then becomes
\[
\frac{\mu}{d^2} + T(1 - 3 \sin^2 \lambda), \text{ where } T = \frac{3}{2d^2} (A - B) \text{: the attraction of the spheroid on P perpendicular to PO and urging P towards the equator is also easily shown to become } T \sin 2\lambda.
\]

Now that the point P may be at rest, it is necessary that the tangential component of the central force acting along PO should be equal to the sum of the tangential components of the centrifugal force (acting on P perpendicular to the earth's axis), and of the force perpendicular to PO; this condition gives an equation from which Clairaut's theorem followsinstantaneously, due regard being had to the difference of the polar and equatorial gravities as determined by the general expression \[
\frac{\mu}{d^2} + T(1 - 3 \sin^2 \lambda), \text{ and the ellipticity of the exterior surface being supposed so small that its square and higher powers may be rejected.}
\]

"On the Change of Refrangibility of Light."—No. II. By Professor Stokes, M.A., F.R.S.

The principal object of this paper is to explain a mode of observation by means of which the author found that he could exhibit, with ordinary day-light, the change of refrangibility produced by substances opaque as well as transparent, even when they possessed only a low degree of sensibility. The method requires hardly any apparatus; it is extremely easy in execution; and it has the great advantage of rendering the observer independent of sun-light. On these accounts the author conceives that it might be immediately applied by chemists to the discrimination between different substances. The method is as follows:—

A large hole, which ought to be several inches in diameter, cut in the window-shutter of a darkened room, serves to introduce the light, and a small shelf, blackened on the top, attached to the shutter immediately underneath the hole, serves to support the objects to be examined, as well as one or two absorbing media. The hole is covered by an absorbing medium, called by the author the principal absorbent, which is so selected as to let through, as far as may be, the feebly illuminating rays of high refrangibility, as well as the invisible rays still more refrangible, but to stop the rays belonging to the greater part of the visible spectrum. A second medium, called by the author the complementary absorbent, is chosen so as to be as far as possible transparent with regard to those rays which the first medium stops, and opake with regard to those which it lets through. The object to be examined is placed on the shelf, and viewed through the second medium. If the media be well-selected, they together produce a very fair approach towards perfect darkness; and if the object appears unduly luminous, that arises in all probability from "fluorescence." To determine whether the illumination be really due to that cause, it is commonly sufficient to remove the complementary absorbent from before the eyes to the front of the hole, when the illumination, if it be really due to fluorescence, almost wholly disappears; whereas, if it be due merely to scattered light
which is able to get through both media, it necessarily remains the same as before. In the case of objects which are only feebly fluorescent, it is sometimes better to leave the second medium in its place, and use a third medium, called by the author the transfer medium, which is placed alternately in the path of the rays incident on the object-end of the rays coming from it to the eyes.

Independently of illumination, the change of colour corresponding to the change of refrangibility, and the difference of colour with which the object appears, according as the transfer medium, or the complementary absorbent used as a transfer medium, is held in front of the eyes or in front of the hole, afford in most cases a ready mode of detecting fluorescence.

Instead of trusting to the absolute appearance of the object, it is commonly better to compare it with some fixed standard. The standard substance ought to be such as to scatter freely visible rays of all refrangibilities, but not to give out rays of one refrangibility when influenced by rays of another. The author employed a white porcelain tablet as such a standard; and the object to be observed was placed on the tablet, instead of being laid directly on the blackened shelf.

Another mode of observation consists in using a prism in combination with the principal absorbent. The object being placed on the tablet, a slit is held close to it, in such a position as to be seen, projected partly on the object and partly on the tablet, and the slit is then viewed through a prism. The fluorescence of the object is evidenced by light appearing in regions of the spectrum, in which, in the case of the rays coming through the principal absorbent, and, therefore, in the case of the rays scattered by the tablet, there is nothing but darkness.

The author states that these methods proved to be of such delicacy, that, even on an unusually gloomy day, he was able readily to detect the fluorescence of white paper; and even in the case of substances standing much lower in the scale, the fluorescence could be detected in a similar manner.

In conclusion, the author states that he had found the property of fluorescence to belong to a peculiar class of salts, the platino cyanides, making a third instance in which this property had been connected with substances chemically isolated in a perfectly satisfactory way. The present instance opens a new field of inquiry in relation to the polarization of the fluorescent light.

"Researches in Embryology; a Note supplementary to Papers published in the Philosophical Transactions for 1838, 1839 and 1840, showing the confirmation of the principal facts there recorded, and pointing out a correspondence between certain structures connected with the Mammiferous Ovum and other Ova." By Martin Barry, M.D., F.R.S., F.R.S.E.

Referring to his account of the process of fecundation of the mammalian ovum and the immediately succeeding phenomena, published in various papers in the Philosophical Transactions, the author calls attention to the confirmation which his views have received from corresponding observations made by subsequent inquirers on the ova.
of other animals. He more particularly adverts to a recently published memoir by Dr. Keber, in which that physiologist describes the penetration of the spermatozoon into the interior of the ovum, in Unio and Anodonta, through an aperture formed by dehiscence of its coats, analogous to the micropyle in plants.

Small pellucid vesicles, lined with ciliated epithelium and enclosing a revolving mulberry-like object, such as the author discovered imbedded under the mucous membrane of the rabbit’s uterus and described in the Philosophical Transactions for 1839, have been likewise observed by Keber, not only under the mucous membrane, but also and most frequently in some part of the cavity of the abdomen. Keber considers these bodies to be fecundated ova. The author agrees with Keber in considering them to be ova, but he does not suppose them to be fecundated, nor does he think that their membrane is the vitellary membrane (“zona pellucida”), which he believes to have been absorbed. He considers such ova to have been detached from the ovary along with their containing ovisac, which in their new situation constitutes the ciliated capsule, and as they present themselves in unimpregnated animals, he now believes that the formation of a mulberry-like group of cells from the germinal spot and the process of division and subdivision of the latter take place without fecundation; but when this happens, the mulberry is not found to contain one cell larger than the rest, the nucleus of which, according to his observations, is the embryo. He is further of opinion, that in all cases of separation of ova, the ovisac or internal coat of the Graafian follicle is detached from the ovary, either entire and along with the ovum, as in the instances alluded to, or after the ovum has first escaped by rupture, as in the instance of the fecundated ovum.

The author is led to the following conclusions with reference to the structures connected with the ovum in different animals:—1. That in the mammalia the vesicle he described as the foundation of the Graafian follicle, and termed the ovisac, does not remain permanently in the ovary, but is expelled and absorbed. 2. That in the Bird, the ovum, when escaping from the ovary, is accompanied by the corresponding vesicle—the ovisac; and that the ovisac becomes the shell-membrane of the Bird’s egg. 3. That the expelled and lost ovisac in the mammalia therefore corresponds to the shell-membrane in the Bird. 4. That after the formation of the ovum the albuminous contents of the ovisac in the mammalia correspond to the albumen in the Bird’s egg. 5. That the author’s retinacula in the mammalia, after all, find their analogue in the chalazae of the Bird; and that both have their origin in the granular contents of the ovisac, which at an early period are in appearance just the same in both. 6. That the shell-membrane of the Bird is thus a primary cell.

He then points out the position which from his observations is to be assigned to the several parts of the ovum in the language of “cells;” and shows the presence of a plurality of ova in a Graafian follicle to be referable to the same cause as that producing more than one yolke (ovum) in the Bird’s egg.

Not many years ago it used to be said, that the geology of England was done, and yet the best investigated localities are constantly affording fresh discoveries. When the Lecturer last year exhibited Captain Ibbetson’s beautiful and accurate model of Whitecliff Bay in the Isle of Wight, in illustration of his views respecting the distribution of species in time, he had not the slightest suspicion that this particular locality, so often and apparently so thoroughly explored, could yield new results and new interpretations. Nevertheless, having had occasion, at the suggestion of Sir Henry De la Beche, to examine the tertiary strata of the Isle of Wight for the purposes of the Geological Survey of Great Britain, this very bay of Whitecliff proved to be a rich source of novel geological information. Moreover, a great portion of the Isle of Wight, on further examination, turned out to belong to a division of the older tertiaries, that had never been demonstrated to exist within the British Islands. As a general statement of these results and of their bearings may be more intelligible to non-professional lovers of geology than the detailed memoirs about to be published on the subject, Professor Forbes has taken this opportunity of communicating them to the Members of the Royal Institution.

The Isle of Wight is divided into two portions by a great chalk ridge running east and west. This is the ridge of vertical chalk beds. To the north of it, the country is composed of tertiary, to the south, of older strata, as far down in the geological scale as the Wealden. The Lower Greensand or Neocomian beds occupy the greater part of the surface of the southern division, and freshwater tertiaries that of the northern. At Alum Bay, on the west, and Whitecliff Bay, on the east, the ends of the older tertiary strata, as they rise above the chalk, are seen truncated and upturned, being all affected by the movement which caused the verticality of the chalk. These tertiaries constitute the following groups, successively enumerated in ascending order: the Plastic clay, the Bognor series (equivalents of the true London clay), the Bracklesham series, and the Barton series, upon which lie the Headon Hill sands, and those freshwater strata that spreading out form the gently undulating country, extending from near the base of the chalk ridge to the sea.

Owing to the sections at Headon Hill near Alum Bay being so clear and conspicuous, and their position being in the loftiest tertiary hill that exhibits its internal structure in the island, the freshwater and fluvio-marine beds which compose that elevation have long attracted attention and have been described by many observers, the first of whom was the late Professor Webster. The apparent slight inclination of these beds, as seen in the Headon section, except at the point where they are suddenly curved in conformity with the verticality of the chalk and the beds immediately above it, appear to have led geologists to the notion that the fluvio-marine portion of the Isle of Wight was composed entirely of continuations of the beds forming Headon Hill. Two observers only suspected a discrepancy, viz. Mr. Prestwich, who, in a short communication to
the British Association at Southampton, expressed his belief that Hempstead Hill, near Yarmouth, would prove to be composed of strata higher than those of Headon; and the Marchioness of Hastings, who, having given much time to the search for the remains of fossil vertebrata in the tertiaries of the Isle of Wight and Hordwell, declared her conviction that these remains belonged to distinct species, according as they were collected at Hordwell, Hempstead, and Ryde, and that these three localities could not, as was usually understood, belong to the same set of strata. The recently published monograph of the pulmoniferous mollusks of the English Eocene Tertiaries, by Mr. Frederic Edwards, afforded also indications of the shells therein so well described and figured having been collected in strata of more than one age.

A few days' labour at the west end of the island convinced Professor Forbes that the surmises alluded to were likely to prove true, and that the structure of the north end of the island had been in the main misunderstood. After four months' constant work at both extremities and along the intermediate country, he succeeded in making out the true succession of beds, with most novel and gratifying results. During this work he was greatly aided by his colleague, Mr. Bristow, and by Mr. Gibbs, an indefatigable and able collector attached to the Geological Survey.

The freshwater strata of Whitecliff Bay proved to be wholly misinterpreted. Instead of their being constituted out of the Headon Hill strata only, more than a hundred feet thickness of them are additional beds characterized by peculiar fossils, and resting upon a marine stratum that overlies the Bembridge limestone, the equivalent of which at Headon is a soft concretionary calcareous marl, scarcely visible except in holes among the grass immediately under the gravel on the summit of the hill.

The beds of the true Headon series, in fact, are all included in the subvertical portion of the Whitecliff sections and are there present in their full thickness. They are succeeded by peculiar strata of intermediate character, for which the name of St. Helen's beds is proposed, and which become so important near Ryde that they constitute a valuable building stone. The Bembridge limestone that lies above is the same with the Binstead limestone near Ryde, out of which were procured the remains of quadrupeds of the genera Anoplotherium, Paleotherium, &c., identical with those found in the gypsiferous beds of Montmartre. The Seconce limestone near Yarmouth is also the same, and none of these limestones are identical with any of those conspicuous among the fluvio-marine strata at Headon Hill, and with which they have hitherto been confounded. They are far above them, and are distinguished by distinct and peculiar fossils.

Almost all the country north of the chalk ridge, exclusive of the small strip occupied by the marine Eocenes, is composed of marls higher in the series than any of the Headon Hill beds, and hitherto wholly undistinguished, except in the Whitecliff section, where the age and relative position had been entirely mistaken. These are the Bembridge marls of Professor Forbes. Above them are still higher beds preserved only in two localities, viz. at Hempstead Hill, to the west of Yarmouth, and in the high ground at Parkhurst. For these
the name of Hempstead series is proposed. Their characteristic fossils are very distinct, and the highest bed of the series is marine. These beds prove to be identical with the Limburg or Tongrien beds of Belgium and with the Grès de Fontainebleau series in France. We thus get a definite horizon for comparison with the Continent, and are enabled to show, that instead of our English series of Eocene terreries being incomplete in its upper stages as compared with those of France and Belgium, it is really the most complete section in Europe, probably in the world. We are enabled by it to correct the nomenclature used on the Continent, and to prove that the so-called Lower Miocene formations of France and Germany are in true sequence with the Eocene strata, and are linked with them both stratigraphically and by their organic contents. We are also enabled to refer, with great probability, the so-called Miocene terreries of the Mediterranean basin, of Spain and Portugal,—those of the well-known Maltese type,—to their true position in the series, and to place them on a horizon with the Tongrien division of the Eocenes. As these Maltese beds are unconfomable, and evidently long subsequent to the deposition of the great nummulitic formation, we are enabled to assign an approximate limit to the estimate of the latest age of that important series. From well-marked analogies we get at a probable date even for the Australian terreries. Thus the deciphering of the true structure of a small portion of the British terreries can throw fresh light upon the conformation of vast and far-apart regions.

The peculiar undulatory contour of the surface of the fluvio-marine portion of the Isle of Wight is due to the gentle rolling of these beds in two directions, one parallel with the strata of the chalk ridge, and the other at right angles to it. The valleys and hills running northwards to the sea depend upon the synclinal and anticlinal curves of the latter system of rolls, a fact hitherto unnoticed, and the non-recognition of which has probably been one cause of the erroneous interpretation of the structure of the Isle of Wight, hitherto received. The truncations of these curves along the coast of the Solent exhibit at intervals beautiful and much neglected sections, well worthy of careful study. There is one of these sections near Osborne. Her Majesty's residence stands upon a geological formation hitherto unrecognized in Britain. Near West Cowes there are several fine sections along the shore. The total thickness of unclassified strata in the Isle of Wight is 400 feet, if not more, and within this range are at least two distinct sets of organic remains. The fluvio-marine beds in all, including the Headon series, are very nearly 600 feet thick.

XLVII. Intelligence and Miscellaneous Articles.

ON AN IMPROVED REGULATOR FOR GALVANIC COIL APPARATUS AND MAGNETO-ELECTRIC MACHINES. BY G. GORE, ESQ.

HAVING found in using galvanic coils with a "Lockey's water regulator" to regulate the strength of the intermittent current, that by constant use the water in the regulator became dirty, and the electrodes became covered with metallic oxide, and with calca-
reous or other matters of inferior conducting power deposited from
the water by electrolysis, which soon diminished the available power
of the coil to a considerable extent, I was induced to invent a regu-
lator in which liquids were dispersed with, and consequently all
electrolytic action avoided, and which, whilst being unaltered in
conducting power by long-continued use, possessed that valuable
property of the water-regulator,—of enabling the power to be regu-
lated by inappreciable degrees instead of by sudden increases and
decreases, as in all other kinds of coil regulators.

The regulator I invented and have used in several instances for a
considerable period may be formed as follows:—Take equal quan-
tities of gold bronze powder, finely powdered black-lead, and finely-
powdered rotten-stone, and form them into a series of separate
powders, each of equal quantity, to the number of eighteen or twenty;
let the first consist entirely of bronze powder, the second of nearly
all bronze powder with a little black-lead, the third with an increased
proportion of black-lead and less bronze, and so on increasing the
proportion of black-lead and diminishing that of bronze in a regular
manner until you come to the middle powder, which should consist
entirely of black-lead; then begin to form them of black-lead and
rotten-stone, gradually decreasing the former and increasing the
latter until you come to the last one, which should consist entirely
of rotten-stone; well mix each powder, and keep them all separate
and in order: now take a moderately strong solution of shell-lac in
alcohol or in vegetable naphtha, and with equal quantities of it form
each powder into a rather firm paste, which should be put immedi-
ately into small bottles numbered according to the composition of
the paste and corked up; now take a glass tube of about 3 or 4
inches long, and about \( \frac{1}{4} \) or \( \frac{3}{8} \) of an inch in internal diameter, and fix
a cork in one end with a hole \( \frac{1}{4} \) of an inch in diameter in it, through
which fix a glass or metal tube of that diameter within the outer
tube and of the same length; also take a metal or glass tube of a
size and length suitable for ramming the composition in the annular
space between the two tubes, and with it carefully and firmly ram
equal portions of all the pastes into the space, commencing with
that formed entirely of bronze powder, and passing on in regular
order through the series to the last—that formed entirely of rotten-
stone, when the tube will be about filled; now carefully withdraw
the internal rod or tube, and set the composition tube in a dry place
for several days or a week until the whole of the spirit has evapo-
rated and the composition has become quite dry and hard, when you
will have a tube of good electrical conducting power (for induced cur-
rents of the usual intensity) at one end, and gradually decreasing in con-
ducting power until at the other end it is an excellent non-conductor.

To complete it as a regulator, fix a metal cap on the bottom or
bronze powder end and in close contact with the composition, also fix
another metal cap on the upper end with a regulating rod to work
up and down in it as in the ordinary water-regulator. The regula-
ting rod should be formed of thin, smooth, brass tube, of a size to slide
loosely within the composition tube, and slit at its lower end for
about \( \frac{1}{4} \) of an inch, and sprung slightly open at that end so as to press
steadily and smoothly upon the internal surface of the composition.
This regulator may be interposed in any circuit by connecting the two wires with the metal fittings on the two ends of the tube; but in order to give it an elegant appearance, I have generally placed the glass tube, or the composition tube alone insulated with paper, firmly within an ornamental brass tube in the form of a pillar, with the metal cap screwed in its upper end and its lower end screwed into an ornamental wood stand, and thus passed the current up the metal tube on the outside and down the composition tube in the inside, and dispensing with all unsightly wire connexions.

It will be evident from this description, that by raising or lowering the regulating rod, its lower end will come into contact with parts of the tube of different degrees of conducting power, and thus the power of the current will be regulated to the greatest nicety.

Though this regulator has, when carefully made, fulfilled its purpose very well, and been unaltered by long-continued use, it is probable that its composition might be rendered harder and less liable to fracture by accident if some little alteration was made in the kind of materials, so as to enable it to be baked in an oven and thus form a kind of ware somewhat similar to that in black-lead crucibles.

Birmingham.

**Meteorological Observations for Aug. 1853.**


Mean temperature of the month ........................................ 50° 69
Mean temperature of Aug. 1852 ........................................ 63° 67
Mean temperature of Aug. for the last twenty-seven years .... 62° 12
Average amount of rain in Aug. ...................................... 2° 35 inches.


**Sandwich Manse, Orkney.**—Aug. 1. Drizzle a.m.: rain, fair p.m. 2. Cloudy a.m.: damp p.m. 3. Showers a.m.: fair p.m. 4. Cloudy a.m.: showers p.m. 5. Cloudy a.m.: drizzle p.m. 6. Drizzle a.m. and p.m. 7, 8. Cloudy a.m. and p.m. 9. Bright a.m.: clear p.m. 10. Clear a.m.: clear, fine p.m. 11. Bright a.m.: cloudy p.m. 12. Cloudy a.m.: fine p.m. 13, 14. Cloudy a.m. and p.m. 15. Damp a.m.: cloudy p.m. 16. Cloudy a.m.: clear, fine p.m. 17. Showers a.m.: clear, fine p.m. 18. Clear a.m.: clear, fine p.m. 19. Damp a.m.: thunder and lightning, showers p.m. 20. Bright a.m.: thunder and lightning, showers p.m. 21. Bright a.m.: clear p.m. 22. Bright a.m.: showers p.m. 23. Cloudy a.m.: showers p.m. 24. Bright a.m.: clear, aurora p.m. 25. Clear, fine a.m.: drizzle p.m. 26. Rain a.m.: cloudy p.m. 27. Rain a.m.: damp p.m. 28. Damp a.m.: drizzle p.m. 29. Damp a.m.: cloudy p.m. 30. Cloudy a.m. and p.m. 31. Bright a.m.: drizzle p.m.

Mean temperature of Aug. for twenty-six previous years ...... 55° 08
Mean temperature of this month ....................................... 55° 98
Mean temperature of Aug. 1852 ....................................... 60° 64
Average quantity of rain in Aug. for seven previous years ... 2° 70 inches.

On the 24th, from 9½ till 10 p.m., a comet was seen with a long bright tail, about 5° above the horizon in the N.W. The nucleus like a star of the first magnitude.
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XLVIII. On the Methods of Measuring very Small Portions of Time, and their application to Physiological Purposes. By Professor Helmholtz*.

It is my intention today to direct the attention of the meeting to a series of experiments with which I have lately been occupied, and the object of which is to ascertain, by means of the refined methods of measurement furnished by modern physics, the duration of certain quickly-passing processes which occur in the living body. In the first place, I may be permitted, so far as it is necessary to the understanding of what is to follow, and so far as it is possible, without demonstration on the apparatus itself, to describe the means by which it is rendered possible to measure as small fractions of a second of time as those into which, by the help of the most powerful microscopes, our smallest unit of space, the line, may be divided.

The perception of small differences of time by means of our senses and without artificial help is not very fine, especially when the processes whose difference of time is to be determined are made the objects of different organs of sense, for example of the eye and ear, or even of different parts of the same organ, for example, of the eye in various portions of the field of view. The remarkable fact discovered by Bessel in the case of different individuals observing the transit of stars may be mentioned here. The observing astronomer perceives by the ear the tick of the clock, at the same time he sees the motion of the star towards the wire of the telescope, and endeavours to mark the positions of the star at the last tick before crossing, and at the

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first tick after crossing, and thus to determine at what point of the interval which has the two ticks for its limits, the star has actually crossed the wire. Every observer accustomed to measure in this way furnishes results which agree very well among themselves, but which differ more or less by a constant quantity from those obtained by other observers; experience teaches, that a divergence of more than an entire second is possible between different individuals. Assuming that two observers diverged both in opposite senses from the truth, it follows that, notwithstanding the most careful practice and the greatest attention, a man may, in the simultaneous determination of a perception of vision and a perception of hearing, commit an error of at least half a second. Our senses inform us more accurately when we have only to compare the perceptions of the same nervous fibres. When we see two flashes of light appear one after the other in the same place, we perceive them as double, if the time of interruption amount to the tenth part of a second; if the time between both flashes be smaller, then both the latter melt into one, as is illustrated by the common experiment of the continuous circle of fire when a glowing coal is swung speedily round. The ear perceives successive taps either as separate noises, or, when more than thirty-two such taps occur in the second, as a uniform and continuous tone, which increases in pitch with the speed at which the taps follow each other. From the height of such a tone we can certainly calculate with the greatest accuracy the number of shocks per second, and hence ascertain the amount of the interval between every two of them; but this kind of perception of differences of time cannot be adduced here, as the magnitude of the differences is not perceived immediately, but is inferred from the scientific knowledge of the process. We can grant to the ear the capability of detecting differences of time, as such, only so far as it is able to detect, say three, four, five, or six successive shocks quickly following each other; and this is only possible where the interval is not less than one-tenth of a second. We here find about the same limits as in the case of the eye.

The defect of the comparison, in point of time, of the perceptions of different nerves, seems to depend upon the circumstance that between them thought must come to consciousness: "now I perceive the first, but not yet the second—now the second also." The quickness of reflection here implied is, however, by no means so great as seems to be assumed in the expression "quick as thought." The considerable differences in the examples above cited in the case of astronomical observations might perhaps oe referred to the fact, that one observer reflects first on the perception of sound, and having brought it to consciousness, directs his attention to the field of view, while the other sees
first and hears afterwards, that is, regards those phenomena of vision as contemporaneous which he had brought to consciousness at the moment when he was disturbed by the sound. If, on the contrary, both perceptions fall upon the same nervous fibres, they melt into a total image of a sensuous perception which we retain in the memory, and by after reflection reduce into its component parts. This is the case when we observe three or four flashes of light, or hear as many sounds which are separated by an interval of one-tenth to half a second. We cannot count them while we perceive them, but do so afterwards by means of the tranquil image which they have left upon the memory.

Thus may the limits be stated within which the determination of portions of time without artificial aid is possible. If intervals of less than one-tenth of a second are to be observed with accuracy, or even measured, we must have recourse to artificial means. The methods made use of are based upon two different principles. In one case we convert the intervals of time into intervals of space, in the other we measure the mechanical effect which a force of known intensity has produced during the interval, and from this we calculate the time of action. The invention, or at least the accurate carrying out of these methods, has occurred within the last twelve years. The necessities of the artillery were the especial cause of experiments in this direction. To calculate the path of the ball, it is necessary to know its velocity and the time necessary for the ignition of the mass of powder. The defective apparatus which were formerly made use of for this purpose have been recently improved upon a degree which leaves nothing to be desired. In most cases the first of the two principles above mentioned forms the basis of the experiments; the differences of time are rendered measurable by converting them into differences of space. Siemens, formerly lieutenant of the Prussian artillery, is the inventor of an instrument for artillery purposes, which, as the most suitable example of instruments of this description, I will here describe.

Let us imagine an accurately formed steel cylinder which rotates round its axis (a single ring-shaped strip of the cylinder is all that is necessary). Let the surface be divided into any number of equal parts, say into 360 degrees. A fine non-rotating point is caused to approach the cylinder. If the cylinder rotate once in a second, then the 360 divisions pass by the point at a uniform velocity; hence each degree occupies the \(\frac{1}{360}\)th of a second in passing. Siemens, by means of clockwork and a conical pendulum, had given to his cylinder a velocity of 60 revolutions in a second. Hence in the course of a second 60 times 360 or 21600 divisions pass the point, or double as many, in round
numbers 40,000, half-degrees, which were marked upon the cylinder, each half-degree thus occupying the $\frac{1}{40,000}$th part of a second. Now if the point make two successive marks upon the cylinder, we know that in the interval between both marks, as many half-degrees will have passed by the point as there are forty-thousandths of a second in the interval. Thus far, indeed, the principle was carried out before Siemens; but the chief difficulty which long defied the ingenuity of experimenters, was to cause the times of marking upon the cylinder to correspond exactly with the times of commencing and ending of the process the duration of which was to be measured, for example, with the precise points of time when the projectile struck two successive objects placed in its path. The point was caused to press against the cylinder and to scratch it, and this was first effected by an observer who moved a lever as soon as he observed the striking of the projectile. The incompleteness of this method is at once evident from what we have already stated regarding the inaccuracy of our senses. The mechanical transmission of the shock of the ball against the body struck, without the intervention of an observer, requires also time, as a shock is transmitted through a solid body with measurable velocity. Breguet and Wheatstone went much further by making use of a galvanic current for the transmission of the effect. The point which makes the marks upon the cylinder is attached to a small lever, which is held by an electro-magnet, as long as the latter is magnetized by the current which circulates round it. As soon as the circuit is interrupted at any place, the marking-point falls upon the cylindrical surface and describes a line. The interruption of the current is caused by the ball tearing asunder a net of thin wire placed in its path, and which forms part of the circuit. The arrangement was for the most part such, that, immediately after the interruption of the current, it was again established and the point raised from the cylinder. By the rupture of a second net a second mark was made. However ingenious these instruments were constructed, it was not possible to make measurements with them to a greater degree of accuracy than the $\frac{1}{40,000}$th of a second, inasmuch as the marking-point, after being freed from the electro-magnet, required a certain time to fall upon the cylinder, and the velocity of the fall with unequal magnetic strength is not, as assumed by the inventors, constant, but variable. In this way, the exactitude, which otherwise might be ascribed to the principle of the rotating cylinder, was greatly damaged.

To Siemens occurred the happy idea of doing away with all the mechanical interventions, and of permitting the electricity itself to mark the times. If even a feeble electric spark passes from a fine point to a well-polished surface, it leaves behind a fine
dark spot which does not vanish until the surface has been polished anew. The electric spark endures only for an immeasurable instant of time; the propagation of the electricity through a wire of the length which comes into action here is also effected in a space of time vanishingly small. If, therefore, in the path of the ball a net be placed composed of insulated wires, which are alternately put in connexion with the external and internal coating of a Leyden jar, the rotating cylinder with the point being also included in the circuit, then the discharge will take place at the moment when the ball by passing through the net establishes the metallic connexion between its different wires. At the same moment the mark is made upon the surface of the cylinder. For one and the same ball any number whatever of wire nets may be made use of and corresponding marks obtained. This method permits of our availing ourselves of the entire accuracy of which the rotating cylinder is capable. If we omit taking into account the displacement of the net by the compressed air which precedes the ball, the time during which the ball passes over the space of half a line might be measured.

Further, the graduation and velocity of the cylinder stated above are by no means near the limits to which both might be extended; we can, indeed, form no judgement of the extent to which the accuracy of such measurements might be carried. Another process, indeed, depending upon a different application of the same principle, has led to far more astonishing results. In the case of the phenomena of light whose duration is to be determined, the difficulty is greatly diminished by making use of a rotating mirror instead of a cylinder. When we look at a mirror and slowly change the inclination of its surface, the images of external objects seem at the same time to move, and moreover with a velocity equal to double of that of the mirror. They move in a circle, the centre of which is in the axis of the mirror, and whose radius is equal to the distance of the object from the said axis. For the measurement of time, the image thus moving in a circle takes the place of the rotating cylindrical surface. Its momentary position may be observed with a telescope with the same exactitude as the mark upon the cylinder surface with the microscope. As, however, it moves in a circle of far greater radius, the accuracy of the measurement, when the telescope is a good one, increases in the proportion in which the radius of the apparent path exceeds that of the steel cylinder. The advantage is, that without increasing the mass of the moving body, by which the mechanical difficulties of construction would be greatly augmented, the velocity, by means of which we convert the time into apparent space, increases in such a great ratio. Supposing, for example, two electric sparks to occur successively at the same
spot, in the rotating mirror they will not appear in the same place if, in the interval between them, the position of the mirror has become sensibly changed; by measuring the angle under which the images appear, the interval between them may be calculated. The invention of the rotating mirror is due to Wheatstone, who made an experiment with it to determine the velocity of propagation of the discharge of a Leyden battery. The most striking application of the idea was made by Fizeau and Foucault during the present year, in carrying out a proposition made by Arago soon after the invention of the mirror; we have here determined in a distance of 12 feet no less than the velocity with which light is propagated, this is known to be nearly 200,000 miles a second; the distance mentioned corresponds therefore to the 77 millionth part of a second. The object of these measurements was to compare the velocity of light in air with its velocity in water, which, when the length is greater, is not sufficiently transparent. The most complete optical and mechanical aids are here necessary; the mirror of Foucault made from 600 to 800 revolutions in a second, while that of Fizeau performed 1200 to 1500 in the same time.

Finally, I have to mention a method of measuring time which depends upon a totally different principle. I have already indicated it by saying, that the time is to be calculated from the effect which a force of known magnitude is able to produce during the time. This force is the electro-magnetic action of a spiral of copper wire upon a magnet suspended by a fibre. I merely remind my hearers that a spiral composed of covered copper wire acts as a magnet, having a south pole at one end and a north pole at the other, as long as a voltaic current circulates through it. In the neighbourhood of this spiral let a magnet be freely suspended. As long as no current is present, the magnet performs smaller or larger oscillations under the directing influence of the earth’s magnetism, which diminish with extreme slowness and never entirely cease, inasmuch as feeble currents of air and alterations of the earth’s magnetic force constitute ever-new sources of motion. Let a current pass through the spiral. As long as it continues, one pole of the magnet is attracted by the adjacent end of the spiral and the other pole repelled. The motion of the pole will be thus changed; and according as its direction coincides with, or is opposed to that of the electro-magnetic force, it will be accelerated or retarded, or perhaps reversed. As soon as the current has ceased, the magnet once more makes regular oscillations, the magnitude of which changes very slowly, and hence can be determined with ease. These oscillations, however, on account of the motion imparted by the voltaic current to the magnet, will not be of the same magnitude.
as the former. As the laws of the motion of such a magnet are accurately known, it may be calculated with precision how much the velocity of the magnet must have been altered by the current in order to produce the observed change in the oscillations, and from this again may be determined how long the force must have acted in order to produce this effect. The best mode of observation is to permit the current to act when the magnet is passing the meridian, and when the direction of its motion coincides with that produced by the electro-magnetic force. In this case the calculation of the time is very simple; it is only necessary to multiply the difference between the arcs of oscillation before and after the operation of the electro-magnet with a constant factor. The magnitude of the latter depends only upon the strength of the current and the time of oscillation of the magnet. As the electro-magnetic force may be increased at pleasure by increasing the number of coils and of voltaic elements, it is possible in any time, however small, to produce a sensible effect upon the magnet.

In applying this method, it is necessary so to arrange matters that the commencement and the end of the galvanic current mentioned above shall exactly coincide with the beginning and end of the process whose duration is to be measured, which of course may be effected in different ways, dependent upon the special object of the measurement. This procedure possesses the great advantage, that it renders the clockwork with constant rotation unnecessary. Up to the present time, indeed, the problem of constructing such instruments is only approximately solved, and all of them require constant control. In short, simpler and more easily managed apparatus are necessary here. The first invention of such is due to Pouillet, in the year 1844; he made a proposition for artillery purposes which was applied practically in some cases, but has not been used further, on account of certain specialities which detract considerably from the accuracy of the instrument. After him I have been the first to make use of the method for physiological purposes. By observing the magnet in the highly convenient and delicate manner introduced by Gauss and Weber, which consists in attaching a mirror to the magnet, and determining the constant factor necessary to convert the differences of oscillation into differences of time, in a more accurate manner than Pouillet, I have been able with comparatively simple apparatus to make accurate determinations up to \(\frac{1}{10,000}\)th part of a second. To extend the delicacy of the measurement beyond this was of no interest to me, and would simply have unnecessarily increased the difficulty.

I now come to my measurements of the physiological processes*.

* Completely described in Müller's Archives, 1850.
You see the methods are here for making infinitely finer measurements than we need at present. The difficulty now is to apply the method to the special cases, to construct the connecting links between the process whose duration is to be determined, and the apparatus to be used for the determination. Indeed, the method must depend upon the object sought. In general I have found Pouillet's electro-magnetic method most advantageous, but for certain purposes the rotating cylinder is to be preferred.

The measurements which I have hitherto made refer partly to the duration of muscular contractions, partly to the velocity with which an impression made upon the nervous fibres is propagated through these fibres. The living muscles in the human and animal body are to be conceived of as strong elastic bands, which, stretched between certain portions of the bony scaffolding, in a tranquil position are either quite lax, or else their tensions completely neutralize each other. The elastic forces of these bands, however, possess the remarkable property that they can be suddenly changed by the influence of the nerves. The state thus brought about by the operation of the nerves is called the state of muscular activity. The active muscle behaves also as an elastic band, but it strives to shorten itself with far greater force than the inactive one. The consequence of this change in the living body is, that the force of the active muscle overpowers that of the inactive, the equilibrium of the members is destroyed, and the points at which the muscle is attached to the bones are caused to approach each other. In the living body the muscle receives the excitation to activity from the threads of nerves which ramify through it; these, in their turn, from the brain. Here the mysterious influence of the will imparts an excitation whose nature is unknown, which propagates itself throughout the entire length of the fibres, and arriving at the muscle excites it to action. If we modernise the comparison of Menenius Agrippa, who pacified the starving plebeians by wisely likening the state to the human body, then the nervous fibres might be compared with the wires of the electric telegraph, which in an instant transmit intelligence from the extremities of the land to the governing centre, and then in like manner communicate the will of the ruling power to every distinct portion of the land. The principal question which I have sought to answer is the following:—In the transmission of such intelligence, is a measurable time necessary for the ends of the nerves to communicate to the brain the impression made upon them; and on the other hand, is time required for the conveyance of the commands of the will from the brain to a distant muscle?

This, perhaps, appears improbable; in our own case we have
never perhaps experienced anything similar. When, however, we reflect on the limited accuracy of our perceptions, and that we naturally cannot perceive more quickly than our nerves, which are the necessary media of all our perceptions, transmit impressions, it is easy to see that the expression of our own experience can give us no information here. Earlier physiological theories were inclined to regard the agent which propagates itself through the nerves as electricity, and hence believed it necessary to ascribe to it an immense velocity. The latest admirable investigations of Du Bois Reymond on the electromotive action of the muscles and nerves have established facts which are perfectly inconceivable, except on the assumption that the propagation of an impression from the end of the nerves towards the brain is always accompanied by a change in the position of the ponderable molecules of the nervous fibres themselves; hence this propagation depends upon the action of one ponderable molecule upon the other. It belongs, therefore, to the same class of physical phenomena as that to which the propagation of sound belongs, and thus an inordinate velocity, such as that supposed, is rendered improbable.

I must commence with the simplest cases of the investigation. I chose the muscle of a frog connected with the nerves proceeding from it, but severed from the body of the animal. Such a muscle retains its vitality long enough to permit of two or three hours' continuous experiment without any considerable change, which is not at all the case with the detached muscles of warm-blooded animals. When any point of the nervous thread is injured by cutting, burning, or what is most effectual, when an electric current is sent through a portion of the nerve, this excitation produces the same effect as that which, in ordinary circumstances, is produced by the will. The muscle contracts, that is, it becomes active for a moment. The contraction passes so quickly, that its single stages cannot be observed. The problem to be decided is, whether the contraction takes place later when a distant portion of the nerve is excited than when the excited portion is nearer to the brain. To resolve this, we must measure the time which passes between the excitation and the contraction of the muscle. Experiment, however, soon showed that the activity of the muscle is by no means instantaneous, but appears some time after the excitation of the muscle, increases gradually to a maximum and then sinks, first quickly and afterwards by slow degrees; so that the greatest part disappears in about one-third of a second, but the remaining portion requires several seconds afterwards. This cannot be recognized in the muscles which act in obedience of the will, on account of the quickness of the contraction; but we may have observed it in the involun-
tary muscles, such as those of the entails, the iris, the fibres which are diffused over the surfaces of vessels, of the glands, &c. In these cases, the process, as is known, occupies from 100 to 1000 times the interval necessary in the former cases, so that we can conveniently observe the single stages. As, however, the commencement of the contraction is, according to this, not sharply defined, we cannot make use of it as the limit of the time to be measured, but we must avail ourselves of the occurrence of a certain stage of the contraction, that is, the moment when the activity of the muscle attains a certain measurable value. We must, however, at the same time assure ourselves that the differences of time, which it is our object to determine, must not be the consequences of an irregular muscular activity; that, on the contrary, the strength and duration of the contraction shall be exactly the same, whatever portion of the nerve may be excited. Our object therefore can only be attained by series of observations, which shall establish that all the stages of activity take place later when the excitation has to proceed through a greater length of nerve. This is, in point of fact, the case.

The measurements were performed by the electro-magnetic method. Their conditions require that the time-measuring current shall commence at the moment when an instantaneous excitement of the nerve takes place—the excitation was effected by a second electric current of vanishing duration—and that the time-measuring current shall end at the moment when a certain definite stage of the contraction is attained, that is, at a point when the tension of the muscle has increased to a certain degree. It is so arranged, that the muscle itself by its contraction interrupts the current, and must at the same time overcome the resistance of a certain weight, the current being thus broken at the moment when the tension of the muscle is sufficient to overpower the gravity of the mass attached to it. The place of interruption is formed by two pieces of metal which are connected with the two poles of a galvanic battery. As long as they are in contact, the current circulates without hindrance; as soon, however, as they are separated by the smallest conceivable space, the current ceases instantaneously. Hence it is not necessary to produce a motion of measurable extent, which would incur the loss of time; the time-measuring current, on the contrary, is interrupted as soon as the muscle commences to move one of the bits of metal, and this occurs as soon as the indicated degree of tension has been attained. That this theoretical deduction corresponds to the reality, I have convinced myself by particular controlling experiments. The series of measurements of the interval between excitation and contraction showed all the regularity that could be expected
in a case of the kind. The probable error of the mean value of successful series amounted to only \( \frac{1}{10} \) part of the whole value. The difference between the measurements in which different points of the nerve were excited was, on account of the shortness of the nerve, also very small, from one to two thousandths of a second; it was, however, ten times as great as the probable error of the results of the measurements. The most probable value of the velocity of propagation in the motor nerves of the frog I found to be 26.4 metres, about eighty feet per second. This quantity is indeed unexpectedly small, more than ten times less than the velocity of sound in the air.

For warm-blooded animals the method described is not applicable, because it requires series of measurements which occupy from one to two hours, during which the state of the body experimented with must remain constant. I have therefore had an apparatus with a rotating cylinder constructed by M. E. Rekoss, with which I have made the first trial experiments on frogs, and which may perhaps be made use of with warm-blooded animals. The principle of the instrument is not quite the same as in the apparatus of Siemens. The glass cylinder, constructed with great exactness, stands vertical; for the purposes of experiment its surface is covered with a thin coating of lampblack; against this a point can be made to press; the point is attached to a lever which is connected with the muscle, and when the latter contracts, the point is elevated. As long as the point remains at the same elevation, it simply describes a horizontal circle round the rotating cylinder. If the cylinder stand still and the muscle contract, a vertical line is drawn upon the surface of the cylinder; but if the cylinder rotates during the contraction of the muscle, a curve which first ascends and afterwards descends is produced. If two different points of a nerve be excited successively, and if the moments of excitation coincide exactly with one and the same position of the point upon the rotating cylinder, two congruent curves are produced, which, however, appear moved towards each other in a horizontal direction. The magnitude of the displacement corresponds to the time of propagation in the length of nerve between the two points of excitation. In this case, also, each single experiment shows whether the duration and strength of the contraction were equal in both instances. If this be the case, the two curves are congruent; if not, incongruent. Thus each single experiment here takes the place of a whole series of experiments according to the former process; but it must be confessed, that, up to the present time, I have not attained the same degree of exactness and agreement in the results.

How stands the question in the case of man? We must experiment on man under much more complicated conditions than
with the frog. Not only can we not remove the still unknown influence of the nervous conduction in the brain and the spinal column, but we must actually make use of them in the course of experiment. After, however, having established by rigorous experiments that in the nerves of the frog a sensible time is required for the propagation of an impression, I believe I need not hesitate to indicate the results of the experiments which up to the present time I have made upon the human subject.

The intelligence of an impression made upon the ends of the nerves in communication with the skin is transmitted to the brain with a velocity which does not vary in different individuals, nor at different times, of about 60 metres (195 feet) per second. Arrived at the brain, an interval of about one-tenth of a second passes before the will, even when the attention is strung to the uttermost, is able to give the command to the nerves that certain muscles shall execute a certain motion. This interval varies in different persons, and depends chiefly upon the degree of attention; it varies also at different times in the case of the same person. When the attention is lax, it is very irregular; but when fixed, on the contrary, very regular. The command travels probably with the above velocity towards the muscle. Finally, about \( \frac{1}{100} \)th of a second passes after the receipt of the command before the muscle is in activity. In all, therefore, from the excitation of the sensitive nerves till the moving of the muscle, \( 1\frac{1}{4} \) to 2 tenths of a second are consumed. The measurements are effected similarly to those on the frog. A slight electric shock is given to a man at a certain portion of the skin, and he is directed the moment he feels the stroke to make a certain motion as quickly as he possibly can, with the hands or with the teeth, by which the time-measuring current is interrupted. We are therefore only able to measure the sum of the intervals above indicated. When, however, the impression is caused to proceed from different spots of the skin, some nearer to the brain and others more distant, we change only the first member of the above sum, that is, the velocity of propagation in the nerves. At all events, we may, I think, assume that the duration of the processes of perceiving and willing in the brain does not depend upon the place on the skin at which the impression is made. I must, however, confess that this is not a strictly proved fact; it can only be proved that the duration does not depend upon the sensitiveness of the place of excitement, or on any particular physiological relations between it and the moving muscle. Our indication is rendered probable by the fact, that the numerical values of the velocity of propagation, deduced from observations in which the impression was received by the ear, the skin of the face, the neck, the hands, the loins and the feet, exhibit a sufficient agreement. It is found, for example, that intelligence
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from the great toe arrives about \( \frac{1}{50} \)th of a second later than from the ear or the face. If from the measured sum of the single intervals be subtracted that which belongs to the conduction in the sensitive and motor nerves, and also the time, determined by other experiments, during which the muscle puts itself in motion, the remainder is the time which passes while the brain is transferring the intelligence received through the sensitive nerves to the motor ones.

Other experiments on man which corresponded to those on the frog, inasmuch as the motor nerves were directly excited, have up to the present time given no exact results, but they suggest other interesting relations connected with the subject. It is possible, for example, to cause the muscles of the fore-arm to contract exactly like those of the frog by means of very feeble electric shocks imparted to the nerves through the skin. In this case both hand and fingers are contracted; and it is shown that these motions are totally independent of the influence of the will, because the will, informed of the shocks by the sensible nerves, cannot exert itself sufficiently soon upon the muscles. Such a series of experiments, in which the hand fell back very speedily, and when the very object sought was to retain it in the bent position which it was caused to assume through the contractions produced by the electric shocks, failed totally, because the influence of the will first reached the muscle after the hand had fallen back again, and simply raised it a second time.

If we reflect on what has been said at the commencement of this discourse regarding the inaccuracy of our impressions of time, we see that the differences of time in the nervous impressions, which we are accustomed to regard as simultaneous, lie near the limits of our capability of perception, and that finer differences cannot be appreciated simply because the nerves cannot operate more quickly. We are taught by astronomy, that on account of the time taken to propagate light, we now see what has occurred in the spaces of the fixed stars years ago; that, owing to the time required for the transmission of sound, we hear after we see, is a matter of daily experience. Happily the distances are short which have to be traversed by our sensuous perceptions before they reach the brain, otherwise our self-consciousness would lag far behind the present, and even behind the perceptions of sound; happily, therefore, the distances are so short that we do not observe their influence, and are therefore unprejudiced in our practical interest. With an ordinary whale the case is perhaps more dubious; for in all probability the animal does not feel a wound near its tail until a second after it has been inflicted, and requires another second to send the command to the tail to defend itself.
XLIX. On the Homographic Transformation of a Surface of the Second Order into itself. By A. Cayley, Esq.*

The following theorems in plane geometry relating to polygons of any number (odd or even) of sides are well known. "If there be a polygon of \((m+1)\) sides inscribed in a conic, and \(m\) of the sides pass through given points, the \(m+1\)th side will envelop a conic having double contact with the given conic." And "If there be a polygon of \((m+1)\) sides inscribed in a conic, and \(m\) of the sides touch conics having double contact with the given conic, the \(m+1\)th side will envelop a conic having double contact with the given conic." The second theorem of course includes the first, but I state the two separately for the sake of comparison with what follows.

As regards the corresponding theory in geometry of three dimensions, Sir W. Hamilton has given a theorem relating to polygons of an odd number of sides, which may be thus stated: "If there be a polygon of \((2m+1)\) sides inscribed in a surface of the second order, and \(2m\) of the sides pass through given points, the \((2m+1)\)th side will constantly touch two surfaces of the second order, each of them intersecting the given surface of the second order in the same four lines."*

The entire theory depends upon what may be termed the transformation of a surface of the second order into itself, or analytically, upon the transformation of a quadratic form of four indeterminates into itself. I use for shortness the term transformation simply; but this is to be understood as meaning a homographic transformation, or in analytic language, a transformation by means of linear substitutions. It will be convenient to remark at the outset, that if two points of a surface of the second order have the relation contemplated in the data of Sir W. Hamilton's theorem (viz. if the line joining the two points pass through a fixed point), the transformation is, using the language of the Recherches Arithmétiques, an improper one, but

* Communicated by the Author.
† See Phil. Mag. vol. xxxv. p. 200. The form in which the theorem is exhibited by Sir W. Hamilton is somewhat different; the surface containing the angles is considered as being an ellipsoid, and the two surfaces touched by the last or \((2m+1)\)th side of the polygon are spoken of as being an ellipsoid, and a hyperboloid of two sheets having respectively double contact with the given ellipsoid: the contact is, in fact, a quadruple contact at the same four points; real as regards two of them in the case of the ellipsoid, and as regards the other two in the case of the hyperboloid of two sheets; and a quadruple contact is the coincidence of four generating lines belonging two and two to the two series of generating lines, these generating lines being of course in the case considered by Sir W. Hamilton, all of them imaginary.
that the relation contemplated in the conclusion of the theorem (viz. that of two points of a surface of the second order connected by a line touching two surfaces of the second order, each of them intersecting the given surface of the second order in the same four lines) depends upon a proper transformation; and that the circumstance that an even number of improper transformations required in order to make a proper transformation (that this circumstance, I say), is the reason why the theorem applies to polygons in which an even number of sides pass through fixed points, i.e. to polygons of an odd number of sides.

Consider, in the first place, two points of a surface of the second order such that the line joining them passes through a given point. Let \( x, y, z, w \) be current coordinates*, and let the equation of the surface be

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(a \ldots)(x, y, z, w)^2 = 0,
\]

and take for the coordinates of the two points on the surface \( x_1, y_1, z_1, w_1 \) and \( x_2, y_2, z_2, w_2 \) and for the coordinates of the fixed point \( \alpha, \beta, \gamma, \delta \). Write for shortness

\[
(a \ldots)(\alpha, \beta, \gamma, \delta)^2 = p
\]

\[
(a \ldots)(\alpha, \beta, \gamma, \delta)(x_1, y_1, z_1, w_1) = q,
\]

then the coordinates \( x_2, y_2, z_2, w_2 \) are determined by the very simple formulae

\[
x_2 = x_1 - \frac{2\alpha}{p} q_1
\]

\[
y_2 = y_1 - \frac{2\beta}{p} q_1
\]

\[
z_2 = z_1 - \frac{2\gamma}{p} q_1
\]

\[
w_2 = w_1 - \frac{2\delta}{p} q_1.
\]

* Strictly speaking, it is the ratios of these quantities, e.g. \( x : w, y : w, z : w \), which are the coordinates, and consequently, even when the point is given, the values \( x, y, z, w \) are essentially indeterminate to a factor près. So that in assuming that a point is given, we should write \( x : y : z : w = a : \beta : \gamma : \delta \); and that when a point is obtained as the result of an analytical process, the conclusion is necessarily of the form just mentioned. But when this is once understood, the language of the text may be properly employed. It may be proper to explain here a notation made use of in the text. Taking for greater simplicity the case of forms of two variables, \((l, m)(x, y)\) means \(lx + my\); \((a, b, c)(x, y)^2\) means \(ax^2 + 2bxy + cy^2\); \((a, b, c)(\xi, \eta)(x, y)\) means \(a\xi x + b(\xi y + \eta x) + c\eta y\). The system of coefficients may frequently be indicated by a single coefficient only: thus in the text \((a \ldots)(x, y, z, w)^2\) stands for the most general quadratic function of four variables.
Mr. A. Cayley on the Homographic Transformation

In fact, these values satisfy identically the equations

\[
\begin{vmatrix}
  x_2 & y_2 & z_2 & w_2 \\
  x_1 & y_1 & z_1 & w_1 \\
  \alpha & \beta & \gamma & \delta
\end{vmatrix} = 0,
\]

i.e., the point \((x_2, y_2, z_2, w_2)\) will be a point in the line joining \((x_1, y_1, z_1, w_1)\) and \((\alpha, \beta, \gamma, \delta)\). Moreover,

\[
(a...) (x_2, y_2, z_2, w_2)^2 = (a...) (x_1, y_1, z_1, w_1)^2
\]

\[- \frac{4q_1}{p} (a...) (\alpha, \beta, \gamma, \delta) (x_1, y_1, z_1, w_1)
\]

\[+ \frac{4q_1^2}{p^2} (a...) (\alpha, \beta, \gamma, \delta)^2
\]

\[= (a...) (x_1, y_1, z_1, w_1)^2 - \frac{4q_1}{p} q_1 + \frac{4q_1^2}{p^2} p,
\]

that is,

\[(a...) (x_2, y_2, z_2, w_2)^2 = (a...) (x_1, y_1, z_1, w_1)^2.
\]

So that \(x_1, y_1, z_1, w_1\) being a point on the surface, \(x_2, y_2, z_2, w_2\) will be so too. The equation just found may be considered as expressing that the linear equations are a transformation of the quadratic form \((a...) (x, y, z, w)^2\) into itself. If in the system of linear equations the coefficients on the right-hand side were arranged square-wise, and the determinant formed by these quantities calculated, it would be found that the value of this determinant is \(-1\). The transformation is on this account said to be improper. If in a system of linear equations for the transformation of the form into itself the determinant (which is necessarily \(+1\) or else \(-1\)) be \(+1\), the transformation is in this case said to be proper.

We have next to investigate the theory of the proper transformations of a quadratic form of four indeterminates into itself. This might be done for the absolutely general form by means of the theory recently established by M. Hermite, but it will be sufficient for the present purpose to consider the system of equations for the transformation of the form \(x^2 + y^2 + z^2 + w^2\) into itself given by me some years since. (Crelle, vol. xxxii. p.119.)*

I proceed to establish (by M. Hermite’s method) the formula for the particular case in question. The thing required is to find

* It is a singular instance of the way in which different theories connect themselves together, that the formula in question were generalizations of Euler’s formula for the rotation of a solid body, and which are formulæ which reappear in the theory of quaternions; the general formulæ cannot be established by any obvious generalization of the theory of quaternions.
of a Surface of the Second Order into itself.

Write

\[ x_2^2 + y_2^2 + z_2^2 + w_2^2 = x_1^2 + y_1^2 + z_1^2 + w_1^2. \]

then putting \( x_2 = 2\xi - x_1 \), &c., the proposed equation will be satisfied if only

\[ \xi^2 + \eta^2 + \zeta^2 + \omega^2 = \xi x_1 + \eta y_1 + \zeta z_1 + \omega w_1, \]

which will obviously be the case if

\[
\begin{align*}
  x_1 &= \xi + \nu \eta - \mu \zeta + a \omega \\
  y_1 &= -\nu \xi + \eta + \lambda \zeta + b \omega \\
  z_1 &= \mu \xi - \lambda \eta + \zeta + c \omega \\
  w_1 &= -a \xi - b \eta - c \zeta + \omega,
\end{align*}
\]

where \( \lambda, \mu, \nu, a, b, c \) are arbitrary.

Write for shortness

\[ a\lambda + b\mu + cv = \phi \]

then we have

\[
\begin{align*}
k \xi &= (1 + \lambda^2 + \beta^2 + \gamma^2) x_1 + (\lambda \mu - \nu - ab - c \phi) y_1 + (v \lambda + \mu - ca + b \phi) z_1 \\
&\quad + (ve - cu - a - \lambda \phi) w_1 \\
k \eta &= (\lambda \mu + \nu - ab + c \phi) x_1 + (1 + \mu^2 + \beta^2 + \gamma^2) y_1 + (\nu \lambda - \lambda - bc + a \phi) z_1 \\
&\quad + (c \lambda - av - b - \mu \phi) w_1 \\
k \zeta &= (v \lambda - \mu - ca - b \phi) x_1 + (\mu \nu + \lambda - bc + a \phi) y_1 + (1 + \nu^2 + \beta^2 + \gamma^2) z_1 \\
&\quad + (a \mu - b \lambda - c - v \phi) w_1 \\
k \omega &= (b \mu - c \mu + a + \lambda \phi) x_1 + (c \lambda - av + b + \mu \phi) y_1 + (a \mu - b v + c + v \phi) z_1 \\
&\quad + (1 + \lambda^2 + \mu^2 + \nu^2) w_1;
\end{align*}
\]

and from these we obtain at once

\[
\begin{align*}
k x_2 &= (1 + \lambda^2 + \beta^2 + \gamma^2 - \mu^2 - \nu^2 - \beta^2 - \phi^2) x_1 + 2(\lambda \mu - \nu - ab - c \phi) y_1 \\
&\quad + 2(\nu \lambda - \mu - ca + b \phi) z_1 + 2(ve - cu - a - \lambda \phi) w_1 \\
k y_2 &= 2(\lambda \mu + \nu - ab + c \phi) x_1 + (1 + \mu^2 + \beta^2 + \gamma^2 - \nu^2 - \lambda^2 - \beta^2 - \phi^2) y_1 \\
&\quad + 2(\nu \lambda - \lambda - bc + a \phi) z_1 + 2(c \lambda - av - b - \mu \phi) w_1 \\
k z_2 &= 2(\nu \lambda - \mu - ca - b \phi) x_1 + 2(\mu \nu + \lambda - bc + a \phi) y_1 \\
&\quad + (1 + \nu^2 + \beta^2 - \lambda^2 - \mu^2 - \phi^2) z_1 + 2(a \mu - b \lambda - c - v \phi) w_1 \\
k w_2 &= 2(b \mu - c \mu + a + \lambda \phi) x_1 + 2(c \lambda - av + b + \mu \phi) y_1 \\
&\quad + 2(a \mu - b v + c + v \phi) z_1 + (1 + \lambda^2 + \mu^2 + \nu^2 - \beta^2 - \gamma^2 - \phi^2) w_1,
\end{align*}
\]

values which satisfy identically

\[ x_2^2 + y_2^2 + z_2^2 + w_2^2 = x_1^2 + y_1^2 + z_1^2 + w_1^2. \]
Dividing the linear equations by \( k \), and forming with the coefficients on the right-hand side of the equation so obtained a determinant, the value of this determinant is \( +1 \); the transformation is consequently a proper one. And conversely, what is very important, every proper transformation may be exhibited under the preceding form*.

Next considering the equations connecting \( x, y, z, w \) with \( \xi, \eta, \zeta, \omega \), we see that
\[
x_1^2 + y_1^2 + z_1^2 + w_1^2 = (\xi + \nu \eta - \mu \zeta + a \omega)^2 \\
+ (-\nu \xi + \eta + \lambda \zeta + b \omega)^2 \\
+ (\mu \xi - \lambda \eta + \zeta + c \omega)^2 \\
+ (-a \xi - b \eta - c \zeta + \omega)^2.
\]

We are thus led to the discussion (in connexion with the question of the transformation into itself of the form \( x^2 + y^2 + z^2 + w^2 \)) of the new form
\[
(x + vy - \mu z + aw)^2 \\
+ (-vx + y + \lambda z + bw)^2 \\
+ (\mu x - \lambda y + z + cw)^2 \\
+ (-ax - by - cz + w)^2;
\]
or, as it may also be written,
\[
(x^2 + y^2 + z^2 + w^2) + (vy - \mu z + aw)^2 + (\lambda x - vx + bw)^2 \\
+ (\mu x - \lambda y + cw)^2 + (ax + by + cz)^2.
\]

Represent for a moment the forms in question by \( U, V \), and consider the surfaces \( U = 0, V = 0 \). If we form from this the surface \( V + q U = 0 \), and consider the discriminant of the function on the left-hand side, then putting for shortness
\[
\kappa = \lambda^2 + \mu^2 + \nu^2 + a^2 + b^2 + c^2,
\]
this discriminant is
\[
(q + 1^2 + \kappa q + \frac{1}{\kappa} + \phi_1)^2,
\]
which shows that the surfaces intersect in four lines. Suppose

* The nature of the reasoning by which this is to be established may be seen by considering the analogous relation for two variables. Suppose that \( x_1, y_1 \) are linear functions of \( x \) and \( y \) such that \( x_1^2 + y_1^2 = x^2 + y^2 \); then if \( 2\xi = x + x_1 \), \( 2\eta = y + y_1 \), \( \xi, \eta \) will be linear functions of \( x, y \) such that \( \xi^2 + \eta^2 = \xi x + \eta y \), or \( \xi(\xi - x) + \eta(\eta - y) = 0 \); \( \xi - x \) must be divisible either by \( \eta \) or else by \( \eta - y \). On the former supposition, calling the quotient \( \nu \), we have \( x = \xi - \nu \eta \), and thence \( y = \nu \xi + \eta \), which leads to a transformation such as is considered in the text, and is a proper transformation; the latter supposition leads to an improper transformation. The given transformation, assumed to be proper, exists and cannot be obtained from the second supposition, it must therefore be obtainable from the first supposition, i.e. it is a transformation which may be exhibited under a form such as is considered in the text.
the discriminant vanishes; we have for the determination of \( q \) a quadratic equation, which may be written
\[
q^2 + (2 + \kappa)q + K = 0.
\]

Let the roots of this equation be \( q_\mu, q_\nu \); then each of the functions \( q_\mu U + V, q_\nu U + V \) will break up into linear factors, and we may write
\[
q_\mu U + V = R_\mu S_\mu,
\]
\[
q_\nu U + V = R_\nu S_\nu.
\]

U and V are of course linear functions of \( R_\mu S_\mu \) and \( R_\nu S_\nu \), the form which puts in evidence the fact of the two surfaces intersecting in four lines.

The equations
\[
x_1 + x_2 = 2\xi, \quad y_1 + y_2 = 2\eta, \quad z_1 + z_2 = 2\zeta, \quad w_1 + w_2 = 2\omega,
\]
show that the point \((\xi, \eta, \zeta, \omega)\) lies in the line joining the points \((x_1, y_1, z_1, w_1)\) and \((x_2, y_2, z_2, w_2)\); and to show that this line touches the surface \( V = 0 \), it is only necessary to form the equation of the tangent plane at the point \((\xi, \eta, \zeta, \omega)\) of the surface in question; this is
\[
(x + vy - \mu z + aw)(\xi + \nu \eta - \mu \zeta + a \omega) + \ldots = 0;
\]
or what is the same thing,
\[
(x + vy - \mu z + aw)x_1 + \ldots = 0,
\]
which is satisfied by writing \( x_1, y_1, z_1, w_1 \) for \((x, y, z, w)\), i.e. the tangent plane of the surface contains the point \((x_1, y_1, z_1, w_1)\).

We see, therefore, that the line through \((x_1, y_1, z_1, w_1)\) and \((x_2, y_2, z_2, w_2)\) touches the surface \( V = 0 \) at the point \((\xi, \eta, \zeta, \omega)\).

Write now
\[
d' = \frac{-\lambda}{\phi}, \quad b' = \frac{-\mu}{\phi}, \quad c' = \frac{-\nu}{\phi}, \quad \lambda' = \frac{-a}{\phi}, \quad \mu' = \frac{-b}{\phi}, \quad \nu' = \frac{-c}{\phi}.
\]

If we derive from the coordinates \( x_1, y_1, z_1, w_1 \), by means of the coefficients \( a', b', c', \lambda', \mu', \nu' \), new coordinates in the same way as \( x_2, y_2, z_2, w_2 \) were derived by means of the coefficients \( a, b, c, \lambda, \mu, \nu \), the coordinates so obtained are \(-x_2, -y_2, -z_2, -w_2\), i.e. we obtain the very same point \((x_2, y_2, z_2, w_2)\) by means of the coefficients \( a', b', c', \lambda', \mu', \nu' \), and by means of the coefficients \( a, b, c, \lambda, \mu, \nu \). Call \( \xi', \eta', \zeta', \omega' \) what \( \xi, \eta, \zeta, \omega \) become when the second system of coefficients is substituted for the first; the point \( \xi', \eta', \zeta', \omega' \) will be a point on the surface \( V' = 0 \), where
\[
V' = \phi(x^2 + y^2 + z^2 + w^2) + (-cy + bz - \lambda w)^2 + (-ax + cx - \mu w)^2 + (-bx + ay - \nu w)^2 + (-\lambda x - \mu y - \nu z)^2.
\]

And since
\[
V + V' = \kappa(x^2 + y^2 + z^2 + w^2),
\]
\( Z_2 \)
and \( V = 0 \) intersects the surface \( x^2 + y^2 + z^2 + w^2 = 0 \) in four lines, the surface \( V' = 0 \) will also intersect this surface in the same four lines. And it is, moreover, clear that the line joining the points \((x_1, y_1, z_1, w_1)\) and \((x_2, y_2, z_2, w_2)\) touches the surface \( V' = 0 \) in the point \((\xi', \eta', \zeta', \omega')\). We thus arrive at the theorem, that when two points of a surface of the second order are so connected that the coordinates of the one point are linear functions of the coordinates of the other point, and the transformation is a proper one, the line joining the two points touches two surfaces of the second order, each of them intersecting the given surface of the second order in the same four lines. Any two points so connected may be said to be corresponding points, or simply a pair. Suppose the four lines and also a single pair is given, it is not for the determination of the other pairs necessary to resort to the two auxiliary surfaces of the second order; it is only necessary to consider each point of the surface as determined by the two generating lines which pass through it; then considering first one point of the given pair, and the point the corresponding point to which has to be determined, take through each of these points a generating line, and take also two generating lines out of the given system of four lines, the four generating lines in question being all of them of the same series, these four generating lines intersecting either of the other two generating lines of the given system of four lines in four points. Imagine the same thing done with the other point of the given point and the required point, we should have another system of four points (two of them of course identical with two of the points of the first-mentioned system of four points); these two systems must have their anharmonic ratios the same, a condition which enables the determination of the generating line in question through the required point: the other generating line through the required point is of course determined in the same manner, and thus the required point (i.e., the point corresponding to any point of the surface taken at pleasure) is determined by means of the two generating lines through such required point.

It is of course to be understood that the points of each pair belong to two distinct systems, and that the point belonging to the one system is not to be confounded or interchanged with the point belonging to the other system. Consider, now, a point of the surface, and the line joining such point with its corresponding point, but let the corresponding point itself be altogether dropped out of view. There are two directions in which we may pass along the surface to a consecutive point, in such manner that the line belonging to the point in question may be intersected by the line belonging to the consecutive point. We have thus upon the surface two series of curves, such that a curve of each
series passes through a point chosen at pleasure of the surface. The lines belonging to the curves of the one series generate a series of developables, the edges of regression of which lie on one of the surfaces intersecting the surface of the second order in the four given lines; the lines belonging to the curves of the other series generate a series of developables, the edges of regression of which lie on the other of the surfaces intersecting the surface in the four given lines; the general nature of the system may be understood by considering the system of normals of a surface of the second order. Consider, now, the surface of the second order as given, and also the two surfaces of the second order intersecting it in the same four lines; from any point of the surface we may draw to the auxiliary surfaces four different tangents; but selecting any one of these, and considering the other point in which it intersects the surface as the point corresponding to the first-mentioned point, we may, as above, construct the entire system of corresponding points, and then the line joining any two corresponding points will be a tangent to the two auxiliary surfaces; the system of tangents so obtained may be called a system of congruent tangents. Now if we take upon the surface three points such that the first and second are corresponding points, and that the second and third are corresponding points, then it is obvious that the third and first are corresponding points;—observe that the two auxiliary surfaces for expressing the correspondence between the first and second point, those for the second and third point, and those for the third and first point, meet the surface, the two auxiliary surfaces of each pair in the same four lines, but that these systems of four lines are different for the different pairs of auxiliary surfaces. The same thing of course applies to any number of corresponding points. We have thus, finally, the theorem, if there be a polygon of \( m + 1 \) sides inscribed in a surface of the second order, and the first side of the polygon constantly touches two surfaces of the second order, each of them intersecting the surface of the second order in the same four lines (and the side belong always to the same system of congruent tangents), and if the same property exists with respect to the second, third, &c. . . and \( m \)th side of the polygon, then will the same property exist with respect to the \((m+1)\)th side of the polygon.

We may add, that, instead of satisfying the conditions of the theorem, any two consecutive sides of the polygon, or the sides forming any number of pairs of consecutive sides, may pass each through a fixed point. This is of course only a particular case of the improper transformation of a surface of a second order into itself, a question which is not discussed in the present paper.
L. On the Condensation of Gases by Smooth Surfaces.
By Professor Magnus*.

In a discourse given in 1845, on the occasion of my becoming ordinary professor, I communicated the results of certain experiments, which, although not without interest, I thought it well to defer publishing until time should enable me to render them more complete. I find, however, in the Comptes Rendus of the 6th of June of the present year, a note by MM. Jamin and Bertrand wherein a similar subject is treated in a different manner. This induces me to lay the results of my experiments, although they are still incomplete, before the Academy; partly because they furnish a corroboration of the observations of MM. Jamin and Bertrand, but more particularly because the method I have adopted permits not only of establishing the fact of condensation, but also to measure the amount of it as referred to the unit of surface.

After having found that different gases expand differently, and M. Regnault, retracting his first communication, having obtained values almost identical with mine, no doubt could exist as to the accuracy of the different coefficients of expansion. It appeared, however, of interest to examine whether the gases were condensed upon the interior surfaces of the glass vessels made use of, and whether such a condensation exerted any influence on the determination of the coefficient of expansion.

The coefficient was therefore determined by first bringing the gas into contact with a certain surface, and afterwards allowing a larger surface in comparison to its volume to act upon it. In the first case a glass tube was made use of, possessing a diameter of 20 millimetres and a length of 250 millimetres; in the second case a tube of the same dimensions, but within which 250 glass rods were placed, of the same length as the tube and of 1 millimetre diameter. The surfaces of the glass in the two tubes were therefore nearly in the ratio of 1 : 13.5; the volume of air in the latter being less than that in the former by the space taken up by the glass rods; so that, referred to the quantities of gas, the surfaces were nearly in the ratio of 1 : 36.

The determination was effected in the same manner as in the case of the expansion of gases†. Assuming that the condensation would be greatest in the case of those gases which were nearest to their point of condensation, the first gas operated on was sulphurous acid. In the determination of such small values as here come under consideration, the coefficient of expansion

* From the Monats Bericht der Akademie der Wissenschaften zu Berlin.
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already found for sulphurous acid could not be made the basis of comparison. We must be sure that the gas made use of was perfectly pure. Hence the coefficient of expansion was always determined with the two surfaces simultaneously, making use of two such instruments as that described in the memoir above alluded to*. The tubes for both were filled at the same time, by conducting the gas first through one and afterwards through the other.

The calculation of the coefficients of expansion is carried out exactly as in the above memoir; and as the glass is of the same kind as that formerly used, the same coefficient of expansion for the glass is taken.

The calculation gave for the coefficient of expansion of the sulphurous acid from 0° to 100° C.

In the tube.

<table>
<thead>
<tr>
<th>With the rods.</th>
<th>Without the rods.</th>
</tr>
</thead>
<tbody>
<tr>
<td>0·3896</td>
<td>0·3822</td>
</tr>
</tbody>
</table>

These numbers prove that a condensation has taken place upon the surface of the glass. In order to calculate the magnitude of the condensation, let \( \frac{1}{n} \) denote the volume of gas at 0° condensed on the surface of the rods, and let the volume of the uncondensed gas, or at least condensed only on the sides of the large tube, be \( =1 \), then we have

\[
\left( 1 + \frac{1}{n} \right) 1 \cdot 3822 = 1 \cdot 3896,
\]

from which we obtain

\[
\frac{1}{n} = 0 \cdot 00535.
\]

As, however, the diameter of the tube amounted to 20 millimetres and its length to 250 millimetres, the tube which contained no rods had a content of 78525 cubic millimetres. Further, as each rod had a diameter of 1 millimetre and a length of 250, that is, a volume of 196·31 cubic millimetres, the volume of the 250 rods amounted to 49078 cubic millimetres. Hence the volume of the air contained in the tube with the rods was

\[
78525 - 49078 = 29447 \text{ cubic millimetres.}
\]

The gas condensed on the surface of the glass rods was therefore

\[
0 \cdot 00535 \cdot 29447 = 157 \cdot 5 \text{ cubic millimetres.}
\]

The surface of the rods amounted to 196704 square milli-

metres, hence the condensation for every such square is

\[
\frac{157.5}{196704} = 0.000800.
\]

For the unit of the smooth surface of glass, the condensation of sulphurous acid at 0° is therefore = 0.0008 of the unit of volume. This calculation is based on the assumption that the condensation at 100° is a vanishing quantity. If at this temperature a condensation still exists, which might be ascertained by comparing the expansion of the gas in the two tubes at high temperatures, then the condensation at 0° would amount to more than 0.0008 of the unit of volume.

It remains to determine the condensation of other gases in a similar manner; this is the more necessary, as the experiments of MM. Jamin and Bertrand, so far as they have been made known, have been confined to the condensation effected by bodies in powder.

Having found the amount of the condensation upon smooth surfaces, it appeared of interest to ascertain whether it is much greater upon rough surfaces.

Determinations of the coefficients of expansion were therefore undertaken where spongy platinum was made use of instead of glass. To enable us to judge of the purity of the gas in these experiments also, a tube was filled which contained no platinum, while another contained 7 grammes of the substance. Both tubes had nearly the same volume as the former ones, and were filled at the same time. In order to be certain that no sulphuric acid was formed by the presence of the platinum and atmospheric air, the tubes were first filled with hydrogen, the spongy platinum being heated to redness by a lamp. The air and water being thus completely expelled, the sulphurous acid was conducted through both tubes, until on its escape it was completely absorbed by caustic potash; the tubes were then cooled, hermetically sealed, and fixed in the two pieces of apparatus above mentioned.

The determination of the coefficient of expansion gave the following result:

\[
\begin{align*}
\text{In the tube.} \\
\text{Without spongy platinum.} & \quad \text{With spongy platinum.} \\
0.3822 & \quad 0.3922 \\
\end{align*}
\]

from which we obtain

\[
\frac{1}{n} = 0.0065.
\]

As the content of the tubes was the same as the tubes with the
glass rods, namely 78525 cubic millimetres, the quantity of gas absorbed by the spongy platinum was 510.4 cubic millimetres.

The surface of the spongy platinum cannot be determined, and hence we are unable to state the condensation due to the unit of surface. The experiments show simply that in 7 grammes of spongy platinum a greater condensation takes place than on the surface of the glass rods, which surface amounted altogether to 196704 square millimetres.

According as the spongy platinum is more or less pressed or shaken together, it takes up a smaller or a greater space. From several weighings of the substance, it was found that 4 grammes of it occupied a space of 1 cubic centimetre; and as in the present instance 7 grammes of spongy platinum absorbed 0.510 cubic centimetres, we find that spongy platinum condenses 0.29, or nearly one-third of its volume of sulphurous acid at 0°.

That a body so porous as spongy platinum should absorb so much less than charcoal, which, according to De Saussure's experiments, takes up sixty-five times its volume of sulphurous acid, is very remarkable, the more so when we consider that, by the so-called law of Henry, according to which the condensation of a gas is proportional to the pressure under which the condensation takes place, we are compelled to assume that the different condensation of the same gas by different substances depends only on the different extent of the surfaces of contact. It is, however, difficult to conceive that the surfaces of equal volumes of charcoal and spongy platinum are so different as to cause such a divergence as the above in the condensation of sulphurous acid. Platinum in the spongy form is not, however, in the state of finest division, and it would therefore have been desirable to determine the condensation produced by platinum black. This substance, however, cannot be rendered sufficiently free from the vapour of water without destroying it. That the number given by De Saussure for charcoal is not too large, I have convinced myself through experiments with finely powdered beechwood-charcoal.

Supported by certain experiments which I carried out some time ago, I believe that I am in a position to assert that the quantity of carbonic acid gas absorbed by water is not proportional to the pressure, and that therefore the law of Henry is not strictly correct. From this it follows that the absorption, at least to some extent, is due to an attraction between the particles of the attracting solid or fluid body and those of the gas; and, moreover, to an attraction analogous to the chemical, being different for different substances. This proposition is not reconcileable with the views of Dalton regarding absorption; I hope, however, after a little time to be able to furnish the complete proof of the proposition.
LI. On some Theorems in Hydrodynamics.

By Professor Challis, M.A., F.R.S., F.R.A.S.*

The questions I now propose to consider are nearly identical with those discussed in my communication to the Philosophical Magazine for February 1853 under the head of Propositions XII. and XIII. As those propositions appear to me absolutely necessary for completing the mathematical theory of hydrodynamics, I have thought it right to confirm and elucidate the reasoning by which they are established by additional arguments. The course of reasoning here followed will be found to be more explicit and satisfactory than that referred to, while it leads to the same conclusions.

It will be necessary for the present purpose to state briefly the theorems that were demonstrated previous to the consideration of Propositions XII. and XIII. Assuming as an axiom that the lines of motion in each elementary portion of a mass of fluid in motion are normals to a continuous surface, the equality

\[ d\psi = \frac{u}{\lambda} \, dx + \frac{v}{\lambda} \, dy + \frac{w}{\lambda} \, dz \]

is true; or, in other words, the function \( udx + vdy + wdz \) is integrable by a factor, without reference to any particular case of motion. Now that condition of integrability is fulfilled in a general manner if \( \lambda \) be a function of \( \psi \) and \( t \). After obtaining (Prop. VI.), on the principle that the above axiom holds good of any given element in successive instants, the general equation

\[ \frac{d\psi}{dt} + \lambda \left( \frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0, \]

it was shown (Prop. VII.) that if \( \lambda \) be a function of \( \psi \) and \( t \), that equation conducts to rectilinear motion. Next (Prop. VIII.) a general equation was obtained on the two principles, that the lines of motion in each element are normals to a continuous surface, and that the mass of the element remains the same in successive instants. If \( R, R' \) be the radii of curvature of the surface of displacement, and \( V \) be the total velocity and \( \rho \) the density at the time \( t \), that equation is

\[ \frac{d\rho}{dt} + \frac{d \cdot V \rho}{dR} + \rho V \left( \frac{1}{R} + \frac{1}{R'} \right) = 0. \]

For an incompressible fluid, \( \rho \) being constant, the equation becomes

\[ \frac{dV}{dR} + V \left( \frac{1}{R} + \frac{1}{R'} \right) = 0. \]

* Communicated by the Author.
Now introducing the condition that the motion is rectilinear, obtained, as above stated, in a general manner, \( dR \) will be the increment of space, *dato tempore*, along a line of motion. Also \( dR = dR' \) from the geometrical relation between the principal radii of curvature. Hence integrating the above equation along the line of motion, we have

\[
V = \frac{\phi(t)}{RR'}.
\]

As this equation has been arrived at without supposing any case of motion, it expresses a general law of the motion of an incompressible fluid. I proceed now to an analogous investigation for a compressible fluid.

It will be required, first, to establish a general theorem respecting the relation between velocity and density in a state of propagation. In accordance with the condition that the lines of motion are normals to a continuous surface, let us suppose the fluid to be bounded by plane faces passing through focal lines, so as to be contained in a slender tube, whose length is very small and transverse section quadrilateral. Let \( P, Q, R \) be three positions on the axis of the tube, separated by equal intervals. Then it will be required to solve the following general problem relating to propagation: viz. to express the rate at which the excess of fluid in the space between \( Q \) and \( R \) above that which would exist in the same space in the quiescent state of the fluid, becomes the same as the excess in the space between \( P \) and \( Q \).

Let \( V, \rho \) be the mean velocity and density of the fluid which in the small time \( \delta t \) passes the section at \( Q \), and \( V', \rho' \) the same quantities relative to the section at \( R \). Let the magnitude of the section at \( Q \) be \( m \), and of that at \( R \) be \( m' \), and the interval between them be \( \delta z \). Then the increment of matter in the time \( \delta t \) in the space between \( Q \) and \( R \) is

\[
V \rho m \delta t - V' \rho' m' \delta t.
\]

Let this be equal to the excess of the matter in the space between \( P \) and \( Q \) due to the state of motion, above that in the space between \( Q \) and \( R \), at the commencement of the small interval \( \delta t \). The expression for this excess, neglecting small quantities of the second order, is

\[
(\rho - 1)m \delta z - (\rho' - 1)m' \delta z.
\]

Hence we have the equation

\[
\frac{d}{dz} V \rho m = \frac{d}{dz} (\rho - 1)m \frac{\delta z}{\delta t},
\]

which gives the required expression for the rate \( \frac{\delta z}{\delta t} \).
If the rate of propagation be supposed equal to a constant \( a' \), we obtain by integration,

\[
V \rho = a'(\rho - 1) + \frac{\phi(t)}{m}.
\]

Since \( m \) varies as the product \( RR' \), this equation may be written

\[
V \rho = a'(\rho - 1) + \frac{\phi(t)}{RR'}.
\]

We have thus arrived at a general relation between \( V \) and \( \rho \) on the hypothesis of uniform propagation of the kind above enunciated. It will be seen that if \( \rho = 1 \), the expression for the velocity coincides with that obtained for an incompressible fluid, in which no propagation can take place. Propagation implies a change of density.

We are now prepared to obtain the general equations which are the object of this research. By differentiating the last equation,

\[
\frac{d}{dR} \frac{dV \rho}{dR} = a' \frac{d\rho}{dR} - \frac{\phi(t)}{RR'} \left( \frac{1}{R} + \frac{1}{R'} \right)
\]

Hence by substitution in the general equation of Prop. VIII. given above, we have

\[
\frac{d\rho}{dt} + a' \frac{d\rho}{dR} + a'(\rho - 1) \left( \frac{1}{R} + \frac{1}{R'} \right) = 0.
\]

Introducing now the condition that the motion is rectilinear, this condition having been obtained in a general manner, and taking \( z \) any abscissa along the line of motion, we shall have \( dz = dR = dR' \). The above equation now admits of being exactly integrated, giving

\[
\rho - 1 = \frac{F(z - a't)}{RR'}.
\]

Hence

\[
V \rho = \frac{a' F(z - a't)}{RR'} + \frac{\phi(t)}{RR'}.
\]

As these equations have been arrived at anterior to any supposed case of motion, it may be concluded that they are expressive of laws of the mutual action of the parts of a compressible fluid. They depend, it is true, on the supposition that the rate of propagation \( \frac{dz}{dt} \) is a constant quantity. But this supposition is justified by leading through exact integration to definite ex-
pressions for the velocity and density. It will also appear from considerations which follow, that no other general supposition respecting propagation is admissible.

If \( \rho = 1 + \sigma \), and the product \( V \sigma \) be neglected, the equations become

\[
\sigma = \frac{F(z - d't)}{R\eta'}, \quad V = d'\sigma + \frac{\phi(t)}{R\eta'}.
\]

Having thus far obtained results confirmatory of those of Proposition XII., I proceed next to treat in a new manner Proposition XIII., which relates to the motion and velocity of propagation in a rectilinear tube of arbitrary and indefinitely small transverse section.

The investigation of the laws of the mutual action of the parts of a compressible fluid in Proposition X. (Philosophical Magazine for December 1852), led by a general process to the conclusion that the motion takes place along a rectilinear axis, and that the rate of propagation of the velocity and density along the axis is constant. On the suppositions that the propagated motion is small, and that no impressed force acts, particular functions expressing the velocity of the fluid parallel and transverse to the axis and the density of the fluid were obtained. Also (Prop. XI.) a numerical value of the rate of propagation in air was found without reference to any arbitrary disturbance. The above-mentioned functions appear to apply to cases in which the motion results from the mutual action of the parts of the fluid, such, for instance, as might be caused by the confluence of two streams, or by the passage of a uniform stream over the mouth of a tube. In my communication to the Philosophical Magazine for last February, I have called the motions defined by these functions, normal motions. The term was not happily chosen, and being liable to be misunderstood, I propose to substitute for it free motions. The course of the inquiry now leads us to deduce from the laws of free motion, equations applicable to motion which is constrained to take place in straight slender tubes. For the sake of simplicity, the reasoning will be confined to small quantities of the first order of approximation with respect to the velocity and condensation.

The motions parallel and transverse to an axis of free motion are defined generally by the two equations,

\[
\phi = \mu \cos \frac{2\pi}{\lambda} (z - \kappa t + c),
\]

\[
f = 1 - e(x^2 + y^2),
\]

in the immediate neighbourhood of the axis. Hence, as the
longitudinal velocity is $\frac{d\phi}{dz}$, and the transverse velocities are $\phi \frac{df}{dx}$ and $\phi \frac{df}{dy}$, it follows that the lines of motion drawn at any instant in the directions of the motions of the particles through which they pass, are continually varying their positions. This variability is due to the transverse velocities, which, as well as the transverse accelerative forces, are indefinitely small very near the axis. Hence by impressing other indefinitely small transverse forces, we may alter ad libitum the positions of the lines of motion, and even give them fixed positions in space. This, in fact, is done by constraining the fluid to move in rigid tubes of small and arbitrary transverse section, the boundary of the tube being supposed at all points to be inclined at indefinitely small angles to the axis of $z$. In case the motion be not symmetrical about an axis, the value of $f$ to be employed is $f = \cos (gx + hy)$, $g^2 + h^2$ being equal to $4\varepsilon$, and $x$ and $y$ being taken very small. Now these impressed forces, being indefinitely small, do not sensibly alter the total velocity $V$; and being transverse, they do not change the rate of propagation. But being impressed in all directions about the axis, they may sensibly alter the relation of the velocity to the condensation. Conceiving, therefore, the fluid to be constrained to move in a straight prismatic tube, we shall have, as in free motion,

$$V = m \sin \frac{2\pi}{\lambda} (z - \kappa at + c).$$

The relation between the velocity and the condensation is to be deduced from the general theorem respecting propagated motion in straight slender tubes, obtained in the previous part of this paper, according to which

$$V = d\sigma + \frac{\phi(t)}{RR}.$$

In the present case $\phi(t) = 0$, $a' = \kappa a$, and consequently $V = \kappa a \sigma$. Thus the motion in the prismatic tube is defined by the equations

$$V = \kappa a \sigma = m \sin \frac{2\pi}{\lambda} (z - \kappa at + c).$$

It is further to be remarked, that the value of $V$ is not restricted to the above form. For by the principle that the parts of the fluid may be separated by a thin partition transverse to the tube without assignable force, the velocity and condensation on the two sides of the partition being the same, we may suppose the quantities $\mu$, $\lambda$, and $c$ to vary in an arbitrary manner from element to element along the line of motion. Consequently we
have generally for propagated motion in a prismatic tube,

\[ V = \kappa a \sigma = \psi(z - \kappa t + c). \]

By changing the sign of \( \kappa a \), these equations would apply to motion propagated in the contrary direction: and if we now suppose \( V \) and \( \sigma \) to represent the velocity and condensation resulting from two coexistent and opposite propagations, we shall have

\[ V = \psi(z - \kappa t + c) + \chi(z + \kappa t + c'), \]

\[ \kappa a \sigma = \psi(z - \kappa t + c) - \chi(z + \kappa t + c'). \]

From these equations we readily obtain by differentiation,

\[ \kappa^2 a^2 \frac{d\sigma}{dz} + \frac{dV}{dt} = 0, \]

by which it appears that the accelerative force of the fluid in a prismatic tube is to that along an axis of free motion in the ratio of \( \kappa^2 \) to 1, the numerical value of \( \kappa \) being 1.18545.

From the above result we may proceed to the consideration of motion in a straight tube of arbitrary transverse section, subject, however, to the condition that the boundaries of the tube are inclined at indefinitely small angles to the axis of \( z \). To take account of the effect of the rigidity of the tube on the accelerative force of the fluid, we may consider the transverse section constant for a very small space, and the equation last obtained may thus be immediately applied; and to take account of the variation of transverse section, the usual equation of constancy of mass for small motions is required. Thus the equations generally applicable to constrained motion in straight tubes are

\[ \kappa^2 a^2 \frac{d\sigma}{dz} + \frac{dV}{dt} = 0 \]

and

\[ \frac{d\sigma}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0. \]

In the February Number of the Philosophical Magazine I have applied these two equations to the case of motion in straight lines drawn from a centre, the velocity being assumed to be a function of the distance from the centre, this case being evidently reducible to motion in a straight slender tube. I beg to invite the attention of mathematicians to the reasoning there employed, no intelligible solution of this important problem having hitherto been given on the commonly received principles of hydrodynamics.

The main object I have had in view in prosecuting these hydrodynamical researches, has been to lay a secure foundation for the undulatory theory of light. The theory which attributes the phenomena of light to the oscillations of the individual atoms
of the æther, can with no propriety be called undulatory, the word \textit{unda} implying the motion of a \textit{mass}. It is desirable that two theories, perfectly distinct in their principles and wholly incompatible with each other, should also be distinguished in name, and I would therefore call the latter the \textit{oscillatory} theory of light, this term having been recently made use of by an advocate of that theory. I venture to add here one consideration, tending to show that the establishment of the theory of light on hydrodynamical principles may have an extensive bearing on the progress of theoretical physics. Light, considered apart from the sensation so called, is simply a \textit{force}, which may be placed in the same rank as heat, electricity, magnetism, \&c. Let it be proved that this force is due to the transverse vibrations of a continuous elastic medium, and the inference can hardly be resisted that heat is due to the direct vibrations of the same medium. Thus these two forces would be resolved into \textit{pressures}, obeying laws which may be investigated on mathematical principles, and it might reasonably be presumed that other forces admit of like explanation. Whatever may be the truth of this induction, I am prepared to maintain that many phenomena of light may be explained by the motions and pressures of a continuous elastic medium, and to this object I propose, as I find leisure, to apply some of the hydrodynamical theorems which have been discussed in various communications to this Journal.

Cambridge Observatory,
October 6, 1853.

LII. \textit{On an Instrument for taking Soundings.}

\textit{By F. Maxwell Lyte, Esq.}

\textit{To the Editors of the Philosophical Magazine and Journal.}

\textsc{Gentlemen,}

As I see, from what Dr. Scoresby has been saying before the Association at Hull, there seems to be some difficulty about obtaining a correct sounding in places where the currents are strong and flow in different directions at the different points of depth, causing the line to assume different curves in its descent; and when it comes to be measured over, after the weight has reached the bottom and been hauled up again, the measurement gives no approximate idea of the real depth,—now it is plain that this mensuration of the depth of water might be as well made by estimating its vertical pressure, as, in measuring the height of mountains, we measure the barometrical pressure of the air; and so I would propose to do it by an instrument constructed as follows:—
An accurately constructed tube of gunmetal or brass, or some metal not very easily corrodisible by salt water, has a glass tube fitted on to it on the top by a screw joint, and again on the top of the glass tube is fitted a strong hollow copper ball by a similar screw joint. The lower tube, which we will call $a$, has a well-turned piston fitted to it, from which runs a rod which is only a trifle longer than the tube $a$, and just enters the tube $b$ when the piston is at its lowest point. A well-made spring is placed in the tube $a$ above the piston, and the tube $a$ being narrowed at the top, so as just to admit the free passage of the rod, and the rod having a little button at its top, the piston is kept at its lowest point by the spring, except when sufficient pressure is applied from below to compress the spring. The glass tube has a small ring fixed in it, just so as to stick at any point to which it is pushed, and the button at the top of the rod serves to push the ring straight, and the ring thus forms an index of the degree to which the spring has been compressed. The ball on the top serves as a mere reservoir of air to equalize the action of the apparatus as much as possible. The whole of this apparatus is enclosed in a wire cage for the sake of protection from blows. To graduate this apparatus, I let it down in a known depth of water, say ten fathoms, and having observed the point to which the ring in the glass tube is pushed, and having marked this point off, the ball is to be unscrewed, and with a small ramrod the ring is to be pushed down till it rests on the top of the piston-rod. The ball being replaced, the apparatus is sunk in twenty fathoms; after a similar manner it is sunk in thirty, and next in forty fathoms. This will test the accuracy of the apparatus; and the marks made on the glass tube $b$ after each trial will give a scale from which the whole tube may be graduated, even to thousands of fathoms, if the tube be long enough or the spring strong enough. I have been induced to make this communication on account of the great use which may be made of such an apparatus.

I remain, your very obedient Servant,

F. MAXWELL LYTE.
LIll. On the Manufacture of large available Crystals of Sulphate of Iodo-quinine (Herapathite) for Optical Purposes as Artificial Tourmalines. By William Bird Herapath, M.D., Bristol*.  

HAVING been repeatedly applied to by various parties for the details of my process for the manufacture of these useful crystals, I have been induced to enter into numerous experiments to obtain greater certainty in the results, and to study the conditions necessary for the production of broad foliaceous plates; permit me to make the formula known to science, together with the precautions necessary for adoption to secure the crystals when obtained, and to mount them so as to be available as polarisers or analysers for the microscope, or even to enable us to perform all the experiments in the polariscope.

The success which I have obtained is so great, that there is no doubt tourmalines and Nichol's prisms will be soon completely superseded by these new crystals, since the scarcity of the one and the difficulty in manufacturing the others render them very costly apparatus. But a little practice in the following process will soon enable anyone to make them large enough for every purpose; and so superior are they in power to the best tourmaline, that two plates scarcely thicker than gold-leaf may (by a slight modification of my formerly published method) be rendered totally impervious to light when they are crossed at right angles.

[Herewith are enclosed two marvellously thin plates of considerable size; one being six-tenths of an inch long and three-tenths of an inch broad, the other the same length but one-tenth of an inch broader. Upon crossing them you will perceive that they are optically perfect as polarisers, and as useful as plates of tourmaline for which you would be charged four guineas each plate. I have succeeded in getting much larger ones by the same process and equally good.]

The materials employed are the same as before, the chief modification being in the proportions of the ingredients, and the care taken in the method of crystallization.

It is necessary to procure pure disulphate of quinine, and for this purpose none approaches so thoroughly to the standard of absolute purity as that manufactured by Messrs. Howard and Kent.

I dissolve it in pyroligneous acid having a specific gravity of 1.042, and dilute the solution with an equal quantity of proof-spirit made by adding rectified spirit of wine, spec. grav. 0.837, to equal bulks of distilled water.

* Communicated by the Author.
The spirituous solution of iodine is made by dissolving 40 grains of iodine in 1 fluid ounce of rectified spirit of wine.

I can after these explanations give the formula.

Take of disulphate of quinine 50 grains,

... pyroglycerine acid 2 fluid ounces,
... proof-spirit 2 fluid ounces,
... spirituous solution of iodine 50 drops;

dissolve the disulphate of quinine in the pyroglycerine acid mixed with the spirit; warm the solution to 130° F., and directly add the solution of iodine by drops, agitating the mixture from time to time.

This formula gives to the mother-liquid, after crystallization, at 52° F., a specific gravity of 0·986, which appears highly favourable to the deposition of the majority of the crystalline production, and yet allows only the very broad and thinner plates to float—thus getting them perfectly free from all interfering and adhering plates.

It is necessary to perform this operation in a wide-mouthed Florence flask or matras, and to take care that the temperature is maintained for a little time after the addition of the iodine, so that the solution should become perfectly clear, dark, sherry-wine colour; then set it aside to crystallize under the following conditions:

1st. It is essential that the apartment should be tolerably equable in temperature, about 45° or 50° F., as a slight variation in the temperature produces currents in the crystallizing fluid which destroy the parallelism of the crystals, and of course negative all the efficiency of the manufacture; and a greater rise, if only to 60° F., redissolves the thinner plates.

2nd. It is equally necessary that the liquid should be kept in a perfect state of repose during the whole act of crystallization— even the common vibration of the apartment must be counteracted, for the same important reason as the last.

The best method to adopt is one which my friend Mr. John Thwaites employs, namely, to suspend the flask by the neck with strong twine, and attach this to a similar string stretching across from one wall of the apartment to the other. This certainly gives the most uniform results, and offers other advantages.

The plan I had usually employed was to set aside the flask on the steadiest support to be found, a wall, pillar or table; and imbed it on a feather, cotton, or tow pillow, to act as a non-conductor and at the same time destroy vibration.

3rd. It is also necessary that the surface of the fluid should not be exposed to too rapid evaporation, as the temperature would fall too quickly, and various currents and intestinal mo-
tions would result; therefore the flask or matrass answers better than the evaporating dish.

4th. It is also decidedly an advantage to have a broad surface in proportion to the depth of liquid; the reason being, that the thinner and most easily reached plates *form on the surface* and float there until the time arrives to remove them; and the greater the surface, the more numerous are the plates.

5th. These broad plates are not always formed; but if after six hours none make their appearance, it is merely necessary to apply a spirit-lamp to the bottom of the flask and warm the liquid to dissolve all the deposited crystals, then add a little spirit and a few more drops of iodine solution and again wait for crystallization.

6th. Supposing that we obtain a crop of these broad floating plates, which generally occurs under the aforementioned conditions, we permit them to remain from twelve to twenty-four hours to complete their disc and fill up all crevices, &c., and to attain a sufficient degree of thickness; for if too thin, they do not stop the red or purple-violet rays, as Haidinger has beautifully shown and admirably explained (*vide* Phil. Mag. Oct. 1853, and Poggendorff’s *Annalen* for June last). If the crystals are allowed to remain too long in their mother-liquid, we run the risk of loss and injury; for after some time a dissolving or disintegrating action appears to occur, and considerable disappointment is occasioned. I have lost several batches of beautiful and magnificent plates from inability to secure them at the nick of time.

Having by these means obtained the object of our best wishes, it now remains to secure the prize. This requires a little patience and a tolerable amount of care; the following plan is the most ready, and requires but little practice and a steady hand to ensure success.

The first stage of the process is to procure a table as near as possible to the crystallizing spot, furnished with the following apparatus:—

1. A gallipot or small mortar to hold the flask on as a support.
2. A supply of perfectly clean circular glass discs, small enough to pass down the neck of the flask with ease.
3. A glass rod of sufficient length to descend to the bottom of the flask if necessary.
4. A little marine glue or sealing-wax.
5. A spirit-lamp and matches.
6. A quantity of blotting-paper cut in strips about an inch wide and two inches long, and also a folded sheet of the same to act as a pad or support.

Now remove the flask with the greatest amount of care from
its attachment to the horizontal string; this is best done by holding the perpendicular twine in the left finger and thumb, at the same time cutting the upper end of it with a pair of scissors to avoid all disturbance. It will now swing easily and steadily, and may be carried and gently deposited upon its gallipot support. Then attach the edge of one of the circular glass discs to the end of the glass rod by a little of the wax or marine glue, and let it, when cold, be carried flatly down the neck of the flask, which should be very gently inclined, as nearly horizontally as possible, to admit of this being easily accomplished. Having selected the largest crystalline plate, pass the glass circle gently beneath it, raise the plate by depressing the hand, and the little crystalline gem is at once caught on its surface.

If this operation be neatly accomplished, it appears spread out as a thin uniformly-coloured film upon the glass; if any black patches appear, they are occasioned by the accidental crossing of some interposed crystals, or from some on the under surface of the glass disc. These last must be at once wiped off by the blotting-paper, the others will sometimes float out upon raising the edge of the glass disc to a perpendicular position; if they are near the edge, they may sometimes be gently drawn out from under the large crystal by a little dexterity on the part of the operator; frequently there are no such precautions necessary.

Now rapidly dry the plate by imbibing all the fluid most scrupulously by blotting-paper. This must be done without touching the crystalline surface, for the least contact destroys its beauty, symmetrical arrangement, and optical usefulness. Having done so, let it dry by exposure to the air in a cool room, say at 40° to 50°; this is to prevent resolution and disintegration of the crystal in its own mother-water, a little of which must remain attached after all our care.

It is sometimes necessary to float them on, or dip them for an instant only in a little cold distilled water somewhat imbued with iodine. This serves two purposes; it removes all mother-liquid, and prevents those crystals of sulphate of quinine subsequently forming and interfering with the perfect polarization of the new tourmaline, as every crystal of this substance interposed between the plates would of course rotate the polarized beam as far as its influence extended, and depolarize it. The iodine acts also in preventing the solution of the new crystals in the water. They must again be dried by imbibition and by exposure to air, as before, and then placed under a cupping-glass, having a watch-glass with a few drops of tincture of iodine in it. This gives a decidedly black tone to the field; and if the crystal were before too thin to obstruct all the light, and thus give a red or purplish violet-tint, its power of polarization will be very materially improved by following the above simple directions.
It is essential in iodizing the plate that the exposure to the vapour be not too long continued; the time necessary will of course depend on the temperature of the apartment, about three hours at 50° F. being generally necessary. The reason of this precaution will be at once evident upon making the experiment, for the crystals assume a rich golden yellow colour, both by reflected and transmitted light; the field will therefore, when the two plates are parallel, be intensely yellow, a most objectionable colour for the examination of objects. The crystals have lost the power of stopping the yellow rays, and the complementary relation of the body to the superficial colours appears to be lost also—a very remarkable fact.

The rationale of the periodizing process appears to be the addition of as much iodine to the crystal as will be sufficient to communicate the exact complement of yellow to neutralize the red and blue rays of the purple "body-colour." These rays are now absorbed by the plates as they would be by yellow glass. If too much iodine be added, an intense yellow light becomes transmissible when the crystals are parallel, and the plate becomes rotten and brittle, and will be almost certainly destroyed in mounting, even if it be exposed to the air for some time before attempting to do so, by which process the superadded iodine again volatilizes, clearly showing that no chemical union could have existed.

Having so far prepared the "artificial tourmalines," it merely remains to cover them by another plate of thin microscopic glass, interposing some highly refractive cement or varnish between the two plates.

Several cements offer themselves to our notice, but some selection is necessary. Canada balsam is one of the best; however, in using this it is necessary to have it very fluid, and not to employ much heat in the process; in fact, I believe it best to use it so as to be fluid at the ordinary temperature. I have found, however, that it appears to attack the crystals and dissolve out the iodine; in order to correct this destructive tendency, it is essential to saturate the fluid Canada with iodine at the ordinary temperature. This is best done by warming some small quantity of the balsam in a test-tube or thin bottle and dropping into it some crystals of iodine, agitating them well together by a glass rod; giving time to cool and the excess of iodine to subside, it is fit for use. It is merely necessary to take a small drop of this fluid on the end of a glass rod, place it on the larger and clean glass circular disc, then invert the disc, carrying the crystal, upon it, press the two together gently and steadily with the finger or a glass rod, or piece of stick, taking great care not to use much force, in case the circles or crystal may sustain injury. Now remove all the extra Canada balsam from
around the edge, and expose the little apparatus to the air so that the balsam may become dry; it is then fit to mount in the brasswork of the microscope in the same manner as a tourmaline.

I have found it best to employ an æthereal solution of Canada balsam in this process, made by dissolving the hard old balsam in washed pure sulphuric æther, afterwards adding a little iodine to it as before. This dries more rapidly, hardens quicker and more perfectly than the usual fluid Canada, and it does not attack the crystals—a very great advantage.

These directions may appear very prolix, tedious, and excessively troublesome; but, however, when set in practice, the whole operation resolves itself into the utmost simplicity, habit soon reconciling oneself to the routine, and the different precautions appear to offer themselves unconsciously to us as we proceed. I have frequently prepared a dozen good tourmalines in an hour, as far as the catching and drying part of the operation; the others of course require longer time, but for these we must wait, and occupy ourselves with some other stages of the same process.

There is not the least doubt that before long these splendid and useful crystals will be offered for sale by opticians at as many shillings as tourmalines now cost pounds, and certainly of equal value and practical utility—in my own opinion, of even greater, for less light is lost by these than by any of our polarizing apparatus at present in use.

I have invariably used in this description the original terms employed by me, namely, "artificial tourmalines" and "crystals of sulphate of iodo-quinine." Professor Haidinger's term of "Hera-pathite" is certainly a highly complimentary one to myself; but as it does not give either an idea as to the optical properties or chemical characters of the substance in question, it does not appear to me so suitable as those I originally attached to them.

32 Old Market Street, Bristol,
October 1853.

LIV. Applications of the Calculus of Operations to Algebraical Expansions and Theorems. By Charles James Hargreave, Esq., LL.D., F.R.S.*

In a paper "On the Solution of Linear Differential Equations," written by me in the year 1847, and published in the Philosophical Transactions for 1848, the following formula was enunciated as a leading theorem in the Calculus of Operations:

$$
\phi(D)\{\psi x. u\} = \psi x. \phi(D)u + \psi' x. \phi'(D)u + \frac{1}{2} \psi'' x. \phi''(D)u + \ldots
$$

* Communicated by the Author.
This theorem is not derived from any principles peculiar to the Calculus of Operations; nor does it in substance contain anything not previously known. Its characteristic consists in the use of the general functional form \( \phi(D) \) in lieu of particular algebraical forms, and in expressing the connexion between the different sets of operations employed by means of \( \phi(D) \) and its derived functional forms \( \phi'(D), \phi''(D), \&c. \) In passing from \( \phi \) to \( \phi', \phi'', \&c., \) we cause the functional form \( \phi(D) \) to undergo the same changes in form that the quantity \( \phi t \) does in the course of successive differentiations with regard to \( t, \) an independent variable. In other words, for this purpose, viz. that of obtaining convenient modes of expression, we treat \( D \) as if it were a quantity, and we make it an independent variable; and it is by an operation of differentiation with regard to this supposed variable that we express the connexion between \( D, \phi(D), \&c. \) If we denote this operation by a symbol having the same relation to \( D \) that \( \phi \) has to \( x, \) and apply the established theorems of the Calculus of Operations to the new symbol, we shall acquire enlarged and more convenient forms for the expression of complex operations; and, if necessity or convenience should require it, there appears no reason against extending this idea without limit.

In establishing a notation, it seems desirable that any symbol expressing a derived function should have some affinity with those already adopted; but the calculus of finite differences and the calculus of variations have already appropriated most of the available types. I propose to denote the operation of passing from any function of \( D \) to its derived function by the symbol \( \nabla; \) and, should occasion require it, we may distinguish the successive grades of this operation by \( \nabla_1, \nabla_2, \ldots, \nabla_n; \) each symbol denoting a differentiation in which the next preceding symbol is the independent variable.

At present we confine ourselves to the first symbol; so that we have

\[
\phi'(D) \text{ or } \frac{d(\phi D)}{dD} = \nabla(\phi D);
\]

and

\[
(a_0 + a_1 \nabla + \ldots + a_n \nabla^n) \phi D = a_0 \phi D + a_1 \phi'D + \ldots + a_n \phi^{(n)}D,
\]

\[
e^{\pm n \nabla}(\phi D) = \phi(D \pm n),
\]

\[
\nabla^{-n}(\phi D) = \phi^n D, \text{ or } \text{nth integral of } \phi D,
\]

\[
(\nabla + c)^{-1}(\phi D) = e^{-cD} \int e^{cD} \phi D dD,
\]

or

\[
\frac{1}{c} \phi D - \frac{1}{c^2} \phi' D + \frac{1}{c^3} \phi'' D - \frac{1}{c^4} \phi''' D + \ldots;
\]

and so on for other forms of \( \psi \nabla. \)

It is scarcely necessary to remark that the symbol \( \nabla \) obeys
the algebraical law of indices, and is distributive in its operation; or, in other words, that

\[ \nabla^n \nabla^n \phi D = \nabla^n \nabla^n \phi D = \nabla^{n+n} \phi D, \]

\[ \nabla^n (\phi D + \psi D) = \nabla^n \phi D + \nabla^n \psi D. \]

It is more important to observe, that it is commutative with regard to \( x \) or any function of \( x \). A slight consideration will show that no kind of relation or connexion exists between \( \nabla \) and \( x \), the former being merely a symbol denoting a change of form in the functions of \( D \). If we perform \( \chi x \cdot \phi D \) upon \( u \), a function of \( x \), we mean that certain operations of differentiation are performed upon \( u \), and that the result is multiplied by \( \chi x \); and, since \( \nabla \) operates upon \( D \) only, it is immaterial whether \( \chi x \cdot \phi' D \) be written in the form \( \nabla (\chi x \cdot \phi D) \) or \( \chi x (\nabla \phi D) \), though the latter is the more natural. We may therefore treat \( x \) and \( \nabla \) as constants to each other; but it must be remembered that \( \nabla \) and \( D \) stand in the same relation to each other as \( D \) and \( x \), and are therefore not commutative. Bearing these considerations in mind, it will be seen that the leading theorem above stated may be placed under the condensed symbolical form,

\[ \phi(D) \{ \psi x \cdot u \} = e^{\nabla \psi} (\psi x \cdot \phi(D) u) ; \]

and the circumstance that the series is terminable whenever either \( \phi \) or \( \psi \) contains positive powers only, constitutes no objection to this mode of writing the theorem.

The advantage of this kind of notation in suggesting theorems displays itself immediately; for the preceding theorem at once leads to the following (which I had previously deduced from distinct considerations in the paper above alluded to),

\[ \psi x \cdot \phi(D) u = e^{-\nabla \psi} \phi(D) \{ \psi x \cdot u \} \]

\[ = \phi D \{ \psi x \cdot u \} - \phi' D \{ \psi x \cdot u \} + \frac{1}{2} \phi'' D \{ \psi'' x. u \} - \ldots \]

Returning to the original theorem, and remembering that \( x \) and \( \nabla \) are commutative, we have

\[ \phi D \{ \psi x \cdot u \} = e^{\nabla \psi} (\psi x \cdot \phi(D) u) \]

\[ = \psi (x + \nabla) \cdot \phi(D) u \]

\[ = \psi \nabla (\phi D) u + x \cdot \psi' \nabla (\phi D) u + \frac{x^2}{2} \cdot \psi'' \nabla (\phi D) u + \ldots \]

Looking to the right-hand side of this equation, it will be observed that we have extricated the function \( \psi x \) from all the operations; so that the differential operations are to be performed upon the function \( u \), which thus becomes the sole subject of operation; thus \( \psi x \) is no longer a part of the subject of opera-
tion, but $\psi$ becomes an instrument in determining the form of the operations to be performed on $u$.

If, in the function $\psi x$ (which we regard as a function actually expressed in terms of $x$ and constants), we give $x$ a particular value, say $a$, the effect on the right-hand side of the equation is to produce a series arranged in powers of $a$, the coefficients of which are functions of $x$ determinable by means of known operations performed upon $u$. The left-hand side of the equation in that case assumes a remarkable form; it represents an operation performed upon $u$ of the following nature: multiplication by an expression containing $x$ and $a$, explicitly $\phi(x, a)$; differential operations with regard to $x$; and lastly, the change of $x$ into a specific value $a$ whenever $x$ appears explicitly, $u$ remaining throughout an implicit function of $x$. The right-hand side of the equation is the expression of this result in the form of an expansion in powers of $a$.

If $\{\phi D \cdot \psi(x, a)\}$ denote the above operation, we have

$$\{\phi D \cdot \psi(x, a)\} u = \psi \nabla (\phi D) \cdot u + a \cdot \psi' \nabla (\phi D) \cdot u + \frac{a^2}{2} \cdot \psi'' \nabla (\phi D) \cdot u + \ldots$$

$$= \psi (\nabla + a) (\phi D) \cdot u.$$

Now the function $\psi(\nabla + a)$ in this expression has no other effect than simply to determine the form of the differential operations which are to be performed upon $u$, the function of $x$. If, after having thus determined the form into which $\phi D$ is changed, we consider $x$ as changed into $a$ in the function $u$, and consider $D$ as now meaning differentiation with regard to $a$, it is evident that we thus obtain the value of $\phi D \{\psi x \cdot u\}$ when $x$ is made equal to $a$ throughout the whole expression. We have, then,

$$\{\phi D \cdot \psi(x, a)\} u = \psi (\nabla + a) (\phi D) \cdot u,$$  
$u$ remaining a function of $x$.

$\phi D \{\psi(x, a) \cdot u\}$ (when $x = a$ generally) $= \psi (\nabla + a) \phi D \cdot \chi a$, $u$ being $\chi x$, and $D$ denoting now $\frac{d}{da}$.

As an example, let $\psi(x, a)$ be $(x-a)^n$, $n$ positive. Then we have $\phi D ((x-a)^n \cdot u)$ (when $x = a$) $= \nabla^n \phi D \cdot u = \phi^{(n)} D \cdot u$. This equation is true in two senses. It is true, first, if $u$ continue to be a function of $x$, the change of $x$ into $a$ being supposed to be made only as part of the operation performed upon $u$. It is also true, secondly, if $x$ be made equal to $a$ generally, the $D$ on the second side then having relation to $a$, of which $u$ becomes a function.

These may be thus verified:—By the original theorem we have

$$\phi D ((x-a)^n \cdot u) = (x-a)^n \cdot \phi D \cdot u + n(x-a)^{n-1} \cdot \phi' D \cdot u$$

$$+ \frac{1}{2} n(n-1)(x-a)^{n-2} \cdot \phi'' D \cdot u + \ldots.$$
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and if \( x \) be made equal to \( a \), the second side vanishes except in the term whose index is \( n-n \): so that

\[
\phi D((x-a)^n, u)(\text{when } x=a \text{ in the operation}) = \frac{n(n-1) \cdots 1}{1 \cdot 2 \cdots n} \phi^{(n)} D \cdot u = \phi^{(n)} D \cdot u.
\]

And if \( u=\chi x \), it follows that

\[
\phi D((x-a)^n\chi x)(\text{when } x=a \text{ generally}) = \phi^{(n)} D \cdot \chi a, \ D \text{ being } \frac{d}{da} \ 	ext{here.}
\]

If \( \phi D \) be simply \( D^m \), we have the well-known formula

\[
D^m((x-a)^n\chi x)(\text{when } x=a) = m(m-1) \cdots (m-n+1)\chi^{(m-n)} a.
\]

The expression \( \{ \phi D, \psi(x, a) \} u \), considered as a function of \( a \), involves an idea of some novelty, which may possibly prove useful as an extension of our means of expression. I shall not, however, pursue this branch of the theorem further than to observe, that since

\[
\{ \phi D, \psi(x, a) \} u = \psi(\nabla + a) \phi D \cdot u;
\]

we have

\[
\frac{d^n}{da^n}(\{ \phi D, \psi(x, a) \} u) = \psi^{(n)}(\nabla + a) \phi D \cdot u = \{ \phi D, \psi^{(n)}(x, a) \} u;
\]

\( \psi^{(n)}(x, a) \) being \( \frac{d^n \psi(x, a)}{dx^n} \).

The other branch of the theorem,

\[
\phi D\{ \psi x, u \} \quad (\text{when } x=a) = \psi(\nabla + a)(\phi D) \cdot u,
\]

possesses an immediate value by enabling us to arrive almost instantaneously at the results of differential operations when the variable receives a particular value, and to express such results explicitly instead of merely indicating them. The formula is one of great simplicity, particularly when \( a=0 \). The following examples will illustrate the nature of the process:

1. \( \phi D(e^{n(x-a)}\chi x) \) (when \( x=a \) = \( e^{\nabla} \psi x \cdot u = \phi(D + n) \cdot \chi a \).

2. \( \phi D\{ (A_0 + A_1(x-a) + A_2(x-a)^2 + \cdots + A_n(x-a)^n)\chi x \} \)

\( (\text{when } x=a) = (A_0 \phi D + A_1 \phi D + \cdots + A_n \phi^{(n)} D)\chi a \).

3. Let

\[
(A_0 + A_1 t + \cdots + A_n t^n)^{-1} = \frac{B_0}{t - \alpha} + \frac{B_1}{t - \alpha_1} + \cdots + \frac{B_n}{t - \alpha_n} = \sum \frac{B}{t - \alpha},
\]

then

\[
\phi D(\{ (A_0 + A_1(x-a) + \cdots + A_n(x-a)^n)^{-1} \psi x \} \text{, when } x=a, = -\left( \sum \frac{B}{\alpha} - \phi D + \sum \frac{B}{\alpha^2} \cdot \phi D + \sum \frac{B}{\alpha^3} \cdot 2 \phi'' D + \sum \frac{B}{\alpha^4} \cdot 2 \cdot 3 \phi''' D + \cdots \right) \psi x \).\]
If the coefficient $A_0$ be zero, or if $x-a$, or $(x-a)^n$ be a factor of the original expression, the case involves some considerations which render it necessary to proceed with caution. Let us consider the equation

$$D^n((x-a)^{-1}u) \text{ (when } x=a) = \nabla^{-1}D^n u = \frac{1}{n+1} D^{n+1} u,$$

which, as it is obtained by the formula, might appear to be true universally. But we have

$$D^n\left(\frac{u}{x-a}\right) = \frac{D^n u}{x-a} - \frac{n}{(x-a)^2} D^{n-1} u + \frac{n(n-1)}{(x-a)^3} D^{n-2} u - \ldots$$

$$+ \frac{n(n-1) \ldots 1}{(x-a)^n+1} u,$$

which becomes infinite when $x=a$.

If, however, $u$ itself should contain $a$, and have $n+1$ roots equal to $a$, then each term assumes the form $\frac{0}{0}$; and, obtaining these singular values by differentiation of the numerators and denominators, it becomes

$$D^{n+1} u \left\{ 1 - \frac{n}{2} + \frac{n(n-1)}{2 \cdot 3} - \frac{n(n-1)(n-2)}{2 \cdot 3 \cdot 4} + \ldots \right\},$$

which is $\frac{1}{n+1} D^{n+1} u$, being the result obtained by the formula.

In the same way

$$D^n((x-a)^m u) \text{ (when } x=a) = \frac{D^{n+m+1} u}{(n+1)(n+2) \ldots (n+m)}$$

may be verified, if $u$ has $m+n$ roots equal to $a$. We are not, however, ordinarily to regard $u$ as containing $a$.

4. Let $\psi x = a_0 + a_1 x + \ldots + a_n x^n$; then, since

$$\psi(\nabla + \alpha) = \psi \alpha + \psi' \alpha \cdot \nabla + \frac{1}{2} \psi'' \alpha \cdot \nabla^2 + \ldots,$$

we have

$$\phi D \{ (a_0 + a_1 x + \ldots + a_n x^n) \chi x \} = \psi \alpha \cdot \phi D(\chi x) + \frac{1}{2} \psi'' \alpha \cdot \phi'' D(\chi x) + \ldots,$$

when $x = a$, a root of the expression $\psi x$.

5. Let $u$ be a function of $x$ such that $x+u$ (or $t$) is expandible in positive powers of $x$, then

$$\phi D (u^{n+r}) = \phi D (x^{n+r}),$$

which, when $x=0$, is $\phi^{(r)} D(t^{n-r})$.

Again, we have

$$\phi D (u^{n} (D^t)) = \phi D (x^{n-r} (D^t)) = \frac{n}{n-r} \phi D (x^{r} (D^t^{n-r}))$$
which, when \( x=0 \), is

\[
\frac{n}{n-r} \nabla^r \phi D(Dt^{n-r}) = \frac{n}{n-r} \phi^{(r)} D(Dt^{n-r}).
\]

If in the first of these we take \( D^n \) for \( \phi D \), and in the second \( D^{n-1} \) for \( \phi D \), we obtain

\[
D^n(u^r t^n) \quad \text{(when } x=0) = n \ldots (n-r+1)D^{n-r}t^{n-r},
\]

\[
D^{n-1}(u^r(Dt^n)) \quad \text{(when } x=0) = n \ldots (n-r+1)D^{n-r}t^{n-r};
\]

from which Burmann's and Lagrange's theorems are immediately deducible.

One of the most obvious applications of the theorem is to algebraical developments by means of Maclaurin's theorem; in which it will be found to give a remarkable degree of facility to the determination of the coefficients.

With the view of obtaining these coefficients in terms of the differences of nothing, I premise the following lemma.

**Lemma.** The absolute term of \( \psi \Delta \cdot t^n \), or its value when \( t=0 \), is \( \psi \Delta \cdot 0^n \). For

\[
\psi \Delta \cdot t^n = \psi \Delta (1+\Delta)^t \cdot 0^n = (1+\Delta)^t \cdot \psi \Delta \cdot 0^n = \psi \Delta \cdot 0^n,
\]

when \( t = 0 \). Or it may be proved by extracting the coefficient of \( t^0 \) in \( \Delta^m t^n \), and it will be found to be identical with \( \Delta^m 0^n \).

Now in the expansion of \( \phi x \) by Maclaurin's theorem, our object is to ascertain the value of \( [D^n \cdot \phi x] \), that value being the coefficient of \( x^n \cdot (1 \cdot 2 \ldots n) \). To effect this object, we have

\[
[D^n \phi x] = \phi \nabla \cdot D^n \cdot 1 = \text{coefficient of } D^0 \text{ in } \phi \nabla \cdot D^n,
\]

or in \( \phi \left( \log(1+\Delta) \right) \cdot D^n \), which by the lemma is \( \phi \left( \log(1+\Delta) \right) \cdot 0^n \); a formula which expresses the required coefficient in terms of the differences of nothing. The theorem may be thus conveniently written,

\[
\phi x = \phi \left( \log(1+\Delta) \right) \cdot e^{\Delta x} (\Delta \text{ operating on } 0).
\]

This theorem may be illustrated by applying it to some of the more important algebraical expansions, such as are given in the 18th chapter of Mr. De Morgan's Differential Calculus, to which I shall refer by the number of the section.

1. To expand \( f(e^x-1) \) in powers of \( x \). Coefficient of \( x^n \) is \( f \Delta \cdot 0^n \). Thus

\[
(e^x-1)^m = \frac{\Delta^m 0^m}{1 \cdot 2 \ldots m} x^m + \frac{\Delta^m 0^{m+1}}{1 \cdot 2 \ldots (m+1)} x^{m+1} + \ldots \quad (\text{Sec. 60}).
\]
2. To expand \( f(e^x) \); Herschel's theorem. Coefficient of \( \frac{x^n}{1.2...n} \) is
\[
f(1 + \Delta) \cdot 0^n. \text{ (Sec. 160.)}
\]

3. To expand \( \frac{x}{e^x - 1} \). Coefficient of \( \frac{x^n}{1.2...n} \) is
\[
\frac{\log(1 + \Delta)}{\Delta} 0^n, \text{ or } 0^n - \frac{\Delta 0^n}{2} + \frac{\Delta^2 0^n}{3} - \ldots + \frac{\Delta^n 0^n}{n+1},
\]
which is therefore the general expression for Bernoulli's numbers.

4. To expand \( \frac{1}{e^x + 1} \). Coefficient of \( \frac{x^n}{1.2...n} \) is \( \frac{1}{2 + \Delta} 0^n \), which is
\[
\frac{1}{2} \left( 0^n - \frac{\Delta 0^n}{2} + \frac{\Delta^2 0^n}{4} - \frac{\Delta^3 0^n}{8} + \ldots + \frac{\Delta^n 0^n}{2^n} \right). \text{ (Sec. 17.)}
\]

The expansion of \( \frac{x}{e^x - 1} \) may be thus obtained:
\[
\left[D^n \left( \frac{x}{e^x - 1} \right) \right] = \left[ \frac{1}{e^x + 1} D^n. x \right] = [\Sigma D^n. x] = \text{ coefficient of } D \text{ in } \Sigma D^n. \text{ Now}
\]
\[
\Sigma D^n = 0^n. \Delta + 0^n. \frac{D(D-1)}{2} + \Delta^0 0^n. \frac{D(D-1)(D-2)}{2 \cdot 3} + \ldots \text{ (sec. 41);}
\]
and the required coefficient is \( 0^n - \frac{1}{2} \Delta 0^n + \frac{1}{3} \Delta^2 0^n - \ldots \), as before.

In like manner to expand \( \left( \frac{x}{e^x - 1} \right)^2 \), we have
\[
\left[D^n \left( \frac{x^2}{e^x - 1} \right)^2 \right] = [\Sigma (\Sigma D^n). x^2] = \text{ twice the coefficient of } D^2 \text{ in } \Sigma (\Sigma D^n). \text{ Now}
\]
\[
\Sigma (\Sigma D^n) = 0^n. \frac{D(D-1)}{2} + 0^n. \frac{D(D-1)(D-2)}{2 \cdot 3}
\]
\[
+ \Delta^0 0^n. \frac{D(D-1)(D-2)(D-3)}{2 \cdot 3 \cdot 4} + \ldots ;
\]
and the required coefficient is
\[
2 \left( \frac{1}{2} 0^n - \frac{1}{3} \left( 1 + \frac{1}{2} \right) \Delta 0^n + \frac{1}{4} \left( 1 + \frac{1}{2} + \frac{1}{3} \right) \Delta^2 0^n - \frac{1}{5} \left( 1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} \right) \Delta^3 0^n + \ldots \right).
\]

Proceeding in a similar way to expand \( \left( \frac{x}{e^x - 1} \right)^p \), we shall require to find \( (1.2.3...p) \times \text{ coefficient of } D^n \text{ in } \Sigma^n D^n \). This
will be found to be

\[
0^n - \frac{1 + 2 + \ldots + p}{p + 1} \Delta 0^n + \frac{2 \cdot (p + 1)}{(p + 1)(p + 2)} \Delta^2 0^n
- \frac{3 \cdot (p + 2)}{(p + 1)(p + 2)(p + 3)} \Delta^3 0^n + \ldots;
\]

where \( q \cdot \frac{p + q - 1}{p - 1} \) denotes the sum of the products of \( q \) individuals in every combination that can be made from the series of natural numbers \( 1 \cdot 2 \cdot 3 \cdot 4 \ldots (p + q - 1) \).

This expression, therefore, is the coefficient of \( \frac{x^n}{1 \cdot 2 \ldots n} \) in

\[
\left( \frac{x}{e^x - 1} \right)^p;
\]

and, this coefficient being, as we have seen,

\[
\left( \frac{\log (1 + \Delta)}{\Delta} \right)^p 0^n,
\]

we have

\[
\left( \frac{\log (1 + x)}{x} \right)^p = 1 - \frac{1 \cdot p}{p + 1} x + \frac{2 \cdot (p + 1)}{(p + 1)(p + 2)} x^2
- \frac{3 \cdot (p + 2)}{(p + 1)(p + 2)(p + 3)} x^3 + \ldots \quad \text{(Sec. 65.)}
\]

Now it is easily shown that the sum of the products now under consideration is obtained by forming

\[
\ldots \Sigma \left( p \Sigma (p \Sigma p) \right) \ldots
\]

(there being \( q \) operations denoting multiplication by \( p \) and summation with regard to \( p \)), and by writing \( p + q \) for \( p \) in the result; so that the coefficient of \( x^q \) in \( \left( \frac{-\log (1 - x)}{x} \right)^p \) is

\[
\ldots \Sigma \left( (p + q) \Sigma (p + q) \right) \ldots \quad \text{\( q \) times}
\]

\[
\frac{1}{(p + 1) \cdot (p + 2) \ldots (p + q)}.
\]

This last example suggests an obvious method by which we can find the expansion of \( \phi \log (1 + x) \) wherever we are acquainted with the coefficient of \( x^n \) in \( \phi x \).

Thus, since the coefficient of \( x^n + 1 \cdot 2 \ldots n \) in \( (e^x - 1) + x \) is \( \frac{1}{n + 1} \), we have

\[
\frac{\Delta}{\log (1 + \Delta)} 0^n = \frac{1}{n + 1}.
\]

Therefore, if

\[
\frac{\Delta}{\log (1 + \Delta)} = V_0 + V_1 \Delta + V_2 \Delta^2 + \ldots + V_n \Delta^n + \ldots,
\]
we have for the determination of $V_n$ the equation

$$\Delta^n 0^n \cdot V_n + \Delta^{n-1} 0^n \cdot V_{n-1} + \ldots + \Delta 0^n \cdot V_1 + 0^n V_0 = \frac{1}{n+1} \text{ (sec. 61.),}$$

from which the values of $V_n$ can be found by giving to $n$ successive numerical values.

In like manner, since the coefficient of $x^n + 1 \cdot 2 \ldots n$ in

$$\left(\frac{e^x - 1}{x}\right)^p = \frac{\Delta^p 0^{n+p}}{(n+1)(n+2) \ldots (n+p)},$$

we have

$$\left(\frac{\Delta}{\log (1+\Delta)}\right)^p 0^n = \frac{\Delta^p 0^{n+p}}{(n+1)(n+2) \ldots (n+p)} \text{;}$$

and, the expansion of $\left(\frac{\Delta}{\log (1+\Delta)}\right)^p$ being $V_0 + V_1 \Delta + V_2 \Delta^2 + \ldots$, we determine $V_n$ from

$$\Delta^n 0^n \cdot V_n + \Delta^{n-1} 0^n \cdot V_{n-1} + \ldots = \frac{\Delta^p 0^{n+p}}{(n+1) \ldots (n+p)}. \text{ (Sec. 61.)}$$

Since the coefficient of $x^n + 1 \cdot 2 \ldots n$ in $\log (1-x)$ is

$$- (1 \cdot 2 \cdot 3 \ldots (n-1)),$$

we have for the determination of $V_n$ in

$$\log (1 - \log (1+\Delta)) = V_1 \Delta + V_2 \Delta^2 + \ldots V_n \Delta^n + \ldots$$

the equation

$$\Delta^n 0^n \cdot V_n + \Delta^{n-1} 0^n \cdot V_{n-1} + \ldots + \Delta 0^n \cdot V_1 = -(1 \cdot 2 \cdot 3 \ldots (n-1)). \text{ (Sec. 61.)}$$

In like manner, if

$$\left(1 - \log (1+\Delta)\right)^{-1} = V_0 + V_1 \Delta + V_2 \Delta^2 + \ldots,$$

we have

$$\Delta^n 0^n \cdot V_n + \Delta^{n-1} 0^n \cdot V_{n-1} + \ldots + 0^n V_0 = 1 \cdot 2 \cdot 3 \ldots n; \text{ and so on for other developments.}$$

Generally, if

$$\phi(\log (1+\Delta)) = V_0 + V_1 \Delta + V_2 \Delta^2 + \ldots,$$

we determine the coefficients $V_0, V_1 \ldots$ by the equation

$$\Delta^n 0^n \cdot V_n + \Delta^{n-1} 0^n \cdot V_{n-1} + \ldots + \Delta 0^n \cdot V_1 + 0^n V_0 = [\phi^{(n)} x]. \text{ (Sec. 166.)}$$

Various properties of the differences of nothing may be obtained very simply by this method. Thus, since the coefficient of $x^n + 1 \cdot 2 \ldots n$ in $x^n$ is 0, save when $n=m$, in which case it is $1 \cdot 2 \ldots m$, we perceive that $\left(\log (1+\Delta)\right)^m 0^n$ is 0, except when $n=m$, in which case it is $1 \cdot 2 \ldots m$. \text{ (Sec. 166.)}$$

The coefficient of $x^n + 1 \cdot 2 \ldots n$ in $x^m$ is $n$; therefore

$$(1+\Delta) \log (1+\Delta) \cdot 0^n,$$
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The coefficient of $x^{n-1} \cdot 2 \cdot n$ in $e^{-px}$ is $\pm p^n$, according as $n$ is even or odd; therefore $(1 + \Delta)^{-p} \cdot 0^n$, or

$$0^n - p \Delta 0^n + \frac{p(p + 1)}{2} \Delta^2 0^n - \frac{p(p + 1)(p + 2)}{2 \cdot 3} \Delta^3 0^n + \ldots$$

$$\pm \frac{p(p + 1) \ldots (p + n - 1)}{2 \cdot 3 \ldots n} \Delta^4 0^n = \pm p^n.$$

Similarly,

$$p^n = 0^n + \Delta 0^n \cdot p + \Delta^2 0^n \cdot \frac{p(p - 1)}{2} + \ldots$$

The coefficient of $x^n \div 1 \cdot 2 \cdot n$ in $(\frac{e^x - 1}{x})^p$ is

$$\frac{\Delta^p 0^{p+n}}{(n+1) (n+2) \ldots (n+p)};$$

therefore

$$\left(\left(\frac{\Delta}{\log (1 + \Delta)}\right)^p \cdot 0^n = \frac{\Delta^p 0^{p+n}}{(n+1) \ldots (n+p)}\right).$$

Again, since

$$\phi x = \phi \left(\log (1 + \Delta)\right) \cdot e^{0x},$$

we have

$$\phi^{(n)} x = \phi^{(n)} \left(\log (1 + \Delta)\right) \cdot e^{0x},$$

and also

$$\phi^{(n)} x = \phi \left(\log (1 + \Delta)\right) \cdot 0^n \cdot e^{0x}.$$

Consequently

$$\phi^{(n)} \left(\log (1 + \Delta)\right) \cdot 0^m = \phi \left(\log (1 + \Delta)\right) \cdot 0^{m+n}.$$

From the nature of the formula which we are exemplifying, it may be expected that its principal applications will be among those theorems which involve operations of differentiation, the variable receiving a particular value after the operations are performed. Of these, the principal instances, after Maclaurin's theorem, which has been considered, are the developments which flow from Lagrange's and Burmann's theorems, of which I shall take some examples.

I shall assume as known the coefficients of the expansion of $(a + bx + cx^2 + dx^3 + \ldots)^{-n}$ in the form

$$A_n + B_n x + C_n x^2 + E_n x^3 + \ldots,$$

which are easily found thus:

\[ A_n = a^{-n}, \quad B_n = b \frac{dA_n}{da}, \quad C_n = c \frac{dA_n}{da} + b \frac{dB_n}{da}, \]
\[ E_n = e \frac{dA_n}{da} + c \frac{dB_n}{da} + b \frac{dC_n}{da}, \]
and so on.

One remarkable development of the character above alluded to is the expansion of \( \psi x^{-1} x \) in powers of \( x \), \( \psi x \) and \( \chi x \) being given forms. If \( \alpha \) be a root of \( \chi x \), then
\[
\psi x^{-1} x = \psi \alpha + \left( \frac{x-\alpha}{\chi x} \right) \cdot \psi' x \right) \cdot x + \frac{d}{dx} \left( \left( \frac{x-\alpha}{\chi x} \right)^2 \cdot \psi' x \right) \cdot \frac{x^2}{2} \\
+ \frac{d^2}{dx^2} \left( \left( \frac{x-\alpha}{\chi} \right)^3 \cdot \psi' x \right) \cdot \frac{x^3}{2 \cdot 3} + \ldots ,
\]
where \( x \) is made equal to \( \alpha \) in the coefficients of the powers of \( x \) after the differentiations have been performed. It is now required to express these coefficients explicitly as functions of \( \alpha \); for which purpose we have the general term
\[
D^{n-1} \left( \left( \frac{x-\alpha}{\chi x} \right)^n \cdot \psi' x \right) = \left( \frac{\nabla}{\chi(\nabla + \alpha)} \right)^n D^{n-1} \cdot \psi' \alpha ,
\]
the \( D \) now denoting differentiation with regard to \( \alpha \).

For example, let
\[
\chi x = ax + bx^2 + cx^3 + \ldots ,
\]
so that a root of \( \chi x \) is zero; then
\[
\left( \frac{x}{\chi x} \right)^n = A_n + B_n x + C_n x^2 + E_n x^3 + \ldots ,
\]
whence
\[
\psi x^{-1} x = \psi \alpha + \left( \frac{\nabla}{\chi x} \right) D^0 \cdot \psi' \alpha \cdot x + \left( \frac{\nabla}{\chi x} \right)^2 \cdot \psi'' \alpha \cdot \frac{x^2}{2} \\
+ \left( \frac{\nabla}{\chi x} \right)^3 \cdot \psi'' \alpha \cdot \frac{x^3}{2 \cdot 3} + \ldots \\
\]
\[
= \psi 0 + A_1 \psi' 0 \cdot x + (A_2 \psi'' 0 + B_2 \psi' 0) \cdot \frac{x^2}{2} \\
+ (A_3 \psi''' 0 + 2B_3 \psi'' 0 + 2 \cdot 1 \cdot C_3 \psi' 0) \cdot \frac{x^3}{2 \cdot 3} \\
+ (A_4 \psi'''' 0 + 3B_4 \psi''' 0 + 3 \cdot 2C_4 \psi'' 0 + 3 \cdot 2 \cdot 1 \cdot E_4 \psi' 0) \cdot \frac{x^4}{2 \cdot 3 \cdot 4} + \ldots
\]

If, however, \( \chi x \) be a function which does not vanish with \( x \), but has a root \( \alpha \), then we have
\[
\left( \frac{\nabla}{\chi(\nabla + \alpha)} \right)^n = \left( \frac{\nabla}{\chi' \alpha + \frac{1}{2} \chi'' \alpha \cdot \nabla^2 + \ldots} \right)^n \\
= \left( \chi' \alpha + \frac{1}{2} \chi'' \alpha \cdot \nabla + \frac{1}{2 \cdot 3} \chi''' \alpha \cdot \nabla^2 + \ldots \right)^{-n},
\]
Calculus of Operations to Algebraical Expansions.

which suppose equal to

$$A_n + B_n \nabla + C_n \nabla^2 + E_n \nabla^3 + \ldots,$$

the law of the coefficients of $\nabla$ being the same as that before given, using $\chi'\alpha, \frac{1}{2} \chi''\alpha, \&c.$ for $a, b, \&c.$ With this variation, we have, as before,

$$\psi \chi^{-1} x = \psi \alpha + A_1 \psi' \alpha \cdot x + (A_2 \psi'' \alpha + B_2 \psi' \alpha) \cdot \frac{x^2}{2} + \ldots$$

$$+ A(3 \psi''' \alpha + 2B_3 \psi'' \alpha + 2C_3 \psi' \alpha) \cdot \frac{x^3}{3} + \ldots$$

This process has the further advantage of avoiding the difficulties arising from $\chi^x$ having equal roots. If $\chi^x$ have $m$ roots equal to $\alpha$, then

$$\left(\frac{\nabla}{\chi(\nabla + \alpha)}\right)^n = \left(\frac{\nabla}{\frac{1}{2.3..m} \chi^{(m)} \alpha \cdot \nabla^{m} + \ldots}\right)^n$$

$$= \nabla^{-(m-1)n} \left(\frac{1}{2.3..m} \chi^{(m)} \alpha + \frac{1}{2.3..(m+1)} \chi^{(m+1)} \alpha \cdot \nabla + \ldots\right)^{-1}.$$

Forming the coefficients $A_n, B_n, \&c.$ with the new values of $a, b, c, \&c.$, we have

$$\psi \chi^{-1} x = \psi \alpha + (A_1 + B_1 \nabla + \ldots) \nabla^{-(m-1)} \cdot D^0 \cdot \psi' \alpha \cdot x$$

$$+(A_2 + B_2 \nabla + \ldots) \nabla^{-(2m-1)} \cdot D \cdot \psi' \alpha \cdot \frac{x^2}{2} + \ldots$$

$$= \psi \alpha + (A_1 + B_1 \nabla + \ldots) \frac{D^{m-1}}{1.2.3..(m-1)} \cdot \psi' \alpha \cdot x$$

$$+(A_2 + B_2 \nabla + \ldots) \frac{D^{2m-1}}{2.3..(2m-1)} \cdot \psi' \alpha \cdot \frac{x^2}{2} + \ldots$$

$$= \psi \alpha + \left(\frac{A_1}{1.2..(m-1)} \psi^{(m)} \alpha + \frac{B_1}{1.2..(m-2)} \psi^{(m-1)} \alpha + \ldots\right) x$$

$$+ \left(\frac{A_2}{2.3..(2m-1)} \psi^{(2m)} \alpha + \frac{B_2}{2.3..(2m-2)} \psi^{(2m-1)} \alpha + \ldots\right) \cdot \frac{x^2}{2} + \ldots$$

It will be seen from the examples which have been given that the object has been to exemplify the peculiar powers of the new process rather than to exhibit any new results, though many of those above given will be found to present themselves in a new form.

Dublin, August 22, 1853.
LV. A Main Cause of Discordant Views on the Structure of the Muscular Fibril. By Martin Barry, M.D., F.R.S., F.R.S.E.*

[With a Plate.]

I LATELY had the pleasure, on more than one occasion, of examining muscle at Glasgow with Prof. Allen Thomson. The microscope used was one of first-rate excellence by Smith and Beck. We saw the states in Plate V. fig. 2 ab and ikl—drawings sent me a few days after by the Professor, accompanied by the following description and remarks:—"Portions of three different muscular fibrillae from the Frog, presenting various aspects. ab, a fibril splitting into two at c, viz. cd and ce; the part ac is probably double; ce and cd are single. db appears single; but it is possible that it may be double, as e seems to have been separated from c to d. From a to c the quadrilateral form of the sarcal particles and the clearer intervening substance prevails; in ce and cf the oval and oblong quadrilateral; from f to d the rhomboidal; and from d to b the rhomboidal, with an appearance of spiral or twist in the intervals.—g and h are portions of another fibrilla, in which the quadrilateral form of the particles, and the distinction between them and the intervals, are particularly well marked; in g the upper surface is in focus, and the cross mark in the intervals is not seen; in h the focus is adjusted for the deeper or further side, the intervening substance is crossed by a distinct transverse line, and each dark quadrilateral particle presents a central spot, or rather a slightly crucial mark in its centre. Although the portion of fibril here represented appears quite single, I think I can see that at the end a portion of it, not represented in this figure, is split into two smaller fibrils.—ikl, two fibrillae separated at one part and united in another; a dislocation of the particles having taken place, so as to produce an appearance of spiral form even more distinctly than the drawing represents. l seems single; but it may be doubted, from what has been observed in following out other fibrillae of the same form, whether the part ending at k is reduced to its smallest elements. The specimen above described has been preserved for three or four years in the moist state in weak spirit or glycerine, I am not sure which. I think it is one which, along with others, I received from Dr. Dobie †.—A. T."

The Professor adds:—"In the upper figure, ab, you will see that I have suggested in the description the possibility of the part from b to d being double. I am induced to think so, both

* Communicated by the Author.
† Dr. William Murray Dobie, at present one of the house surgeons in the Royal Infirmary of Edinburgh.—M. B.
because $ce$ appears to have been separated from $ca$ only as far as $d$, and because in several adjustments I have observed the appearance of each particle being made up of two slightly dislocated (a disposition which is slightly indicated in the last particle at $b$). I still admit that the portion of fibril, including these four particles from $b$ to $d$, present very much the appearance of a spiral or twist; but, since it is the only portion among a very great many fibrils in which I have seen the spiral form (that is, a spiral appearance not obviously caused by dislocation, as in $ik$, which, as you know, might mislead), I must in the meantime suspend my judgement as to the cause of this seeming spiral.” He further remarks:—“As I stated to you in conversation, I cannot make up my mind as to the nature of this structure;” and adds:—“I shall therefore look with much interest for the more full explanation of your observations.” With regard to specimens that I had shown him, sent me by Dr. Thomas Spencer Cobbold*, from the tentacula of the Actinia,—in which he (Dr. C.) had discovered proofs of the accuracy of my views,—Prof. Allen Thomson, after remarking that they present a clear view of the double spiral, adds:—“I admit that if these double spiral prehensile filaments of the Actinia are contractile, they may be fairly used by you as an argument in favour of your views.” Besides this, the Professor admits “the existence of the spirals very clearly—in the heart.”

If all observers were as circumspect in making up their minds as Allen Thomson, science would progress with less of fluctuation. The more full explanation of my observations, for which that distinguished physiologist is good enough to say he shall look with much interest, requires however but very few words.

Divisions such as those we together saw, and which have been most faithfully delineated by him in fig. 2, I had figured in the Edinburgh New Philosophical Journal for October 1843, plate 5. fig. 2; and in Müller’s Archiv for 1850†. When subsequently examining, along with the Professor, a preparation sent me by Dr. Dobie, we found several of the states in fig. 4, which fully confirm and extend the observations now referred to as recorded by myself in 1843 and 1850‡. For this drawing also (fig. 4) I

* Under-Conservator of the Museum of Anatomy and Physiology in the University of Edinburgh.
† Taf. XVII. fig. 29. See also Phil. Mag. for August 1852, Plate I. fig. 6.
‡ Changes such as those in fig. 4 are obviously intimated by the “crucial mark” at $h$ in fig. 2. Concerning this crucial mark, the Professor, in a letter I have since received from him, remarks:—“This was well given, if I remember right, in Dr. Dobie’s paper.” [Published, I hear, in the Annals of Natural History for February 1849; but I have not seen it.—M. B.]
am indebted to the kindness of Prof. Allen Thomson. It needs no description, affording unquestionable evidence of division and subdivision—changes which observers have overlooked, or at least in their consequences disregarded. These changes, with those seen in fig. 2, and with what I am about to state, furnish the explanation he requires.

There first exists a line of bodies comparable to germinial spots. Each spot divides into halves, and then each half into four parts; so that each spot comes to consist of eight particles, which eight particles lie in two strata—four in each. This is shown by fig. 5 (also from nature, and drawn by Prof. Allen Thomson), the particles in outline representing the upper, and the shaded particles the lower stratum. Dislocation takes place, a change immediately following the division into halves. Of dislocation an example is afforded by fig. 2 ikl, which presents a side view. As dislocation proceeds, there arises in the clear space an appearance which we call a transverse line. Of this line no satisfactory explanation has yet been given. My belief is that it results from particles belonging to the stratum not in focus. This, I think, is shown by b and c in the drawings from nature, fig. 4, where it is no longer a mere line that is seen, but there have come into view particles not differing from those of the stratum that is in focus. In harmony with this opinion is the following remark by Prof. Allen Thomson, written by him opposite the drawing from nature, fig. 5, viz. "The transverse line in the clear space is seen when the lower side is in focus; and coincides exactly in the specimen figured with the margin a of the square particles when the upper side is in focus."—The line of particles b, fig. 4, affords an instance of longitudinal separation, exhibiting one half of such a line as that at c in the same figure. There can be no doubt that in these lines we see the smallest elements discernible with our highest magnifying powers.—The line a in the same figure (fig. 4) appears to represent a state corresponding to that of the line b; but with this important difference, that in a the particles are flat. Such flattened particles I sketched in Müller's Archiv for 1850*, and reproduce the sketch in fig. 3.

In now proceeding to point out the ways, in one or more of which I think it possible that such a line may pass into a spiral form, I would ask a reference to drawings I gave from nature in 1842, after a long-continued examination of the elements of fibre at the earliest period†. For I presume that no one will say that what was seen of the earliest formation of fibre may not be applied in endeavours to throw some light upon its mode of reproduction.

* Taf. XVII. fig. 29.
† Phil. Trans. 1842, plate 7, figs. 45 to 48.
Those drawings show spirals to arise out of piles of particles having a ring-like form. The rings were met with and represented arranged in three ways, viz. 1st, in a single pile, as in fig. 6 A; 2nd, arranged in alternate or overlapping order, as in fig. 6 B; 3rd, connected like links of a chain, as in fig. 6 C. Rings arranged in all these ways were found in piles; and rings arranged in all these ways were seen passing into spirals*. I further showed the existence of such bodies as that in fig. 6 D†, an altered ring, which if produced must pass into some form of spiral. When each ring of the first arrangement, A, assumes the form D, union of the extremities of a pile of bodies such as D forms a single spiral, and this by longitudinal division passes into two, as in fig. 6 E‡. When, according to the second arrangement, the rings overlap each other, as in fig. 6 B, or, according to the third arrangement, are connected, as at C, the union of the extremities of a pile of such bodies as that at D is attended with interlacement, forming at once the twin spiral E.

[Of this twin spiral a drawing from nature (Heart of Frog) is seen in fig. 7. It represents neither full contraction nor complete relaxation, but four intermediate states; and these were seen at different parts of the same fibril. As the two spirals run in the same direction, i.e. as they are parallel, I have been accustomed latterly to term this fibril, and indeed all organic fibre, a twin spiral§.—In all three of the arrangements I showed the rings to have become segmented, as in fig. 7 ½ b, an appearance of course familiar to all accustomed to examine the elements of tissues. The segments intimate the formation of the particles of which spirals are composed||.—The pellucid centres of the rings are left as elements of future offspring, to assume the form of spirals when their progenitors the old spirals as contractors are worn out.]

What has just been referred to as seen of the mode of origin of the muscular fibril, I would now apply in considering its mode of reproduction.

Thus the flat particles in fig. 4 a I apprehend to be in a state resembling or approaching that of rings. They are, in fact,

* Phil. Trans. 1842, plates 6 and 7, figs. 31–33, 47, 48.
† Phil. Trans. 1842, plates 5, 6 and 11, many figures.
‡ Fig. 6 E represents an apparatus for constructing a model of the twin spiral muscular fibril, to be explained further on.
§ I had previously called it a double spiral; but this seems not so fully to imply that the direction of the two spirals is the same. [Originally I believed their directions to be different, but corrected the error in Müller’s Archiv for 1850, and in the Phil. Mag. for 1852.]
|| Phil. Trans. 1842, plate 10, fig. 125. I also showed, by the action of acetic acid, that spiral filaments are made up of particles. (Phil. Trans. 1842, plate 8, fig. 68.)
bodies of the same form as mammiferous blood-discs, fig. 7½ a. Each has its pellucid centre or nucleolus, which, when the outer part assumes the spiral form, is left behind—a line of such nucleoli being the foundation of future offspring.

Now I have no doubt that a, fig. 4, passes into a twin spiral in one of the ways just described. If in the first way, it probably undergoes longitudinal separation into two single piles such as that in fig. 3; and then each pile forms a single spiral, which by longitudinal division becomes a double one. But if a, fig. 4, assumes the spiral form in either of the two other ways, it undergoes no longitudinal separation, and it forms but one twin spiral.

It is obvious that the bodies df, fig. 2, pass into the bodies bd in the same figure. The question is: what is the condition of bd? Prof. Allen Thomson believes it to be double. I am of the same opinion. Even he admits it to present "very much the appearance of a spiral." And here also agreeing with him, I would direct attention to a change in the direction of the transverse line in the clear spaces, which direction in bd crosses that of the transverse line in df. It may therefore be that df, fig. 2, consists of two strata of particles, which particles come to alternate with, or overlap one another, as at B in fig. 6; and that further changes, such as those just described, produce an approach towards the completion of a twin spiral in fig 2 bd.

Whether, however, an approach towards the completion of a twin spiral is or is not exhibited in the fibril seen by Prof. Allen Thomson and myself, and delineated by him in fig. 2 bd, I certainly saw a twin spiral at the upper end of the fibril in fig. 1. This figure I published in the Edinburgh New Philosophical Journal for October 1843, plate 5. fig. 2*. And as it exhibits three states, viz. that of quadrilateral particles (c), division and dislocation of these (b), and then as a continuation of the latter the twin spiral (a) in the same fibril, I have thought the figure deserving of reproduction here (fig. 1). Along with this figure I published the following remarks, viz. "Were filaments formed by each half-nucleus (fig. 1 b) of two adjacent rows to assume the spiral form and interlace, and the filaments of the same row to then unite, we should have the double spiral. [The oblique position of the two rows of half-nuclei in fig. 1 b is not undeserving of notice here.]†"

It is satisfactory to find, that while the renewed inquiries made known in this communication enable me to explain some details,

* It represents a young fibril of muscle from the ventricle of a frog's heart; drawn as magnified 600 diameters.
they do not show that views thus long since published require an essential change.

A friend, long accustomed to use the microscope, and gifted with a keen microscopic eye, whom I had convinced of the existence of spirals in muscle, once suggested that the spiral structure might "be the earlier rather than the fully-developed condition of the fibril, the quadrilateral particles representing its fully-formed state." Referring him to the drawing reproduced in fig. 1, I was compelled, with almost rude brevity, to say that I should find it about as easy to admit that thread exists before the flax that forms it, or a chain before the links of which it is composed.

Consisting as it does of two spiral filaments, the muscular fibril, in its movements between contraction and relaxation, of course presents a variety of forms. And as the microscopic inquirer into the structure of muscle is sure to have one or more of such forms before him (unless all the muscle in the field of view is in full contraction or complete relaxation), I have constructed models by which the eye may be prepared for these forms.

In fig. 6E is shown a very simple method of constructing such a model. At a are two lead wires of equal length, which, held parallel and together, were obliquely wound upon a large knitting needle, b. The needle having been withdrawn, there remained a model of the mature and acting muscular fibril (and indeed of the original form of all organic fibre). So easily did the two wire spirals then change their positions, that on merely rolling the model on a white surface, I saw it present the three varieties of form a, b, c, drawn from nature in fig. 7; which include two states of the single and one of the double cylinder.

While, however, from their weight, wires of lead have the advantage just mentioned of presenting changes in relative position on the model being simply rolled, they are not elastic. I therefore used wire of gutta percha, fig. 6 F. At a in this figure is a bit of small wire of gutta percha. This I bent upon itself at c; and then, having firmly tied with thread the loop c to a knitting needle b, I wound the double wire upon the needle in a direction almost as nearly transverse as possible. Then, keeping firm pressure applied by the finger and thumb at the lower ends of the wires, I plunged the wires for a few seconds into water of about 130° Fahrenheit (44° Reaumur*). Thus softened and deprived of their elasticity, the wires were slipped off the needle.

* If the water be too hot, the wires over-softened adhere together.
In a few seconds they had hardened and recovered their elasticity. After having been minutely examined for the detection and breaking up (with a needle) of adhesions that might have occurred in the softening, the wires were gently extended, and found to spring back into the previous comparatively contracted state; thus beautifully illustrating the movements of the muscular fibril. The union seen in this model of the two spirals at the end of the fibril, is to be inferred from analogy with what I have elsewhere shown in cilia.

I cannot refrain from once more recommending the heart of some reptile as especially adapted for the examination of the fully-formed muscular fibril. The heart of the Turtle has often afforded me unquestionable specimens of the twin spiral*; but far more easily obtainable is that of the common Frog.

In order thoroughly to understand the structure of this tissue, however, it is essential to see it in its most incipient state, and patiently to follow it through every stage. At that early period its elements are very large, of course an immense advantage to the observer. I had this advantage; and using, as I for the most part did, the larva of the large Jersey Toad (which a friend informs me is a variety of the common Toad), I had a further advantage, the first elements of muscle in that larva being of enormous size. Out of these I saw spirals to arise of corresponding size; so large as to enable me not only to discern the particles of which they were composed†, but also to observe that by division and subdivision spirals pass into membrane, forming for instance the sarcolemma, the cells of cartilage and the cells of coagulating blood. [An observation confirmed ten years after by Agardh, who in his paper De cellula vegetabili fibrillis tenuissimis contexta, Lundae, 1852, shows not only that vegetable membrane is formed by fibre, but that the fibre forming vegetable membrane has the very structure that I maintain to be that of all organic fibre, being composed of spirals which in number he delineates as two, and moreover represents as dividing—each of them into a fasciculus of spirals‡.] And of course I refer to spirals the gentle undulations which constitute the first movements of the Tadpole’s tail; in other words, I conclude that it is the spirals that are endowed with contractile power; for to what else is to be attributed this power where all that can be recognized as muscle is made up of spirals?

The eye, when thus accustomed to spirals of large size, discerns them though exceedingly minute, as in older muscle.

* See, for instance, in the Phil. Trans. for 1842, plate 7, fig. 56.
† Phil. Trans. 1842, plate 8, fig. 68.
‡ Agardh, loc. cit. Tab. I. fig. 8.
Here observers, not thus prepared, mistook the winds of spirals for varicosities or beads; for instance, Bowman, who in single fibrillae figures as such what were evidently spirals*. Some of his delineations of fasciculi, also, afford beautiful illustrations of spirals distorted in manipulation, of which the appearance he aptly compares to "engine-turning" is an exquisite example. I can attest the accuracy of those delineations of fasciculi. They are faithful representations of nature; but, at the same time, they are proofs of the existence of a structure, which for the reason just mentioned the delineator did not understand. As for the "discs" of Bowman, they are transverse slices of fasciculi in full contraction, cut off where least capable of withstanding violence in manipulation. At such parts each spiral of a pair is then in contact with its fellow; there they cross and antagonize each other; in full contraction this crossing is at the acutest angles, and consequently there they cut each other through. And if maceration has been used, a practice too common with some observers, no wonder if the "discs" present little more than particles; for it is of particles that the spirals are composed.

How true the words of Huschke, that "in order to understand an organ or the structure of a tissue, we must inquire: How did it originate?" When muscle, a tissue more complicated than any other, has been thus dealt with by observers generally, they will understand and acknowledge its spiral structure; but not till then.

Since Bowman wrote, observers, in their endeavours to reach the ultimate structure of the muscular fibril, have actually gone too far. They passed over what really admits of examination—the mature fibril, and arrived at what almost defies the microscope—its embryo; mistaking and delineating for the fibril a row of quadrilateral particles, the mere elements thereof; mistaking for the chain, as it were, a row of half-formed links destined to compose the chain. It is not surprising, that, as the embryo fibril passes through many stages in the course of its development, it should have presented different appearances to different observers, producing a corresponding variety of descriptions and drawings. And I certainly cannot wonder that in a row of quadrilateral particles no one could discern my twin spirals! Without therefore questioning the accuracy of these observers in describing and delineating what they saw, I maintain, in the first place, that it was impossible for them to agree

* Phil. Trans. 1840, plate 16, figs. 10, 12, 15, drawings copied by myself into Müller's Archiv, 1850, Taf. 17, and into the Phil. Mag. 1852, Plate I., for the purpose of showing them to represent spirals.
I.VI. Further Remarks on the Muscularity of Cilia.

By Martin Barry, M.D., F.R.S. and R.S.E.*

[With a Plate.]

The Philosophical Magazine for August and September 1852 contains the substance of a paper of mine which had been translated into German from the English MS. by Prof. Purkinje, Foreign Member R.S., and communicated by him to Müller's Archiv for 1850; confirming by renewed inquiries, made in his house, the observations I had recorded in the Philosophical Transactions eight years before (1842) on the spiral structure of muscle, and announcing the muscular character of cilia. It was in bivalve Mollusca and in Infusoria that I saw the latter.

Arisen like independent beings each in its own cell or ovum, and endowed with contractile power, cilia of the Mussel's gill were shown to grow and pass through stages of development both in action and in form; and at length, when matured, and not till then, to combine in large numbers for the production of a current. A pellucid membranous canal, apparently destined to absorb oxygen from the water, was seen to exhibit on each side a phalanx of cilia (Plate V. fig. 10 a, o); while the extremities of these, arching over, united to form an avenue through which by their vehement undulatory vibrations to drive a stream, and thus arterialize the blood. It was further shown that young cilia (m, in the same figure) are continually arising to take the place of those that drive the stream; the latter, when worn out, being one after another cast off and swept away by that same stream, to be succeeded, as just said, by fresh generations, which produce and in their turn undergo like changes. My observation of continued renewal of muscular fibrils in the ever-acting heart, was thus confirmed by what I subsequently saw of continued renewal among incessantly vibrating cilia.

I have now to add, that, arising coil-like from the nucleus of a cell, the young cillum pushes forth the cell-wall, as in the outline fig. 8, to some little length before the extremity is free, often giving to its cell the flask- or retort-like appearance I formerly described as produced in blood-cells of one of the Am-

* Communicated by the Author.
phibia, from a like pushing forth of the cell-wall by the fibre given off by the nucleus in those cells. And when the extremity of the cilium is free, fig. 9, there is still to be recognized in the bent form of young cilia a trace of that in which the nucleus gave them off.

Every cilium is a twin spiral, like the muscular fibril. Fig. 9, a drawing from nature, represents a young cilium from the gill of the common Oyster, in the form in which it proceeds from its cell. At the extremity its two spirals pass into one another, and are bent over hook-like towards one side. At the base they separate, to bestride as it were a bulb consisting of minute pel-lucid globules of high refractive power. These globules I have elsewhere termed the contents of the cell, and such they are. More particularly considered, however, they represent the remains of the nucleus of the cell resolved into globules. The two after-threads derive their nourishment from, or rather they are formed by the globules in the bulb. The bulb thus gradually becoming exhausted diminishes in size, and at length entirely disappears. Hence it was that I found the bulb at the base of some cilia much smaller than at that of others. Hence, too, an explanation of the cause why some observers have denied the existence of a bulb; their attention having probably been directed to cilia from which the bulb had disappeared.

The two after-threads may perhaps be considered as the roots of the cilium, in which it has its early growth. The extremity of each of them takes up new substance from what had been the nucleus of the cell, while the cilium, by its rotatory movements, which consist in twisting and untwisting (contraction and relaxation), spins up the after-threads—its early mode of elongation. For this the after-threads are prepared by their spiral form, which also seems to be a provision for rendering the elongation of the cilium rapid. It will be seen from the figure that each after-thread is a single spiral, being twisted on itself; and that the direction of the spiral winds is in both of them the same; the same, moreover, as that of the twin spirals of the cilium of which the after-threads are continuations. Very few movements, therefore, of the kind just mentioned sufficed to apply to one another these after-threads, and thus to make them part of the cilium itself.

I have presented a model in lead wire of the muscular fibril, and of a young cilium such as that in fig. 9, to the Royal Society, to the Museum of the Royal College of Surgeons in London, to the Royal Society of Edinburgh, to various colleges and museums in Edinburgh, and to the Museum of Physiology in the University of Prague.
Along with the model of a young cillum presented to the Prague Museum, I left the following remark, translated into German by my honoured friend Prof. Purkinje:—"From analogy it appears extremely probable that the heart arises in like manner out of the nucleus of a cell, being originally such a double spiral [as that in fig. 9]. If so, the spiral form of the heart may be explained by the continued division of what was originally a double spiral fibre."

To the subject of self-division, as part of the process of reproduction, more importance will by and by be attached than heretofore. For as the properties of the simplest form of separate independent locomotive Infusoria descend to it from progenitors by fission,—by the same fissiparous mode it appears to me do properties descend from cell to cell, or rather from nucleolus to nucleolus, though these are not separate but combined, and merely parts of a more complicated organism. Having made known my observations on this subject in Müller's Archiv and in former numbers of this Journal*, I have here merely to repeat, for the purpose of applying it to the suggestion referred to concerning the heart, that the filaments of all organic fibre are made up of particles (nucleoli), and that these particles, and therefore the filaments of fibre, are reproduced in no other way than by self-division†.

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LVII. On a Generalization of the Lagrangian Theorem of Interpolation. By J. J. Sylvester, F.R.S.‡

There is a well-known theorem of Lagrange for determining the form of a rational integral function of one variable and of the degree \( m \), when its values corresponding to \( m + 1 \) values of the variable are assigned. M. Cauchy, in his Cours d'Analyse de l'Ecole Polytechnique, has extended this theorem to the case of a rational fraction, of which values corresponding to a sufficient number of values of the variable are given; but the solution of the question there given, although of course correct, is unsatisfactory, as it presents the numerator and denominator under forms not strictly analogous.

The theorem of Lagrange, in respect of its subject matter, may be best generalized as follows.

Suppose any number of functions of \( x \) of the several degrees \( m_1 - 1, m_2 - 1, \ldots, m_i - 1 \), say \( U_1, U_2, \ldots, U_i \), and that the equation

\[
l_1 \cdot U_1 + l_2 \cdot U_2 + \ldots + l_i \cdot U_i = 0
\]

* Müller's Archiv, Heft vi. 1850. Phil. Mag. August and September 1852.
† See a paper of mine in the Edinburgh New Philosophical Journal for October 1853, "On Animal and Vegetable Fibre."
‡ Communicated by the Author.
is known to be satisfied for \( m_1 + m_2 + \ldots + m_i - 1 \) (say) \( \mu - 1 \) assigned values of the system of quantities \( l_1, l_2, \ldots, l_b, x \); there will then be \( \mu - 1 \) linear equations connecting the \( \mu \) coefficients comprised in \( U_1, U_2, \ldots, U_b \) and therefore the ratios of these coefficients, and consequently of the functions to one another, may be determined. There is no difficulty in representing, by aid of the method of determinants, the result of solving these equations whatever be the number of functions; but for the sake of greater simplicity, I shall suppose three only of the several degrees, \( e - 1, i - 1, \omega - 1 \) in \( x \), which I shall call \( U, V, W \). Now suppose that \( U + mV + nW = 0 \) is known to be satisfied for \( l = l_0, m = m_0, n = n_0, x = x_0, t \) taking all possible values from 1 to \( e + i + \omega - 1 \), say \( \tau - 1 \); let the indices 1, 2, 3, \ldots, \( \tau - 1 \) be partitioned in every possible way into 3 groups, containing respectively \( e - 1, i \) and \( \omega \) indices, as

\[
\theta_1 \theta_2 \ldots \theta_{e-1}; \quad \theta_e \theta_{e+1} \ldots \theta_{e+i+1}; \quad \theta_{e+i} \ldots \theta_{\tau-1}
\]

(the terms in any group may be arranged indifferently in any order, but are not to be permuted). Let \( \xi^k(p, q, r \ldots s) \) denote in general

\[
(p - q) \times (p - r) \times (p - s) \times (q - r) \times (q - s) \times (r - s),
\]

and write

\[
K_1 = \Xi \left\{ l_0, l_0, \ldots, l_0; m_0, m_{e+1}, m_{e+1}, \ldots, m_{e+i+1}, n_0, n_{e+i}, \ldots, n_{\tau-1} \right\}
\]

\[
\xi(x_0, x_0, \ldots, x_0) \xi^k(x_{e+1}, x_{e+1}, \ldots, x_{e+i+1}) \xi^k(x_{\tau-1}, \ldots, x_{\tau-1})
\]

The mark (?) is used to denote \((-\ldots-\) raised to a power whose index is the number of exchanges of place whereby the arrangement 1, 2, \ldots, \( \tau - 1 \) can be shifted into the arrangement \( \theta_1, \theta_2, \ldots, \theta_{\tau-1} \).

In like manner, let

\[
K_2 = \Xi \left\{ l_0, l_0, \ldots, l_0; m_0, m_{e+1}, m_{e+1}, \ldots, m_{e+i+1}, m_{e+i+1}, \ldots, m_{\tau-1} \right\}
\]

\[
\xi(x_0, x_0, \ldots, x_0) \xi^k(x_{e+1}, x_{e+1}, \ldots, x_{e+i+1}) \xi^k(x_{\tau-1}, \ldots, x_{\tau-1})
\]

and

\[
K_3 = \Xi \left\{ l_0, l_0, \ldots, l_0; m_0, m_{e+1}, m_{e+1}, \ldots, m_{e+i+1}, m_{e+i+1}, \ldots, m_{\tau-1} \right\}
\]

\[
\xi(x_0, x_0, \ldots, x_0) \xi^k(x_{e+1}, x_{e+1}, \ldots, x_{e+i+1}) \xi^k(x_{\tau-1}, \ldots, x_{\tau-1})
\]

Then, using \( c \) to denote any arbitrary constant, we shall have
Mr. A. Cayley on the Porism of

\[ U = eK_1 \]
\[ V = (-)^e eK_2 \]
\[ W = (-)^e + i eK_3 ; \]

and so, in general, the ratios to one another of any number of functions of one variable, of which the linear conjunctives for a sufficient number of given values of the variable and of the coefficients of conjunction are known to vanish, may be expressed in terms of those values.

August 1853.

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LVIII. Correction of two Theorems relating to the Porism of the in-and-circumscribed Polygon. By A. Cayley, Esq.*

The two theorems in my "Note on the Porism of the in-and-circumscribed Polygon" (see August Number) are erroneous, the mistake arising from my having inadvertently assumed a wrong formula for the addition of elliptic integrals. The first of the two theorems (which, in fact, includes the other as a particular case) should be as follows:—

Theorem. The condition that there may be inscribed in the conic \( U = 0 \) an infinity of \( n \)-gons circumscribed about the conic \( V = 0, \) depends upon the development in ascending powers of \( \xi \) of the square root of the discriminant of \( \xi U + V ; \) viz. if this square root be

\[ A + B\xi + C\xi^2 + D\xi^3 + E\xi^4 + F\xi^5 + G\xi^6 + H\xi^7 + \ldots, \]

then for \( n = 3, 5, 7, \ldots \) respectively, the conditions are

\[
\begin{vmatrix}
C & D & E
\end{vmatrix} = 0, \\
\begin{vmatrix}
D & E & F
\end{vmatrix} = 0, \\
\begin{vmatrix}
E & F & G
\end{vmatrix} = 0, \text{ &c.} ;
\]

and for \( n = 4, 6, 8, \ldots \) respectively, the conditions are

\[
\begin{vmatrix}
D & E & F
\end{vmatrix} = 0, \\
\begin{vmatrix}
E & F & G
\end{vmatrix} = 0, \text{ &c.} .
\]

The examples require no correction; since for the triangle and the quadrilateral respectively, the conditions are (as in the erroneous theorem) \( C = 0, D = 0. \)

* Communicated by the Author.
The second theorem gives the condition in the case where the conics are replaced by the circles $x^2 + y^2 - R^2 = 0$ and $(x-a)^2 + y^2 - r^2 = 0$, the discriminant being in this case

$$-(1 + \xi)^2 \{ r^2 + \xi (r^2 + R^2 - a^2) + \xi^2 R^2 \}.$$ 

As a very simple example, suppose that the circles are concentric, or assume $a = 0$; the square root of the discriminant is here

$$(1 + \xi) \sqrt{r^2 + R^2} \xi;$$ 

and putting for shortness $\frac{R^2}{r^2} = \alpha$, we may write

$$A + B \xi + \ldots = (1 + \xi) \sqrt{1 + \alpha \xi},$$

i. e.

$$A = 1, \quad B = \frac{1}{2} \alpha + 1, \quad C = -\frac{1}{8} \alpha^2 + \frac{1}{2} \alpha, \quad D = \frac{1}{16} \alpha^3 - \frac{1}{8} \alpha^2,$$

$$E = -\frac{5}{128} \alpha^4 + \frac{1}{16} \alpha^3, \text{ &c.};$$

thus in the case of the pentagon,

$$CE - D^2 = \frac{1}{1024} \alpha^4 \{(\alpha - 4)(5\alpha - 8) - 4(\alpha - 2)^2\}$$

$$= \frac{1}{1024} \alpha^4(\alpha^2 - 12\alpha + 16),$$

and the required condition therefore is

$$\alpha^2 - 12\alpha + 16 = 0.$$ 

It is clear that, in the case in question,

$$\frac{r}{R} = \cos 36^\circ = \frac{\sqrt{5} + 1}{4};$$

i. e. $\frac{R}{r} = \sqrt{5} - 1$, or $(R + r)^2 - 5r^2 = 0$, viz. $(\sqrt{\alpha} + 1)^2 - 5 = 0$,

i. e. $\alpha + 2 \sqrt{\alpha} - 4 = 0$, the rational form of which is

$$\alpha^2 - 12\alpha + 16 = 0,$$

and we have thus a verification of the theorem for this particular case.

LIX. Proceedings of Learned Societies.
ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. v. p. 147.]

May 13, ON a new Solution of the Problem of Planetary Perturbation. By Professor Encke. (Extract of a Letter to the Astronomer Royal.)

"The subject of the small planets has continued to occupy my attention so much, that I could not communicate with you until I had relieved my mind of this load of anxiety. I consider myself very fortunate on this account in being enabled, through the meritorious services of my excellent friend Dr. Brünnow, to transmit to you in the annexed paper the mean elements of Flora, with the perturbations of the same planet as produced by Jupiter and Saturn. From the mode in which the calculation of these elements has been worked out, I am induced to entertain a confident hope that we have it now in our power to compute the perturbations of the first power of the masses for all the small planets, not even excepting Pallas, and that the time bestowed on the calculations is not immoderately long.

"The perturbations produced by Jupiter upon Flora, which possesses the advantage of being pretty distant from the disturbing planet, but which, on the other hand, has an excentricity of 9° and an inclination of 6°, will admit of being computed in four weeks without any immoderate haste; and even with respect to those of Pallas, I would pledge myself, without hesitation, to compute them for Jupiter and Saturn in the course of a winter, if nothing unusual interfered with the discharge of my ordinary duties.

"On a former occasion I had the honour of transmitting to you a copy of a paper which I communicated to the Academy, and in which I proposed an indirect method. This was not suitable to the object in view, as any one may easily convince himself, since the approximations conducted to the required result only in a few rare cases. Still the proposed method has not been useless, since it has given me the form which is most convenient for calculation, of the integral of the assigned differential equation which always reappears in the problem. By a mere simple consideration of the combinations which present themselves in this inquiry, the following solution has been obtained.

"If the differential equation has the form

\[ \frac{dd\xi}{dt^2} + \frac{k^2}{r^{03}} \xi = \Sigma (a_i^0 \cos (iM - i'M')), \]

in which the cosine may be exchanged for the sine, and M, M' denote the mean anomalies of the disturbed and disturbing planets, the mean motions of which are represented by \( \mu \) and \( \mu' \); then, in every case, will the coefficient of \( \cos (iM - i'M') \) or of \( \sin (iM - i'M') \) be thus determined by integration:
Let \( x^0 = r^0 \cos \nu^0 = p_0 + \Sigma (p_m \cos mM) \)

Also let \( \frac{1}{r_{\mu - i' \mu'}} = [r, s] \);

then the coefficient of \( \cos (iM - i'M') \) in the integral will have the following form:

\[
\frac{a_i'}{k \sqrt{p}} \left\{ [i+1, i-1] \frac{p_1 q_1}{2} + [i+2, i-2] \frac{p_2 q_2}{4} + [i+3, i-3] \frac{p_3 q_3}{4} + [i+4, i-4] \frac{p_4 q_4}{4} \right. \\
+ [i+5, i-5] \frac{p_5 q_5 + p_5 q_3}{4} + [i+6, i-6] \frac{p_6 q_6 + p_6 q_3}{4} ... \\
\left. + [i+7, i-7] \frac{p_7 q_7 + p_7 q_5}{4} + [i+8, i-8] \frac{p_8 q_8 + p_8 q_6}{4} \right\}
\]
This expression proceeds in an infinite series of terms, both in the vertical and horizontal directions. It, however, converges very quickly for any given values of $i$ and $i'$, so that in general only a very small number of coefficients require to be taken into consideration in order to determine the coefficient of the integral; and since, for the sine and cosine, the expression is merely a numerical quantity, when $\mu$ and $\mu'$ are given, so is the calculation for the very small number of values of $i$, when $i'$ is constant, by no means difficult.

Dr. Brünnnow has succeeded in avoiding the indirect solution by adding to the equation relative to the undisturbed orbit, viz.

$$\frac{1}{2} \frac{d^2(r^2)}{dt^2} = k^2 \left( \frac{1}{r} - \frac{1}{a} \right);$$

the equation for the disturbed orbit, viz.

$$\frac{1}{2} \frac{d^2(r^2)}{dt^2} = k^2 \left( \frac{1}{r} - \frac{1}{a} \right) + xX + yY + zZ + 2\int \left( \frac{dx}{dt} + \frac{dy}{dt} + \frac{dz}{dt} \right);$$

Whence, by comparison, there result the following rigorous equations:

$$\frac{1}{2} \frac{d^2(r^2 - r^2)}{dt^2} - \left( \frac{k^2}{r} - \frac{k^2}{r^2} \right) = xX + yY + zZ + 2\int \left( \frac{dx}{dt} + \frac{dy}{dt} + \frac{dz}{dt} \right),$$

$$\frac{d^2(x - x^0)}{dt^2} + \frac{k^2x}{r^2} - \frac{k^2x^0}{r^{10}} = X,$$

$$\frac{d^2(y - y^0)}{dt^2} + \frac{k^2y}{r^2} - \frac{k^2y^0}{r^{10}} = Y,$$

$$\frac{d^2(z - z^0)}{dt^2} + \frac{k^2z}{r^2} - \frac{k^2z^0}{r^{10}} = Z.$$

Let us now suppose

$$r = r^0 + \hat{r},$$

$$x = x^0 + \xi,$$

$$y = y^0 + \eta,$$

$$z = z^0 + \zeta,$$

and let us collect together the squares and products of $\hat{r}$, $\xi$, $\eta$ and $\zeta$. We hence deduce together these four equations:

$$\frac{dd(r^0 \hat{r})}{dt^2} + \frac{k^2}{r^{10}} (r^0 \hat{r}) = xX + yY + zZ + 2\int \left( \frac{dx}{dt} + \frac{dy}{dt} + \frac{dz}{dt} \right),$$

$$\frac{1}{2} \frac{dd(\hat{r})}{dt^2} + \frac{k^2}{r^{10}} (\hat{r})^2 - \frac{k^2}{r^{10}} (\hat{r})^3,$$

$$\frac{dd\xi}{dt^2} + \frac{k^2}{r^{10}} \xi = X + 3 \frac{k^2x^0}{r^{10}} (r^0 \hat{r}) - 6 \frac{k^2x^0}{r^{10}} (r^0 \hat{r})^2 + 3 \frac{k^2\xi}{r^{10}} (r^0 \hat{r})$$
\[
\frac{dd\eta}{dt^2} + \frac{k^2}{r^{0\alpha}} \eta = Y + \frac{3k^2y^9}{r^{0\alpha}} (r^0\delta r) - 6\frac{k^2y^8}{r^{0\alpha}} (r^0\delta r)^2 \\
+ 3\frac{k^2\eta}{r^{0\alpha}} (r^0\delta r) \\
\frac{dd\xi}{dt^2} + \frac{k^2}{r^{0\alpha}} \xi = Z + \frac{3k^2z^0}{r^{0\alpha}} (r^0\delta r) - 6\frac{k^2z^0}{r^{0\alpha}} (r^0\delta r)^2 \\
+ 3\frac{k^2\xi}{r^{0\alpha}} (r^0\delta r).
\]

"The first of these equations gives \( r^0\delta r \), and this substituted in the others gives \( \xi, \eta, \zeta \).

"These are essentially the first equations of the M"ecanique C"eleste.

"A second step due to Dr. Br"unnnow is, that he has executed the numerical calculations of all the multiplications of the prescribed infinite series. This multiplication admits of being made with convenience and accuracy; and strictly affords the advantage of being far shorter than the substitutions would be if the numerical values for \( e \) were determined by multiplications executed analytically. M. Hansen had already recommended this mode of executing the numerical calculation without having further recourse to any analytical process; but it has not, so far as I know, been so generally applied in practice.

"These are the fundamental principles of the solution. Dr. Br"unnnow, however, has not executed the calculations absolutely in this form, but has introduced the polar coordinates \( r^0 \) and \( \varphi^0 \), conformably to his method of treating the subject of special perturbations. It is quite possible, since the solution of the equations is thereby considerably simplified (\( r^0\delta r \) being already obtained by the first equation, and consequently there only remaining \( \delta \varphi \) and \( z \) to be determined), that the abbreviation is so considerable as to outweigh the disadvantages attending the somewhat less elegant symmetry of the final equations. In the meanwhile, being curious to know to what extent the expressions for \( \xi, \eta, \zeta \) would be found to be less convergent than \( \delta \varphi \), I have also begun the calculation by the formula here given, and hope to be soon prepared with it; should the difference be not too great, the simple mode of deducing the formulæ for \( \xi, \eta \) and \( \zeta \) will always captivate me. However, this is a mere matter of taste.

"We have both found the expression for the disturbing force by the actual development of \( \frac{1}{\rho^3} \) (\( \rho \) being the distance between the two planets), and then by multiplication have deduced the values of the resolved forces. We thereby derive the advantage of having fewer values of \( \rho^3 \) to calculate throughout the entire extent of the periphery. Perhaps it would be better to develope \( \frac{1}{\rho} \) into a series
Intelligence and Miscellaneous Articles.

according to powers of $M$ and $M'$, and then by two multiplications to find $\frac{1}{\rho^3}$.

"The terms of the second order admit of being easily obtained according to this method by means of Taylor's theorem, since the quantities $x, y, z$ are only involved, the perturbations of which are already known. But this process of calculation, which in every case is very troublesome, would be useless when the question refers to the small planets, since the elements of these bodies are not known with sufficient accuracy. Dr. Brünnow, by means of his osculating elements, has obtained pretty accurate values of the disturbing forces; and by applying the resulting perturbations has deduced the mean elements of the planet. To me it is as good as certain that the perturbations thus calculated (the hundredths of seconds are omitted) are sufficiently accurate to be employed next year for a more exact determination of the mean elements. The terms of the first power of the masses which are here given will therefore subserv the object in view, and on a future occasion the question may be investigated, whether the terms of the second order exercise any sensible influence.

"The perturbational values are all given as they follow from the calculations. Moreover, the secular equations of the elements which might be deduced from the terms involving $t$ (the unit of $t$ is the Julian year) are not deduced, since for the next year the calculation in its present form is more convenient. By the way, I see that I have not made any mention of these terms in speaking of the integration of the differential equations. However, what I have said above will be quite sufficient to give you an idea of this method of computing the perturbations."

"Berlin, April 24, 1853."

LX. Intelligence and Miscellaneous Articles.

ON THERMIC CONDUCTION IN METALS. BY G. GORE, ESQ.

The following experiments were made to ascertain if, in the case of thermic conduction between different metals, or between similar metals but of different sizes, the rapidity of conduction was affected by the direction of transmission; for instance, to ascertain if thermic conduction was more or less rapid when heat passed from an iron wire to a copper one, than when it passed from the copper to the iron; or from a small iron wire to a larger one than vice versa.

The arrangement adopted was, to form compound bars of the metals to be tested by taking equal lengths of each and soldering them end to end; then taking two such compound bars, each of similar metals, and after sloping one end of the dissimilar metals in each, soldering them together at those parts so as to form a double compound bar or triangle of an acute V form, each limb of which
is thus a compound bar, and presents dissimilar metals at its acute angle.

Now by fixing a little cocoa-nut tallow on the free extremity of each rod, and applying heat in a uniform manner to the acute junction, with the two compound bars in a horizontal position, the least difference in the rapidity of conduction in the two directions of the similar compound bars could be ascertained with satisfactory accuracy by the difference of time at which the tallow upon the two ends melted.

The source of heat was a very small and steady flame of a spirit lamp carefully protected from all currents of air, and the acute junction of the double pair was just in contact with the flame in such a manner as to apply the heat equally and uniformly to the pointed extremity of each pair.

In nearly every case the experiments were repeated to ensure correct results, and in those cases where the results were less certain, rods of different diameters and lengths were tried in order to render the effects more conspicuous: the rods varied from $\frac{1}{4}$-inch to $\frac{3}{8}$-inch, and the following were the results obtained:

Experiment 1. In this experiment each arm of the pair was composed of brass and nickel-silver wires, each metal being 3 inches long and $\frac{1}{10}$-inch in diameter; on applying heat to the junction for a short time, the tallow upon the nickel-silver extremity melted about 10 or 12 seconds before that upon the brass end, indicating that the heat conduction was more rapid from brass to nickel-silver than vice versa: this experiment was repeated with similar results.

Exp. 2. Wires of copper and nickel-silver, each being 3 inches long and $\frac{1}{10}$-inch in diameter, the conduction of heat was more rapid from copper to nickel-silver than vice versa by 10 or 15 seconds: repeated with like results.

Exp. 3. Iron and nickel-silver wires 3 inches long and $\frac{1}{8}$-inch in diameter, heat travelled faster from iron to nickel-silver by 2 or 3 seconds: repeated with wires 2 inches long and $\frac{1}{10}$-inch in diameter with similar results.

Exp. 4. Wires of nickel-silver and lead, each 2 inches long and $\frac{1}{8}$-inch in diameter, heat conduction faster from nickel-silver to lead than vice versa by 6 or 8 seconds: repeated several times with similar results.

Exp. 5. Zinc and nickel-silver wires, each 2$\frac{1}{2}$ inches long and $\frac{1}{10}$-inch in diameter, conduction of heat faster from zinc to nickel-silver than vice versa by about 60 seconds: repeated with wires 2 inches long with similar results.

Exp. 6. Wires of nickel-silver alone but of different diameters, 3 inches long and $\frac{1}{8}$-inch and $\frac{1}{4}$-inch in diameters, heat travelled faster from the large to the smaller wire than vice versa by 10 or 12 seconds: repeated several times with similar results; also with wires of $\frac{1}{10}$-inch and $\frac{1}{12}$-inch in diameters and 2$\frac{1}{2}$ inches long, several times with similar results.
Exp. 7. Copper and brass wires, each 3 inches long and \( \frac{1}{8} \)th of an inch in diameter, more rapid conduction from copper to brass than vice versa by 6 or 8 seconds.

Exp. 8. Wires of brass and iron, each 3 inches long and \( \frac{1}{8} \)th of an inch in diameter, quickest transmission of heat was from brass to iron by 5 or 8 seconds: repeated with similar results.

Exp. 9. Brass and lead wires, each 3 inches long and \( \frac{1}{8} \)th of an inch in diameter, the quickest heat conduction was from brass to lead by about 50 or 60 seconds.

Exp. 10. Wires of zinc and brass, each \( 2\frac{1}{2} \) inches long and \( \frac{1}{16} \)th of an inch in diameter, fastest heat conduction was from zinc to brass by 2 or 3 seconds: similar results obtained when repeated with wires 2 inches long and \( \frac{1}{16} \)th of an inch in diameter.

Exp. 11. Large and small brass wires, 3 inches long and \( \frac{1}{4} \)th and \( \frac{1}{10} \)th of an inch in diameters, the quickest transmission of heat was from the larger to the smaller wire by 5 or 8 seconds: repeated several times with similar results.

Exp. 12. Copper and iron wires, each 3 inches long and \( \frac{1}{10} \)th of an inch in diameter, the conduction of heat was more rapid from copper to iron than vice versa by 10 or 15 seconds: repeated with similar results.

Exp. 13. Copper and lead wires, each 2 inches long and \( \frac{1}{16} \)th of an inch in diameter, the conduction of heat was fastest from copper to lead by 20 or 25 seconds: repeated with similar results.

Exp. 14. Zinc and copper wires, each \( 2\frac{1}{2} \) inches long and \( \frac{1}{10} \)th of an inch in diameter, the conduction of heat was more rapid from copper to zinc than vice versa by 8 or 10 seconds.

Exp. 15. Large and small copper wires, 3 inches long and \( \frac{1}{8} \)th and \( \frac{1}{4} \)th of an inch in diameters, the quickest transmission of heat was from the large to the small wire by 8 or 10 seconds: repeated several times with similar results.

Exp. 16. Iron and lead wires, each 3 inches long and \( \frac{1}{8} \)th of an inch in diameter, the fastest conduction of heat was from iron to lead by 20 or 30 seconds.

Exp. 17. Zinc and iron wires, each \( 2\frac{1}{2} \) inches long and \( \frac{1}{10} \)th of an inch in diameter, the most rapid transmission of heat was from zinc to iron by 12 or 15 seconds: repeated with wires 2 inches long with similar results.

Exp. 18. Large and small iron wires, 3 inches long and \( \frac{1}{8} \)th and \( \frac{1}{4} \)th of an inch in diameters, the fastest conduction of heat was from the large to the small wire by 15 or 20 seconds: repeated with wires 3 inches long and \( \frac{1}{10} \)th and \( \frac{1}{8} \)th of an inch in diameters with similar effects.

Exp. 19. Zinc and lead wires, each \( 1\frac{1}{2} \) inch long and \( \frac{1}{16} \)th of an inch in diameter, the quickest conduction of heat was from zinc to lead by 30 or 40 seconds: repeated with similar effects.

Exp. 20. Large and small zinc wires, \( 2\frac{1}{2} \) inches long and \( \frac{1}{10} \)th and \( \frac{1}{16} \)th of an inch in diameters, the quickest conduction of heat was from the large to the small wire by 6 or 8 seconds.

Taking the following series of metals, viz. copper, brass, iron, zinc,
lead, and nickel-silver, as the order of their relative degrees of heat conductivity (which I have determined by experiments), and comparing the results of the foregoing experiments with the relative conductibility of the corresponding metals for heat, we find that in every experiment, excepting those of numbers 4, 10 and 17, heat travelled more rapidly from the better heat conductor to the worse than vice versa. Also taking the following series, viz. copper, zinc, brass, iron, lead, and nickel-silver as the order of their relative degrees of electric conductibility, and comparing them in like manner, we find that in every experiment except that of number 4, heat travelled more rapidly from the better electrical conductor to the worse than vice versa. Also in every case where wires of the same metal but of different diameters composed the arrangement, as in Exps. 6, 11, 15, 18 and 20, heat travelled more rapidly from the larger to the smaller wire than vice versa.

Birmingham.

RESEARCHES ON EVAPORATION. BY PROFESSOR MARCET OF GENEVA.

The following experiments were instituted with the view of throwing some light on the tendency of certain circumstances to promote or diminish the evaporation of liquids. Water and alcohol were the liquids chiefly used. The results obtained by the author may be recapitulated as follows:

1. The temperature of a liquid, allowed to evaporate freely in an open vessel, is always inferior to that of the surrounding atmosphere. The higher the temperature of the atmosphere, the greater is the difference between its temperature and that of the liquid exposed to evaporation. Between 40° and 50° Centigrade the difference was found to vary from 5° to 7°; between 20° and 25° it varied from 1°3 to 1°4; at 12° it was 0°8 only, and between 3° and zero about 0°2. The explanation of this result is obvious. The evaporation of a liquid diminishing with the external temperature, the cold, which is the consequence of this evaporation, must diminish in the same proportion; and if it were possible to prevent evaporation altogether, the author presumes that there would be no difference whatever between the temperature of a liquid and that of the surrounding medium.

2. The temperature of liquids, such as water and alcohol, as well as the rapidity with which they evaporate, varies, all other circumstances remaining the same, according to the nature of the vessel in which these liquids are contained. For instance, the temperature of the surrounding atmosphere being from 15° to 20°, water is on the average 0°3 warmer in an open metallic vessel than in a similar one of polished porcelain, and 0°2 warmer than in a similar one of glass. It is the same with alcohol. Again, both water and alcohol evaporate more rapidly from a porcelain vessel than from a metallic or glass vessel of precisely the same size. For example: three similar vessels, one of metal, the second of porcelain, and the third of glass,
containing each 600 grains of water, having been exposed to evaporation during seven days, the temperature of the surrounding atmosphere varying from 20° to 25°, it was found, that at the end of that time the porcelain vessel had lost 303 grains of its previous weight, the metallic one 277, and the glass vessel 275·5 grains only. The author enters into considerable detail as to the precautions he took to make sure that these differences could not be attributed to any difference in the radiating or conducting powers of the vessels employed.

The differences observed in the temperature of liquids according to the nature of the vessels in which they are contained, depends, no doubt, on the property with which these vessels appear to be endowed, of accelerating or delaying evaporation. It is evident that in each case the quantity of sensible heat subtracted from the liquid, or, in other words, the diminution of its temperature, must be in proportion to the quantity of vapour formed. For instance, the fact that water and alcohol are constantly colder in a porcelain vessel than in a similar vessel of metal or glass, is the natural result of the more rapid evaporation of these liquids from the former of these vessels than from the latter. The reason why a porcelain vessel evaporates more freely than a metallic or glass one is far less evident. The author has proved, by placing a hermetically-closed bottle of porcelain containing water under the vacuum of the air-pump, that it cannot be owing to any perviousness of the sides of the vessel, as he was at first inclined to suspect.

3. The influence of the mass or depth of a liquid was next examined. The author's experiments appear to lead to the curious fact, that the rapidity with which any given liquid evaporates depends not only on the extent of its surface, but also, within certain limits, on its depth. He found, for instance, that with two similar cylindrical porcelain vessels, containing; the first a layer of water of one-twelfth of an inch in depth, and the second a layer of half an inch, the evaporation from the latter exceeded that of the former in the proportion of nearly 4 to 3. A similar result was obtained with alcohol. If thin glass vessels were used, the same increase of depth accelerated the evaporation in the proportion of 6 to 5. As the author himself observes, this apparent influence of the depth of a liquid on its evaporation, may very possibly be merely owing to the greater facility with which the different layers are conveyed one after the other to the surface, when the liquid is of a certain depth than when it is quite shallow.

4. Water containing a solution of salt in about the same proportion as sea-water, evaporates less rapidly, and consequently produces less cold than the same quantity of distilled water. The higher the temperature of the surrounding atmosphere, the greater the difference between the quantities of salt and fresh water evaporated in a given time under similar circumstances.

5. A given quantity of water, mixed with certain pulverulent substances, such as siliceous sand, for the particles of which it has but a slight adhesion, evaporates more rapidly than the same quan-
Intelligence and Miscellaneous Articles.

6. The last result which we shall mention, and which may be regarded as a direct consequence of the preceding one, is the following:—Water mixed with sand remains habitually at a slightly lower temperature than an equal surface of water standing alone. The difference varies to a certain extent, according to the nature of the vessels in which the experiment is performed, never however exceeding \( \frac{1}{2} \) a degree Centigrade. It is greater, when the comparison is made between water and wet sand placed in two similar metallic vessels, than when they are placed in porcelain or glass vessels; in the latter case it seldom exceeds \( 0^\circ.1 \) to \( 0^\circ.2 \).

The author concludes by remarking, that the foregoing result tends to confirm an opinion expressed some time since by Professor De la Rive, in a letter to M. Arago, published in the Comptes Rendus de l'Académie des Sciences for October 1851. In this letter, M. De la Rive attributes the sudden appearance of vast glaciers in divers parts of Europe to a temporary refrigeration produced at the period of the elevation of the most recent European strata, by the evaporation of the water with which they were previously covered. If, as the author's experiments tend to show, evaporation takes place more rapidly from water mixed with sand, earth, or any similar substance, than from a surface of clear water, it becomes natural to conclude, that the cold produced by evaporation from the recently elevated and still humid strata, must have been greater than that resulting from the evaporation of the sea or freshwater lake which covered them previously to a great depth.—Bibliothèque Universelle, April 1853.

ON AN UNUSUAL LUMINOUS APPEARANCE IN THE SKY.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Sidmouth, Oct. 19, 1853.

I have been favoured by a lady (Mrs. Bennett) with some account of a meteoric appearance in the sky, which she witnessed on the evening of the 2nd of September last at Exeter. I am induced to
forward this account, because this same phenomenon, I learn from the Illustrated News (date 24th September), was also seen at Dublin, York and Huddersfield, and is considered by the editor as an unusual phase of the Aurora.

My correspondent says, her attention was called to the meteor at Exeter about 10 o'clock P.M. of the 2nd of September. "In appearance it resembled a thin, luminous, white cloud, more opaque than the Aurora, but allowing stars to be seen through it; in shape, like the tail of a comet; in length, by estimation, about 15°, and in breadth about 4° at the broadest part. The direction was vertical, occasionally inclining (bowed out as it were) towards the south. The situation, between Arcturus Bootes and the Milky Way, nearly in the west. The meteor was nearly stationary, as we saw from a star of the third magnitude, which at first looked like its nucleus, and then gradually left it. The stationary position was also shown by a tree below. The light was mild. It hardly flashed, but rather heaved or throbbed, if such terms can be used in reference to light. Sometimes it beamed upwards to a great height, and once it appeared to separate at the lower part, still preserving its former appearance, and the higher becoming elongated. I watched it about three quarters of an hour, when it paled, and at five minutes to 11 faded away; but even then there seemed to be a slight mist diffused over the sky in the west, which had not quite disappeared at 10 minutes past 11.—A. R. Bennett."

To the above I would add, that my attention was directed to the same appearance here at Sidmouth at 9 o'clock, and that I observed it, at intervals, till about 15 minutes to 11. The meteor was of a brilliant white colour; at one time the upper part divided; a star also formed for a length of time an apparent nucleus. In length it was about 10° or 12°; its lower end was elevated about 10° or 11° above the horizon, as I afterwards ascertained from stationary objects, but roughly, with a common quadrant. Its direction was about 50° west of magnetic north.

I am, Sir,
Respectfully yours,
N. S. Heineken.

P.S. I have been informed that the same meteor was observed in Cheshire about 10, that it was very bright and stationary.

ON THE EMPLOYMENT OF HEAT, SULPHURIC ACID, AND DEUTEROXIDE OF MANGANESE IN BUNSEN'S BATTERY, AND ON A MEANS OF GREATLY DIMINISHING THE COST OF NITRIC ACID FOR THIS BATTERY. BY F. P. LEROUX.

M. Guignet has recently stated* that the nitric acid of Bunsen's pile might be replaced, at ordinary temperatures, by a mixture of

* Comptes Rendus.
sulphuric acid and deutoxide of manganese. The author employed several coke elements cut from the same piece in order that the porosity and conducting power might be the same; he had as many porous cells, and introduced the pieces of coke successively into the same Bunsen's element, the carbon being inside, and the surrounding zinc perfectly amalgamated; he took care to operate with acidulated water of a constant strength. With these precautions he ascertained—

1. That the effect of deutoxide of manganese mixed with concentrated sulphuric acid and employed at ordinary temperatures, is not to be compared with that of nitric acid, and that it does not sensibly increase the production of electricity due to the solution of the zinc; the sulphuric acid contained in the porous cell acts merely as a conductor. The author has verified the latter fact by employing alternately a cell filled with the mixture and one containing only sulphuric acid.

2. When the mixture has been left several hours, it is then put into the pile; a current almost as energetic as that produced by nitric acid is observed just at first; but the intensity of this current rapidly decreases, and in ten minutes, or a quarter of an hour at the utmost, it is as weak as already stated.

This arises from the mixture disengaging oxygen only very slowly at ordinary temperatures; when left to itself, the oxygen accumulates, so that in this case the apparatus acts like a gas battery; but as soon as all the free oxygen is consumed, the production of electricity is only supported by that which is slowly disengaged.

3. From this it appears, that if the disengagement of oxygen could be increased, the current produced would be much more energetic. This is most simply effected by an elevation of temperature, which the author proposes to produce by placing his battery in a vessel of boiling water; a thermometer placed in the battery shows the temperature. He found that towards 140° F. the intensity of the current increased very rapidly, and that the maximum was at about 167° F. Some bubbles of oxygen are disengaged from the mixture. The current thus obtained is more energetic than that produced by nitric acid. It is consequently only by an elevation of temperature that this mixture can furnish a sufficient quantity of electricity; it may be observed also, that, in proportion to the augmentation of temperature, the conductibility of the fluids diminishes the internal resistance.

This process is only applicable on a large scale. The author proposes the following as more convenient. Starting with the supposition that M. Guignet's experiments were made with coke cylinders which had already been used, and to which, in consequence, some nitric acid still adhered, he soaked a cylinder in nitric acid and then plunged it into a cell filled with concentrated sulphuric acid. The current produced was more energetic than with pure nitric acid. The advantage to be obtained by the employment of sulphuric acid results, consequently, from the fact, that by its avidity for water it
Intelligence and Miscellaneous Articles.

dehydrates the nitric acid so as to render it less stable and more fit to give up its oxygen. The nitric acid employed in batteries becomes too weak for use when it has reached a certain state of dilution; by means of sulphuric acid the last atom of actual nitric acid may be made use of. Sulphuric acid can readily dehydrate about its volume of nitric acid, added to it in small quantities as required. It is to be observed that this sulphuric acid is not lost; it may be employed for the solution of the zinc of the pile, taking care to boil it for a few moments so as to drive off any portions of nitric acid, the presence of which might dissolve the mercury and destroy the amalgamation.—Comptes Rendus, August 29, 1853, p. 355.

ANALYSIS OF AURIFEROUS QUARTZ. BY A. B. NORTHCOTE, ASSISTANT AT THE ROYAL COLLEGE OF CHEMISTRY.

The specimen, the analysis of which is detailed below, was consigned to England by an Australian merchant among many other pieces which came to him in the way of business, and nothing unfortunately is therefore known respecting its origin.

Its aspect was decidedly waterworn, and as regarded the quantity of precious metal present, not at the first glance very promising. Quartz was the predominating feature, with small nodular masses of gold dispersed here and there over the surface. On breaking the lump, however, a very different appearance presented itself; these masses were found to extend inwards, and, assuming the character of veins, to run rather extensively throughout the mineral; and so much richer did it prove than was expected, that the gold separated constituted about half the weight of the original specimen. The whole of the gold existing in it was contained in these large veins, there being no finely divided particles disseminated through the substance of the associated bodies.

The matrix consisted almost entirely of quartz, with a little carbonate of lime and a trace of alumina; but probably the true matrix was quartz alone, the other matters being merely lodgements in the crevices of the rock,—impurities contracted at some period subsequent to its formation.

The analysis of the contained metal, conducted in the ordinary way, the gold being reduced by means of oxalic acid, gave the following result in 100 parts:

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<th>Quantity</th>
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<td>.008</td>
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\[ \text{Total} = 100.000 \]
DR. MARTIN BARRY'S CONTRIBUTIONS TO EMBRYOLOGY.

The last Number of this Journal contains the abstract of a Note n Embryology by Dr. Martin Barry, who requests us to correct an error in it. Dr. Keber is there stated to have considered certain vesicles containing a revolving mulberry-like body as fecundated ova. This was not Dr. Keber's opinion. He believed the vesicles in question not to have been fecundated, and Dr. Barry agrees with him. [The membrane of those vesicles, however, Dr. Barry believes to have been his "ovisac," and not the vitellary membrane (zona pellucida) which had probably been absorbed.]

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1853.


Mean temperature of the month ........................................ 55°45
Mean temperature of Sept. 1852 ........................................ 56.21
Mean temperature of Sept. for the last twenty-seven years . 57.04
Average amount of rain in Sept. .................................... 2.57 inches.


Mean temperature of Sept. for twenty-six previous years ... 52°26
Mean temperature of this month ...................................... 53.28
Mean temperature of Sept. 1852 ...................................... 53.45
Average quantity of rain in Sept. for thirteen previous years 2.75 inches.
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Mean. | 29'973 | 29'877 | 29'47 | 29'805 | 29'798 | 65'00 | 45'90 | 55'2 | 54'93 | 51'63 | 2'41 | 1'50 | 3'54 |
LXI. On the Metallic Reflexion exhibited by certain Non-metallic Substances. By Professor G. G. Stokes, M.A., F.R.S.*

In the October Number of the Philosophical Magazine is a translation of a paper by M. Haidinger of Vienna, containing an account of his observations relating to the optical properties of Herapathite. In this paper he refers to a communication which I made to the British Association at the meeting at Belfast; and indeed one great object of his examination of this salt was to see whether a law which he had discovered, and already extensively verified, relating to the connexion between the reflected and transmitted tints of bodies which have the property of reflecting a different tint from that which they transmit, would be verified in this case. The report of my communication published in the Abbé Moigno's Cosmos† had led him to suppose that my observations were at variance with his law.

My attention was first directed to this subject while engaged in some observations on safflower-red (carthamine), which I was led to examine with reference to its fluorescence. In following out the connexion which I had observed to exist between the absorbing power of a medium and its fluorescence, I was induced to notice particularly the composition of the light transmitted by the powder; and I found that the medium, while it acted powerfully on all the more refrangible rays of the visible spectrum, absorbed green light with remarkable energy. I need not now describe the mode of absorption more particularly. During these experiments I was struck with

* Communicated by the Author.
† Vol. i. p. 574.

the metallic yellowish-green reflexion which this substance exhibits. It occurred to me that the almost metallic opacity of the medium with respect to green light was connected with the reflexion of a greenish light with a metallic aspect. I found, in fact, that the medium agreed with a metal in causing a retardation in the phase of vibration of light polarized perpendicularly to the plane of incidence relatively to light polarized in that plane. The observation was made by reflecting light polarized at an azimuth of about 45° from the surface of the medium to be examined, the angle of incidence being considerable, about equal to the angle of maximum polarization, and viewing the reflected light through a Nicol’s prism capped by a plate of calcareous spar cut perpendicularly to the axis. Now by using different absorbing media in succession, it was found that with red light, for which safflower-red is comparatively transparent, the reflected light was sensibly plane-polarized, while for green and blue light the ellipticity was very considerable.

In the case of a transparent medium, light would be polarized by reflexion, or at least very nearly so, at a proper angle of incidence. Hence if the light reflected by such a medium as safflower-red were decomposed into two pencils, one, which for distinction’s sake may be called the ordinary, polarized in the plane of incidence, and the other, or extraordinary, polarized perpendicularly to the plane of incidence, the extraordinary pencil would vanish at the polarizing angle, except in so far as the laws of the reflexion deviate from those belonging to a transparent substance. Hence the light remaining in the extraordinary pencil might be expected to be more distinctly related to the light absorbed with such energy. Accordingly, it was found that under these circumstances the extraordinary pencil (in the case of safflower-red) was of a very rich green colour, whereas without analysis the light was of a yellowish-green colour. Similar observations were extended to specular iron.

These phenomena recalled to my mind a communication which Sir David Brewster made at the meeting of the British Association at Southampton in 1846*; and on referring to his paper, I found that the appearance of differently coloured ordinary and extraordinary pencils in the light reflected by safflower-red was the same phenomenon as he has there described with reference to chrysammate of potash.

The observations above-mentioned were made towards the end of 1851. Accordingly, when Dr. Herapath’s first paper on the

* Notice of a new property of light exhibited in the action of chrysammate of potash upon common and polarized light. (Transactions of the Sections, p. 7.)
new salt of quinine appeared*, I was prepared to connect the metallic green reflected light with an intense absorbing action with respect to green rays. Having prepared some crystals according to his directions, I was readily able to trace the progress of the absorption in the case of light polarized in a plane perpendicular to what is usually the longest dimension of the crystalline plates, and to observe how the light passed from red to black as the thickness increased. Even the thickest of these crystals was so thin as to show hardly any colour by light polarized in the plane of the length. The result of crossing two such plates is of course obvious to any one who is conversant with physical optics. The intense absorption was readily connected with the metallic reflexion. An oral account of these observations was given at a meeting of the Cambridge Philosophical Society on March 15, 1852; but it was not till the observations were a second time described, with some slight additions, at the meeting of the British Association at Belfast, that any account of them was published. A notice of this communication appeared in the *Atheneum* of September 25, 1852, and from this the report in *Cosmos* seems to be taken, though the latter is not free from errors. In the report in the *Atheneum*, the colour of the more rapidly absorbed pencil is briefly described in these words: "But with respect to light polarized in the principal plane of the breadth, the thicker crystals are perfectly black, the thinner ones only transmitting light, which is of a deep red colour." The comparative transparency of the crystals with regard to red light is afterwards expressly connected with the green colour of the light reflected as if by a metal. But in the report in *Cosmos*, the passage just quoted is replaced by "tandis que pour le cas de la lumière polarisée dans le plan principal de la largeur ils sont opaques et noirs, quelque minces qu’ils soient d’ailleurs." This error led M. Haidinger to suppose that my observations were opposed to his law; whereas the fact is, that, without knowing at the time what he had done, I had been led independently to a similar conclusion.

In mentioning my own observations on safflower-red, Herapathite, &c., nothing is further from my wish than to neglect the priority of those to whom priority belongs. M. Haidinger had several years before discovered the phænomenon of the reflexion of differently coloured oppositely polarized pencils, which Sir David Brewster shortly afterwards, and independently, discovered in the case of chrysammate of potash. M. Haidinger had from the first observed a most important character of the phænomenon in the case of many crystals, namely, the orientation of the polarization of the reflected light, which Sir David Brewster does.

* Phil. Mag. for March 1852 (vol. iii. p. 161).
not seem to have noticed in the case of chrysommate of potash, and which perhaps was not very evident in that salt. In a later paper M. Haidinger had announced the complementary relation of the reflected and transmitted tints*. There is nothing new in employing the rings of calcareous spar as a means of detecting elliptic polarization; and the property of producing elliptic polarization in reflecting plane-polarized light had previously been observed in substances even of vegetable origin†. I am not aware, however, that the chromatic variations of the change of phase had been experimentally connected with the chromatic variations of an intense absorbing action on the part of the medium. I have hitherto mentioned but one instance of this connexion, but I shall presently have occasion to allude to another.

I think it but justice to myself to point out the error in Cosmos (from whence M. Haidinger derived his information respecting my observations), in consequence of which I would appear to have been guilty of a grievous oversight in the examination of Herapathite: but I would hardly have ventured to mention my observations on carthamine, &c., were it not that, when different persons arrive independently at a similar conclusion, it frequently happens that views present themselves to the mind of one which may not have occurred to another. In the present case, in stating in detail my own views as to the nature of the phenomenon,

* In a paper of M. Haidinger's, entitled "Über den Zusammenhang der Körperfärben, oder des farbig durchgelassenen, und der Oberflächenfarben, oder des zurückgeworfenen Lichtes gewisser Körper," from the Jamuary Number of the Proceedings of the Mathematical and Physical Class of the Academy of Sciences at Vienna for the year 1852, will be found a list of M. Haidinger's previous papers on this subject. This paper contains a methodized account of the properties, with reference to surface and substance colour, of the substances up to that time examined by the author, amounting in number to thirty. For a copy of this, as well as several others of his papers, I am indebted to the kindness of the author.

† More than twenty years ago Sir David Brewster, in his well-known paper "On the Phenomena and Laws of Elliptic Polarization, as exhibited in the action of Metals upon Light," pointed out the modification produced on the rings of calcareous spar as a character of polarized light after reflexion from a metal. (Phil. Trans. for 1830, p. 291.) In a communication to the British Association at the meeting at Southampton in 1846, Mr. Dale mentions indigo among a set of substances in which he had detected elliptic polarization by means of the rings of calcareous spar. In this case, however, he connects the property, not with the intense absorbing power of the substance, but with its high refractive index.

I do not here mention the minute degrees of ellipticity which have been detected in polarized light reflected from transparent substances in general by the delicate researches of M. Jamin, partly because they are so small as to be widely separated from the ellipticity in the case of carthamine, &c., partly because they seem to have no relation to the present subject.
I hope to be able to add something to what has been already done by M. Haidinger and Sir David Brewster, and it seemed not out of place to mention the observations in which those views originated.

It appears, then, that certain substances, many of them of vegetable origin, have the property of reflecting (not scattering) light which is coloured and has a metallic aspect. The substances here referred to are observed to possess an exceedingly intense absorbing action with respect to rays of the refrangibilities of those which constitute the light thus reflected, so that for these rays the opacity of the substances is comparable with that of metals. Contrary, however, to what takes place in the case of metals, this intense absorbing action does not usually extend to all the colours of the spectrum, but is subject to chromatic variations, in some cases very rapid. The aspect of the reflected light, which itself alone would form but an uncertain indication, is not the only nor the principal character which distinguishes these substances. In the case of transparent substances, or those of which the absorbing power is not extremely intense (for example, coloured glasses, solutions, &c.), the reflected light vanishes, or almost entirely vanishes, at a certain angle of incidence, when it is analysed so as to retain only light polarized perpendicularly to the plane of incidence*, which is not the case with metals. In the case of the substances at present considered, the reflected light does not vanish, but at a considerable angle of incidence the pencil polarized perpendicularly to the plane of incidence becomes usually of a richer colour, in consequence of the removal, in a great measure, of that portion of the reflected light which is independent of the metallic properties of the medium; it commonly becomes, also, more strictly related to that light which is absorbed with such great intensity. The reflexion from a transparent medium is weakened or destroyed by bringing the medium into optical contact with another having nearly or exactly the same refractive index. Accordingly, in the case of these optically metallic substances, the colours which they reflect by virtue of their metallicity† are brought out by putting the medium in optical contact with glass or water. A remarkable

* I do not here take into account the peculiar phenomena which have been observed by Sir David Brewster with reference to the influence of the double refraction of a medium (such as calcareous spar) on the polarization of the reflected light, which, indeed, are not very conspicuous unless the medium be placed in optical contact with a fluid having nearly the same refractive index.

† I use this word to signify the assemblage of optical properties by which a metal differs from a transparent medium, or one moderately coloured, such as a coloured glass.
character of metallic reflexion consists in the circumstance, that as the angle of incidence increases from $0^\circ$, the phase of vibration of light polarized in the plane of incidence is accelerated relatively to that of light polarized in the perpendicular plane. Accordingly, the same change takes place, with the same sign, in the case of these optically metallic substances; but the amount of the change is subject to most material chromatic variations, being considerable for those colours which are absorbed with great energy, but insensible for those colours for which the medium is comparatively transparent, so that the absorption may be neglected which is produced by a stratum of the medium having a thickness amounting to a small multiple of the length of a wave of light. If the medium be crystallized, it may happen that one only of the oppositely polarized pencils which it transmits suffers, with respect to certain colours, an exceedingly intense absorption; or, if that is the case with both pencils, that the colours so absorbed are different. It may happen, likewise, that the absorption varies with the direction of the ray within the crystal. In such cases the light reflected by virtue of the metallicity of the medium will be subject to corresponding variations, so that the medium is to be regarded as not only doubly refracting and doubly absorbing, but doubly metallic.

The views which I have just explained are derived from a combination of certain theoretical notions with some experiments. They have need of being much more extensively verified by experiment; but, so far as I at present know, they are in conformity with observation.

To illustrate the effect of bringing a transparent medium into optical contact with an optically metallic substance, I may refer to safflower-red. If a portion of this substance be deposited on glass by means of water, and the water be allowed to evaporate, a film is obtained which reflects on the upper surface a yellowish-green light, but on the surface of contact with the glass a very fine green inclining to blue. A green of the latter tint appears to be more truly related to the colours absorbed with greatest energy. Similar remarks apply to the light reflected by Herapathite, according as the crystals are in air or in the mother-liquor. If a small portion of Quadratite (platino-cyanide of magnesium) be dissolved on glass in a drop or two of water, and the fluid be allowed to evaporate, the tints reflected by the upper and under surfaces of the film of crystals are related to one another much in the same way as in the case of safflower-red. For a fine specimen of the salt last mentioned I am indebted to the kindness of M. Haidinger. I may mention in passing, that the platinocyanides as a class are of extreme optical interest. The crystals are generally at the same time doubly refracting,
exhibited by certain Non-metallic Substances.

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doubly absorbing, doubly metallic, and doubly fluorescent. By the last expression I mean that the fluorescence, which the crystals generally exhibit in an eminent degree, is related to directions fixed relatively to the crystal, and to the azimuths of the planes of polarization of the incident and emitted rays.

M. Haidinger has expressed the relation between the surface and substance colours of bodies by saying that they are complementary. This expression was probably not intended to be rigorously exact; and that it cannot be so, is shown by the following simple consideration. The tint of the light transmitted across a stratum of a given substance almost always, if not always, varies more or less according to the thickness of the stratum. Now one and the same tint, namely, that of the reflected light, cannot be rigorously complementary to an infinite variety of tints of different shades, namely, those of the light transmitted across strata of different thickness. In most cases, indeed, the variation of tint may not be so great as to prevent us from regarding the reflected and transmitted tints in a general sense as complementary. But as media exist (for example, salts of sesquioxide of chromium, solutions of chlorophyll) which change their tint in a remarkable manner according to the thickness of the stratum through which the light has to pass, it is probable that instances may yet be observed in which M. Haidinger's law would appear at first sight to be violated, although in reality, when understood in the proper sense, it would be found to be obeyed. As the existence of surface-colour seems necessarily to imply a very intense absorption of those rays which are reflected according to the laws which belong to metals, it follows that it is in the very thinnest crystals or films of those which it is commonly practically possible to procure, that the transmitted tint is to be sought which is most properly complementary to the tint of the reflected light.

I will here mention another instance of the connexion between metallic reflexion and intense absorption. I choose this instance because a different explanation from that which I am about to offer has been given of a certain phenomenon observed in the substance. The instance I allude to is specular iron. As it is already known that various metallic oxides and sulphures possess the optical properties of metals, there is nothing new in bringing forward this particular mineral as a substance of that kind. It is to the chromatic variation of the metallicity that I wish to direct attention. If light polarized at an azimuth of about 45° be reflected from a scale of this substance at about the polarizing angle, and the reflected light be viewed through a plate of calcareous spar and a Nicol's prism, it will be found, by using different absorbing media in succession, that the change of phase, as
indicated by the character of the rings, while it is very evident for red light, becomes much more considerable in the highly refrangible colours. Now specular iron is almost opaque for light of all colours, but as it gives a red streak it appears that the substance-colour is red; and, in fact, it is known that very thin laminae are blood-red by transmitted light. Accordingly, the chromatic variation of the change of phase corresponds to that of the intense absorbing power.

The light reflected by specular iron is not extinguished by analysation, whatever be the angle of incidence; but at the angle of incidence which gives the nearest approach to complete polarization, a quantity of blue light is observed to remain. This has been explained by comparing specular iron to a substance of high dispersive power, so that the polarizing angle for red light is considerably less than for blue; and accordingly on increasing the angle of incidence, the light (which is here supposed to be analysed so as to retain only the portion polarized perpendicularly to the plane of incidence), while it becomes much less copious near the polarizing angle, becomes at the same time of a decided blue colour*. I believe, however, that the blue light is mainly due to the chromatic variation of the metallicity, the medium, considered optically, being much more metallic for blue light than for red, though it may in some measure be due to the cause previously assigned.

Specular iron is a good example of a substance forming a connecting link between the true metals and substances like safflower-red. It resembles metals in the circumstance that the absorbing power, as inferred from the chromatic variation of the metallicity, and as indicated by the tint of the streak, is not subject to the same extensive chromatic variations as in the case of colouring matters like safflower-red. It resembles safflower-red in being sufficiently transparent with respect to a portion of the spectrum to allow the connexion between the metallicity and the substance-colour to be observed; whereas the substance-colour of metals is not known from direct observation, except, perhaps, in the case of gold, which in the state of gold-leaf lets through a greenish light.

I am now able to bring forward a striking confirmation of the relation which seems to exist between the light reflected as if from a metal, and that absorbed with great energy. On reading M. Haidinger's paper, of which the title has been already quoted, I was particularly interested by finding crystallized permanganate of potash mentioned among the substances which exhibit distinct surface- and substance-colours. I had previously noticed

* See Dr. Lloyd's Lectures on the Wave-Theory of Light, part 2. p. 18.
the very remarkable mode of absorption of light by red solution of mineral chameleon*. This solution, which may be regarded as an optically pure solution of permanganate of potash, inasmuch as it is associated with only a colourless salt of potash, absorbs green light with great energy, as is indicated by the tint, even without the use of a prism. But when the light transmitted by a pale solution is analysed by a prism, it is found that there are five remarkable dark bands of absorption, or minima of transparency, which are nearly equidistant, and are situated mainly in the green region. The first is situated on the positive or more refrangible side of the fixed line D, at a distance, according to a measurement recently taken, of about four-sevenths of the interval between consecutive bands; the last nearly coincides with F. The first minimum is less conspicuous than the second and third, which are the strongest of the set. Now it occurred to me, that as the solution is so opaque for rays having the refrangibilities of these minima of transparency, corresponding maxima might be expected in the light reflected from the crystals. This expectation has since been realized by observations made on some small crystals. On analysing the reflected light by a prism, I was readily able to observe four bright bands, or maxima, in the spectrum. These, as might have been expected, were more easily seen when the light was incident nearly perpendicularly than at a large angle of incidence. The first band was yellow, the others green, passing on to bluish-green. On decomposing the light reflected at a considerable angle of incidence, in a plane parallel to the axis, into two streams, polarized respectively in, and perpendicularly to, the plane of incidence, and analysing them by a prism, the bands were hardly or not at all perceptible in the spectrum of the former pencil, while that of the latter consisted of nothing but the bright bands.

The tint alone of the first bright band already indicated that the band was more refrangible than the light lying on the negative side of the first dark band seen in the spectrum of the light transmitted by the solution, and less refrangible than the light lying between the first and second dark bands, so that its position would correspond, or nearly so, to the first dark band. However, the eye is greatly liable to be deceived, in experiments on absorption, by the effects of contrast, and therefore an observation of the nature of that just mentioned cannot, I conceive, be altogether relied on. The smallness of the crystals occasioned some difficulty; but a more trustworthy observation was made in the following manner.

The sun’s light was reflected horizontally into a darkened

* Phil. Trans. for 1852, p. 558.
room, and allowed to fall on a crystal. The reflected light was limited by a slit, placed at the distance of two or three feet from the crystal. This precaution was taken to ensure making the observation on the regularly reflected light. Had no slit been used, or else a slit placed close to the crystal, it might have been supposed that the light observed was not regularly reflected, but merely scattered, as it would be by a coloured powder. The appearance of a spot of green light on a screen held at the place of the slit showed that the light was really regularly reflected. The slit was also traversed by the light scattered by the support of the crystal, &c. The slit was viewed through a prism and small telescope; and the position of the dark bands, or minima of brilliancy, in the reflected light could thus be compared with the fixed lines, which were seen by means of the scattered light in the uninterrupted spectrum corresponding to that portion of the slit through which the light reflected from the crystal did not pass. The minimum situated on the positive side of the first bright band lay at something more than a band-interval on the positive side of the fixed line D; the minimum beyond the fourth bright band lay at the distance of about half a band-interval on the negative side of F. It thus appears that the minima in the light reflected by the crystal were intermediate in position between the minima seen in the light transmitted through the solution, so that the maxima of the former corresponded to the minima of the latter.

It might have been considered satisfactory to compare the reflected light with the light transmitted, not by the solution, but by the crystals themselves. But the crystals absorb light with such energy as to be opake; and even when they are spread out on glass, the film thus obtained is too deeply coloured for the purpose. For to show the bands well, the solution must be so dilute, or else seen through so small a thickness, as to be merely pink. As M. Haidinger states that the phenomena of the reflected light are the same for all the faces in all azimuths, and for the polished surface of a mass of crushed crystals, it may be presumed that the absorption is not much affected by the crystalline arrangement, and that the composition of the light transmitted by the solution is sensibly the same as that which would be observed across a crystalline plate, were it possible to obtain one of sufficient thinness.

The first bright band in the reflected light does not usually appear to be very distinctly separated from the continuous light of lower refrangibility; but the latter may be got rid of by observing the light reflected about the polarizing angle, and analysing it so as to retain only the portion polarized perpendicularly to the plane of incidence. As the surface of the crystals
is liable to become spoiled, it is safest in observations on the reflected light to make use of a crystal recently taken out of the mother-liquor. I have only observed four bright bands in the reflected light, whereas there are five distinct minima in the light transmitted by the solution. However, the extreme minima are less conspicuous than the intervening ones, besides which the fifth occurs in a comparatively feeble region of the spectrum. The fourth bright band in the reflected light was rather feeble, but with finer crystals perhaps even a fifth might have been visible. As the metallicity of the crystals is almost or perhaps quite insensible in the parts of the spectrum corresponding to the maxima of transparency, we may say, that, as regards the optical properties of the reflected light, the medium changes four or five times from a transparent substance to a metal and back again, as the refrangibility of the light changes from a little beyond the fixed line D to a little beyond F.

Pembroke College, Cambridge,
November 14, 1853.


1. Difficulties of the present Hypothesis.

NOTWITHSTANDING the perfection to which the geometrical part of the undulatory theory of light has been brought, it is admitted that great difficulty exists in framing, to serve as a basis for the theory, a physical hypothesis which shall at once be consistent with itself and with the known properties of matter.

The present paper is a summary of the results of an attempt to diminish that difficulty. All the conclusions stated have been deduced by means of strict mathematical analysis; and although it is impossible to read the investigations before the British Association in detail, their results can easily be verified by every mathematician who is familiar with the undulatory theory in its present form.

It may be considered as established, that if we assume the supposition that plane-polarized light (out of the varieties of which all other light can be compounded) consists in the wave-like transmission of a state of motion, the nature and magnitude of which are functions of the direction and length of a line transverse to the direction of propagation, we can deduce from this supposition, with the aid of experimental data, and of certain auxiliary hypotheses, the laws of the phenomena of the inter-

* Communicated by the Author, having been read to the British Association for the Advancement of Science, Section A, at Hull, September 10, 1853.
ference of light, of its propagation in crystalline and uncrystalline substances, of diffraction, of single and double refraction, of dispersion by refraction, and of partial and total reflection.

It has hitherto been always assumed, that the kind of motion which constitutes light is a vibration from side to side, transmitted from particle to particle of the luminiferous medium, by means of forces acting between the particles. In order to account for the transmission of such transverse vibrations, the luminiferous medium has been supposed to possess a kind of elasticity which resists distortion of its parts, like that of an elastic solid; and in order to account for the non-appearance in ordinary cases of effects which can be ascribed to longitudinal vibrations, it has been found necessary to suppose further, that this medium resists compression with an elasticity immensely greater than that with which it resists distortion; the latter species of elasticity being, nevertheless, sufficiently great to transmit one of the most powerful kinds of physical energy through interstellar space with a speed in comparison with which that of the swiftest planets of our system in their orbits is appreciable, but no more.

It seems impossible to reconcile these suppositions with the fact, that the luminiferous medium in interstellar space offers no sensible resistance to the motions of the heavenly bodies.

A step towards the solution of this difficulty was made by Mr. MacCullagh. The equations which he used to express the laws of the propagation of light, when interpreted physically, denote the condition of a medium whose molecules tend to range themselves in straight lines, and when disturbed, to return to those lines with a force depending on the curvature of the lines into which they have been moved. But even this hypothesis requires the assumption, that the elasticity of the luminiferous medium to resist compression is immensely greater than the elasticity which transmits transverse vibrations.

The difficulty just referred to arises from a comparison of the hypothesis of transverse vibrations with the observed phenomena of the world.

Another difficulty arises within the hypothesis itself. Fresnel originally assumed, that in crystalline media, where the velocity of light varies with the position of the plane of polarization, the direction of vibration is perpendicular to that plane. This is equivalent to the supposition, that the velocity with which a state of rectilinear transverse vibration is transmitted through such a medium, is a function simply of the direction in which the particles vibrate. From this hypothesis he deduced the form of that wave-surface which expresses completely the law of the propagation of plane-polarized light through crystalline media, and he obtained also a near approximation to the laws of the inten-
sity of plane-polarized light reflected from singly refracting substances.

But it was afterwards demonstrated, that the elastic forces which propagate a transverse movement in any medium must necessarily be functions, not merely of the direction of the movement, but jointly of this direction and the direction of propagation; that is to say, of the position of the plane containing these two directions. Consequently, if the various velocities of variously polarized light in a doubly refracting medium arise from variations of elasticity in different directions, the direction of vibration is in the plane of polarization, contrary to the hypothesis of Fresnel. Fresnel's wave-surface, and his approximate formulæ for the intensity of reflected light, are deducible from this supposition as readily as from his original hypothesis; and Mr. MacCullagh obtained from it formulæ for the intensity of light reflected from doubly refracting substances, agreeing closely with the experiments of Sir David Brewster.

On the other hand, the formulæ of M. Cauchy, and those of Mr. Green as modified by Mr. Haughton*, expressing the effects of reflexion on the intensity and phase of polarized light, all of which are founded on the supposition that the direction of vibration is perpendicular to the plane of polarization, have been shown to be capable, by the introduction of proper constants, of giving results agreeing closely with those of the important experiments of M. Jamin (Annales de Chimie et de Physique, 3rd series, vol. xxix. 1850); and it is difficult, if not impossible, to see how such formulæ could have been deduced from the opposite supposition.

But the true crucial experiment on this subject has been furnished by the researches of Professor Stokes on the Diffraction of Polarized Light (Camb. Trans. vol. ix.). Whatsoever may be the nature of the motion that constitutes light, if it can be expressed by a function of the direction and length of a line perpendicular to the direction of propagation (which may be called a transversal), it is certain that this motion will be more abundantly communicated round the edge of an obstacle when its transversal is parallel than when it is perpendicular to that edge; so that the effect of diffraction is, to bring every oblique transversal into a position more nearly parallel to the diffracting edge. But it has been shown by the experiments of Professor Stokes, that the effect of diffraction upon every ray of light polarized in a plane oblique to the diffracting edge, is to bring the plane of polarization into a position more nearly perpendicular to the diffracting edge. Therefore the transversal of a ray of plane-polarized light (which, if light consists in linear vibratory

* Philosophical Magazine, August 1853.
movement, is the direction of vibration) is perpendicular to the plane of polarization.

Hence it follows, that in a crystalline medium the velocity of light depends simply on the direction of the transversal characteristic of the movement propagated, and not on the direction of propagation.

This conclusion is opposed to the laws of the propagation of transverse vibrations through a crystalline elastic solid, or through any medium in which the velocity of propagation depends on elasticity varying in different directions. Therefore the velocity of light depends on something not analogous to the variations of elasticity in such a medium.

To solve this difficulty, the author of this paper some time since suggested the hypothesis that the luminiferous medium consists of particles forming the nuclei of atmospheres of ordinary matter; that it transmits transverse vibrations by means of an elasticity which is the same in all substances and in all directions; and that the variations in the velocity of the transmission of vibrations arise from variations in the atmospheric load which the luminiferous particles carry along with them in their vibrations, and which is a function of the nature of the substance, and, in a crystalline body, of the direction of vibration.

But although this hypothesis removes the inconsistency just pointed out as existing within the theory itself, it leaves undiminished the difficulty of conceiving a medium pervading all space, and possessed of an elasticity of figure, at once so strong as to transmit the powerful energy of light with its enormous velocity, and so feeble as to exercise no direct appreciable effect on the motions of visible bodies.

2. Statement of the Proposed Hypothesis of Oscillations.

The hypothesis now to be proposed as a groundwork for the undulatory theory of light, consists mainly in conceiving that the luminiferous medium is constituted of detached atoms or nuclei distributed throughout all space, and endowed with a peculiar species of polarity, in virtue of which three orthogonal axes in each atom tend to place themselves parallel respectively to the corresponding axes in every other atom; and that plane-polarized light consists in a small oscillatory movement of each atom round an axis transverse to the direction of propagation.

Such a movement would be transmitted through such a medium with a velocity proportional,—directly, to the square root of the total rotative force exercised by the luminiferous atoms in a given small space, upon those in a given adjacent small space lying in the direction of propagation, in consequence of a given amount of relative angular displacement round the axis of oscill-
Oscillatory Theory of Light.

lation;—and inversely, to the square root of the sum of the moments of inertia round the axes of oscillation of the atoms contained in a given space, loaded with such portions of molecular atmospheres surrounding them as they may carry along with them in their oscillations.

Then denoting by

\[ h \]  

the velocity, in a given direction of plane-waves, of oscillation round transverse axes parallel to a given line;

\[ C \]  

a coefficient of polarity or rotative force for the given directions of propagation and of axes;

\[ M \]  

a coefficient of moment of inertia for the given direction of axes;

the above principle may be represented by this equation,

\[ h^2 = \frac{C}{M}. \]

The coefficient of polarity in question is proper only to an axis of oscillation transverse to the direction of propagation. To account for the stability of direction of the axes of the atoms, and also for the non-appearance, in ordinary cases, of phenomena capable of being ascribed to oscillations round axes parallel to the direction of propagation, it is necessary to suppose the corresponding coefficient for the latter species of oscillations to be much greater than the coefficient for transverse axes of oscillation.

It is evident, that how powerful soever the polarity may be, which is here ascribed to the atoms of the luminiferous medium, it is a kind of force which must be absolutely destitute of direct influence on resistance to change of volume or change of figure in the parts of that medium, or of any body of which that medium may form part; and that, consequently, the difficulty which in the hypothesis of vibrations arises from the necessity of ascribing to the luminiferous medium properties like those of an elastic solid, has no existence in the hypothesis of oscillations now proposed.

The luminiferous atoms may now be supposed to be diffused throughout all space, and as molecular nuclei, throughout all bodies; the distribution and motion of their centres being regulated by forces wholly independent of that species of polarity which is the means of transmitting a state of oscillation round those centres.


In the diffraction of an oscillatory movement round transverse axes past the edge of an obstacle, a law holds good exactly ana-
logous to that demonstrated by Professor Stokes for a transverse vibratory movement, substituting only the axis of oscillation for the direction of vibration; that is to say,

The direction of the axes of oscillation in the diffracted wave is the projection of that of the axes of oscillation in the incident wave on a plane tangent to the front of the diffracted wave.

Consequently, oscillations in the incident wave, round axes oblique to the diffracting edge, give rise to oscillations in the diffracted wave round axes more nearly parallel to the diffracting edge.

But the experiments of Professor Stokes have proved, that light polarized in a plane oblique to the diffracting edge, becomes, after diffraction, polarized in a plane more nearly perpendicular to the diffracting edge.

Therefore the axes of oscillation in plane-polarized light are perpendicular to the plane of polarization.

Therefore the velocity of transmission of oscillations round transverse axes through the luminiferous medium in a crystalline body is a function simply of the direction of the axes of oscillation.

Now if the variations of the velocity of transmission arose from variations of the coefficient of transverse polarity (denoted by C), they would depend on the direction of propagation as well as upon that of the axes of oscillation, so that the plane of polarization would be that which contains these two directions. Since the velocity of transmission depends on the direction of the axes of oscillation only, it follows that its variations in a given crystalline medium arise wholly from variations of the moment of inertia of the luminiferous atoms, together with their loads of extraneous matter.

Consequently the coefficient of polarity C for transverse axes of oscillation is the same for all directions in a given substance.

To account for the known laws of the intensity and phase of reflected and refracted light consistently with the hypothesis of oscillations, it is necessary to suppose also that this coefficient is the same for all substances; so that the variations of the velocities of light and indices of refraction for different media depend solely on those of the moments of inertia of the loaded luminiferous atoms.

There is reason to anticipate, that, upon further investigation, it will appear that this condition is necessary to the stability of the luminiferous atoms.

4. Of the Wave-surface in Crystalline Bodies.

Let the axes of coordinates be those of molecular symmetry in a crystalline medium.

Let \( M_1, M_2, M_3 \) be coefficients proportional to the moments
of inertia of the luminiferous atoms with their loads of extraneous matter, round axes parallel to $x, y, z$ respectively.

Let $r$ be a radius vector of the diverging wave-surface in the direction $(\alpha, \beta, \gamma)$.

Then the equation of that surface for polar coordinates is

$$\frac{1}{r^2} - \frac{1}{r^2} \cdot \frac{1}{C} \{(M_2 + M_3) \cos^2 \alpha + (M_3 + M_1) \cos^2 \beta + (M_1 + M_2) \cos^2 \gamma\}$$

$$+ \frac{1}{C^2} \{M_2M_3 \cos^2 \alpha + M_3M_1 \cos^2 \beta + M_1M_2 \cos^2 \gamma\} = 0;$$

and for rectangular coordinates,

$$\frac{1}{C^2}(x^2 + y^2 + z^2) \cdot (M_2M_3x^2 + M_3M_1y^2 + M_1M_2z^2)$$

$$- \frac{1}{C} \{(M_2 + M_3)x^2 + (M_3 + M_1)y^2 + (M_1 + M_2)z^2\} = 1.$$

The above equations are exactly those of Fresnel's wave-surface, with the following semiaxes:

- Directions. Semiaxes.
  - $x$, $\sqrt{\frac{C}{M_2}}$, $\sqrt{\frac{C}{M_3}}$;
  - $y$, $\sqrt{\frac{C}{M_3}}$, $\sqrt{\frac{C}{M_1}}$;
  - $z$, $\sqrt{\frac{C}{M_1}}$, $\sqrt{\frac{C}{M_2}}$;

the squares of the semiaxes of the wave-surface along each axis of coordinates being inversely proportional to the moments of inertia of the loaded luminiferous atoms in a given space round the other two axes of coordinates.

The plane of polarization at each point of the wave-surface is perpendicular to the direction of greatest declivity.

The equation of the index-surface, whose radius in any direction is inversely proportional to the normal velocity of the wave, is formed from that of the wave-surface by substituting respectively,

$$C, \frac{1}{M_1}, \frac{1}{M_2}, \frac{1}{M_3};$$

for

$$\frac{1}{C}, M_1, M_2, M_3.$$

These equations are obtained on the supposition that the coefficient of polarity for axes of oscillation parallel to the direction of propagation (which we may call $A$) is either very large or very small compared with that for transverse axes. By treat-

ing the ratio of these quantities as finite, there is obtained an
equation of the sixth order, representing a wave-surface of three
sheets, differing somewhat from that of the propagation of vibra-
tions in an elastic crystalline solid; inasmuch as the former has
always three circular sections, while the latter has none, unless
it is symmetrical all round one axis at least. By increasing the
ratio \( \frac{A}{C} \) without limit, this equation is made to approximate
indefinitely to the product of the equation of Fresnel’s wave-
surface by the following,

\[
\frac{M_1}{A} x^2 + \frac{M_2}{A} y^2 + \frac{M_3}{A} z^2 = 1;
\]

which represents a very large ellipsoidal wave of oscillations
round axes parallel to the direction of propagation.

5. Of Reflexion and Refraction.

According to the proposed hypothesis of oscillations, the laws
of the phase and intensity of light reflected and refracted at the
bounding surface of two transparent substances are to be deter-
mined by conditions analogous to those employed in the hypo-
thesis of vibrations by M. Cauchy and Mr. Green. They are
the consequences of the principle, that if we have two sets of
formulae expressing the nature and magnitude of the oscillations
in the two substances respectively, then either of those formulae,
being applied to a particle at the bounding surface, ought to give
the same results.

According to this principle, the following six quantities for a
particle at the bounding surface must be the same at every
instant, when computed by either of the two sets of formulae:—

The three angular displacements round the three axes of
coordinates,

The three rotative forces round the same three axes.

There is, generally speaking, a change of phase when light
undergoes refraction or reflexion. It is known that we may
express this change of phase by subdividing each reflected or
refracted disturbance into two, of suitable intensities and signs;
one synchronous in phase with the corresponding incident dis-
turbance, and the other retarded by a quarter of an undulation.
There are thus twelve quantities to be found, viz. the amplitudes
of the six components of the reflected disturbance, and those of
the six components of the refracted disturbance. To determine
these quantities there are twelve conditions, viz. the equality at
every instant, according to the formulae for either medium, of
the total angular displacements, and of the total rotative forces,
round each of the three axes of coordinates, for the set of waves
composed of the incident wave and those synchronous with it, and for the set of waves retarded by one quarter of an undulation. The results of these conditions have been investigated in detail for singly refracting substances.

The indices of refraction of such substances are proportional to the square roots of the moments of inertia of the loaded luminiferous atoms in a given space. Thus, if the coefficients $M', M''$ are proportional to these moments in two given substances respectively, then the index of refraction of the second substance relatively to the first is

$$\mu = \sqrt{\frac{M''}{M'}}.$$

In the case of light incident on a plane surface between two such media, the axes of coordinates may be assumed respectively perpendicular to the reflecting surface, perpendicular to the plane of reflexion, and along the intersection of those two planes; and oscillations round axes normal and parallel to the plane of reflexion may be considered separately.

When the axes of oscillation are normal to the plane of reflexion, that is to say, when the light is polarized in that plane, the formulae for the intensities of the reflected and refracted light agree exactly with those of Fresnel. When the reflexion takes place in the rarer medium, the reflected light is retarded by half an undulation; when in the denser, there is no change of phase unless the reflexion is total, when there is a certain acceleration of phase depending on the angle of incidence. In the last case, the disturbance in the second medium is an evanescent wave, analogous to those introduced into the vibratory theory by M. Cauchy and Mr. Green; that is to say, a wave in which the amplitude of oscillation diminishes in proportion to an exponential function of the distance from the bounding surface (called by M. Cauchy the modulus), and which travels along that surface with a velocity less than the velocity of an ordinary wave; the square of the negative exponent of the modulus being proportional to the difference of the squares of those velocities, divided by the square of the velocity of an ordinary wave.

This is an evanescent wave of oscillation round transverse axes.

How large soever the coefficient of polarity for oscillations round longitudinal axes may be, an evanescent wave of such oscillations may travel along the bounding surface of a medium with any velocity, however slow, provided the negative exponent of the modulus is made large enough. Consequently, in framing the formulae to represent oscillations round axes parallel to the plane of incidence, we must introduce in each medium two such evanescent waves of suitable exponents and indeterminate amplitudes; one travelling along the surface with the incident wave,
and the other a quarter of an undulation behind it. The maximum amplitudes of oscillation in these evanescent waves constitute four unknown quantities; the amplitudes in the two ordinary reflected waves and the two ordinary refracted waves, differing by one quarter of an undulation, constitute four more unknown quantities, making eight in all: four conditions having been fulfilled by the waves polarized in the plane of incidence, there remain to be fulfilled eight conditions; viz. the identity, as calculated by the formulae for the first and second substance respectively, of the following eight functions at the bounding surface; the angular displacement, and the rotative forces, round each of the two axes in the plane of incidence, for the incident wave and the set of waves synchronous with it, and for the set of waves retarded by one quarter of an undulation. These conditions are sufficient to determine the unknown quantities, and to complete the solution of the problem.

The following is a general statement of the results of the solution when the second medium is the denser. They agree with the results of the experiments of M. Jamin, and are in every respect analogous to those deduced from the hypothesis of vibrations by M. Cauchy, Mr. Green, and Mr. Haughton.

Light polarized in a plane perpendicular to the plane of incidence, suffers by reflexion at a perpendicular incidence no alteration of phase.

At a grazing incidence (or when the angle of incidence differs insensibly from 90°), the phase, like that of light polarized in the plane of incidence, is retarded by half an undulation.

The variation of phase with the angle of incidence is, in fact, continuous; but it is, generally speaking, not appreciable by observation, except in the immediate neighbourhood of an angle called by M. Jamin the principal incidence, where the retardation of phase is a quarter of an undulation.

This angle differs by a very small amount, appreciable only in certain substances, from the polarizing angle, at which the intensity of light polarized in a plane at right angles to the plane of incidence is a minimum.

The "Law of Brewster," that the tangent of the polarizing angle is equal to the index of refraction, is, theoretically, only approximately true; but the error is quite inappreciable.

When the second medium is the less dense, the phase of the reflected light is half an undulation in advance of its value when the second medium is the denser.

In either case, light polarized in planes perpendicular to the plane of incidence is less retarded, that is to say, is accelerated in phase, as compared with light polarized in that plane, according to the following table:—
6. Of Circular and Elliptic Polarization.

Light polarized in a plane oblique to the angle of incidence is, generally speaking, elliptically polarized after reflexion, the plane-polarized components of the disturbance being in different phases. According to the hypothesis of oscillations, circularly and elliptically polarized light, being compounded of oscillations in different phases round two transverse axes, consist in a sort of nutation of the longitudinal axis of each luminiferous atom. The direction of this nutation, and the form of the circle or ellipse described by the ends of the longitudinal axes, serve to define the character of the light. The ellipse of nutation has its axes in the same proportion with, but perpendicular in position to, those of the elliptic orbit supposed to be described by each atom according to the hypothesis of vibrations.

The molecular mechanism by which certain media transmit right- and left-handed circularly or elliptically polarized light with different velocities, is still problematical according to either hypothesis. The laws of the phenomena, however, may be represented by means of the assumption, that in the substances in question the extraneous load on the luminiferous atoms is a function of the direction of nutation.

7. Of Dispersion.

If we assume the extent of sensible direct action of the polarity of the luminiferous atoms to be appreciable as compared with the length of a wave, the velocity of propagation (precisely as with the vibratory hypothesis) is found to consist of a constant quantity, diminished by the sum of a series in terms of the reciprocal of the square of the length of a wave.

It may be doubted, however, whether this supposition is of itself adequate to explain the phenomena of dispersion; and
whether it may not be necessary to assume, also, that the load upon the luminiferous atoms is a function of the time of oscillation, as well as of the nature of the substance and the position of the axes of oscillation.

In conclusion, it may be affirmed, that, as a mathematical system, the proposed theory of oscillations round axes represents the laws of all the phenomena which have hitherto been reduced to theoretical principles, as well, at least, as the existing theory of vibrations; while as a physical hypothesis, it is free from the principal objections to which the hypothesis of vibrations is liable.

Glasgow, September 2, 1853.

LXIII. On the Geometrical Representation of an Abelian Integral. By A. Cayley, Esq.*

THE equation of a surface passing through the curve of intersection of the surfaces

\[ x^2 + y^2 + z^2 + w^2 = 0 \]
\[ ax^2 + by^2 + cz^2 + dw^2 = 0 \]

is of the form

\[ \sigma(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0, \]

where \( \sigma \) is an arbitrary parameter. Suppose that the surface touches a given plane, we have for the determination of \( \sigma \) a cubic equation the roots of which may be considered as parameters defining the plane in question. Let one of the values of \( \sigma \) be considered equal to a given quantity \( k \), the plane touches the surface

\[ k(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0, \]

and the other two values of \( \sigma \) may be considered as parameters defining the particular tangent plane, or what is the same thing, determining its point of contact with the surface.

Or more clearly, thus:—in order to determine the position of a point on the surface

\[ k(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0, \]

the tangent plane at the point in question is touched by two other surfaces,

\[ p(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0, \]
\[ q(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0; \]

\( p \) and \( q \) are the parameters by which the point in question is

* Communicated by the Author.
of an Abelian Integral.

determined. We may for shortness speak of the surface
\[ k(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0 \]
as the surface \((k)\). It is clear that we shall then have to speak of
\[ x^2 + y^2 + z^2 + w^2 = 0 \]
as the surface \((\infty)\).

I consider now a chord of the surface \((\infty)\) touching the two
surfaces \((k)\) and \((k')\); and I take \(\theta, \phi\) as the parameters of the
one extremity of this chord; \((p, q)\) as the parameters of the
point of contact with the surface \((k)\); \(p', q'\) as the parameters of the
point of contact with the surface \((k')\); and \(\theta', \phi'\) as the parameters of the
other extremity of the chord; and the points in question may therefore be distinguished as the points \((\infty; \theta, \phi)\),
\((k; p, q)\), \((k'; p', q')\), and \((\infty; \theta', \phi')\). The coordinates of the
point \((\infty; \theta, \phi)\) are given by
\[
x : y : z : w = \sqrt{(a + \theta)(a + \phi)} + \sqrt{(a - b)(a - c)(a - d)}
\]
\[
\cdot \sqrt{(b + \theta)(b + \phi)} + \sqrt{(b - c)(b - d)(b - a)}
\]
\[
\cdot \sqrt{(c + \theta)(c + \phi)} + \sqrt{(c - d)(c - a)(c - b)}
\]
\[
\cdot \sqrt{(d + \theta)(d + \phi)} + \sqrt{(d - a)(d - b)(d - c)};
\]

those of the point \((k; p, q)\) by
\[
x : y : z : w = \sqrt{(a + p)(a + q)} + \sqrt{(a - b)(a - c)(a - d)} + a + k
\]
\[
\cdot \sqrt{(b + p)(b + q)} + \sqrt{(b - c)(b - d)(b - a)} + b + k
\]
\[
\cdot \sqrt{(c + p)(c + q)} + \sqrt{(c - d)(c - a)(c - b)} + c + k
\]
\[
\cdot \sqrt{(d + p)(d + q)} + \sqrt{(d - a)(d - b)(d - c)} + d + k;
\]

and similarly for the other two points.

Consider, in the first place, the chord in question as a tangent
to the two surfaces \((k)\) and \((k')\). It is clear that the tangent
plane to the surface \((k)\) at the point \((k; p, q)\) must contain the
point \((k'; p', q')\), and vice versa. Take for a moment \(\xi, \eta, \zeta, \omega\)
as the coordinates of the point \((k; p, q)\), the equation of the
tangent plane to \((k)\) at this point is
\[ \Sigma(a + k)\xi x = 0; \]
or substituting for \(\xi, \ldots\) their values
\[ \prod \left( a + p \right) \left( a + q \right) \sqrt{a + k} + \sqrt{(a - b)(a - c)(a - d)} = 0; \]
or taking for \(x, \ldots\) the coordinates of the point \((k', p', q')\), we have
for the conditions that this point may lie in the tangent plane in
question,
\[ \Sigma \left( \sqrt{(a + p)(a + q)} \sqrt{(a + p')(a + q')} \sqrt{(a + k)} + \sqrt{(a + k')(a - b)(a - c)(a - d)} = 0; \]
or under a somewhat more convenient form we have

$$\Sigma \left( (b-c)(c-d)(d-b) \sqrt{(a+p)(a+q)} \sqrt{(a+p')(a+q')} \frac{\sqrt{a+k}}{\sqrt{a+k'}} \right) = 0$$

for the condition in order that the point \((k', p', q')\) may lie in the tangent plane at \((k; p, q)\) to the surface \((k)\). Similarly, we have

$$\Sigma \left( (b-c)(c-d)(d-b) \sqrt{(a+p)(a+q)} \sqrt{(a+p')(a+q')} \frac{\sqrt{a+k'}}{\sqrt{a+k}} \right) = 0$$

for the condition in order that the point \((k, p, q)\) may lie in the tangent plane at \((k'; p', q')\) to the surface \((k')\). The former of these two equations is equivalent to the system of equations

$$\sqrt{(a+p)(a+q)(a+p')(a+q')} \frac{\sqrt{a+k}}{\sqrt{a+k'}} = \lambda + \mu a + \nu a^2,$$

and the latter to the system of equations

$$\sqrt{(a+p)(a+q)(a+p')(a+q')} \frac{\sqrt{a+k'}}{a+k} = \lambda' + \mu' a + \nu' a^2;$$

where in each system \(a\) is to be successively replaced by \(b, c, d\), and where \(\lambda, \mu, \nu\) and \(\lambda', \mu', \nu'\) are indeterminate. Now dividing each equation of the corresponding equation in the other system, we see that the equation

$$x + k = \frac{\lambda + \mu x + \nu x^2}{\lambda' + \mu' x + \nu' x^2}$$

is satisfied by the values \(a, b, c, d\) of \(x\); and, therefore, since the equation in \(x\) is only of the third order, that the equation in question must be identically true. We may therefore write

$$\lambda + \mu x + \nu x^2 = (px + \sigma)(x + k), \quad \lambda' + \mu' x + \nu' x^2 = (px + \sigma)(x + k'),$$

and the two systems of equations become therefore equivalent to the single system,

$$\sqrt{(a+p)(a+q)(a+p')(a+q')} = (pa + \sigma) \sqrt{(a+k)(a+k')}$$

$$\sqrt{(b+p)(b+q)(b+p')(b+q')} = (pb + \sigma) \sqrt{(b+k)(b+k')}$$

$$\sqrt{(c+p)(c+q)(c+p')(c+q')} = (pc + \sigma) \sqrt{(c+k)(c+k')}$$

$$\sqrt{(d+p)(d+q)(d+p')(d+q')} = (pd + \sigma) \sqrt{(d+k)(d+k')}$$

a set of equations which may be represented by the single equation

$$\psi(x+p)(x+q)(x+p')(x+q') - (px + \sigma)^2(x + k)(x + k') = \chi(x-a)(x-b)(x-c)(x-d),$$

where \(x\) is arbitrary; or what is the same thing, writing \(-x\)
of an Abelian Integral.

Instead of \( x \),
\[
\chi(x+a)(x+b)(x+c)(x+d) + (px-\sigma)^2(x-k)(x-k')
\]
\[
= \psi(x-p)(x-q)(x-p')(x-q').
\]

So that, putting
\[
\Pi x = \int \frac{dx}{\sqrt{(x+a)(x+b)(x+c)(x+d)(x-k)(x-k')}}
\]
\[
\Pi x = \int \frac{dx}{\sqrt{(x+a)(x+b)(x+c)(x+d)(x-k)(x-k')}}
\]
we see that the algebraical equations between \( p, q ; p', q' \) are equivalent to the transcendental equations
\[
\Pi p \pm \Pi q + \Pi p' \pm \Pi q' = \text{const.}
\]
\[
\Pi p \pm \Pi q + \Pi p' \pm \Pi q' = \text{const.}
\]

The algebraical equations which connect \( \theta, \phi \) with \( p, q ; p', q' \), may be exhibited under several different forms; thus, for instance, considering the point \((\infty; \theta, \phi)\) as a point in the line joining \((k; p, q)\) and \((k'; p', q')\), we must have
\[
\begin{vmatrix}
\sqrt{(a-p)(a-q)} + \sqrt{a+k}, & \sqrt{(b-p)(b+q)} + \sqrt{b+k}, & \ldots \\
\sqrt{(a-p')(a-q')} + \sqrt{a+k'}, & \sqrt{(b-p')(b+q')} + \sqrt{b+k'}, & \ldots \\
\sqrt{(a-\theta)(a+\phi)}, & \sqrt{(b+\theta)(b+\phi)} & \\
\end{vmatrix}
\]
i.e. the determinants formed by selecting any three of the four columns must vanish; the equations so obtained are equivalent (as they should be) to two independent equations.

Or, again, by considering \((\infty; \theta, \phi)\) first as a point in the tangent plane at \((k; p, q)\) to the surface \((k)\), and then as a point in the tangent plane at \((k'; p', q')\) to the surface \((k')\), we obtain
\[
\Sigma ((b-c)(c-d)(d-b) \sqrt{(a-p)(a+q)} \sqrt{(a+k)} \sqrt{(a+\theta)(a+\phi)}) = 0
\]
\[
\Sigma ((b-c)(c-d)(d-b) \sqrt{(a-p')(a+q')} \sqrt{(a+k')} \sqrt{(a+\theta)(a+\phi)}) = 0.
\]

Or, again, we may consider the line joining \((\infty; \theta, \phi)\) and \((k; p, q)\) or \((k'; p', q')\), as touching the surfaces \((k)\) and \((k')\); the formulae for this purpose are readily obtained by means of the lemma.

"The condition in order that the line joining the points \((\xi, \eta, \xi', \omega)\) and \((\xi', \eta', \xi', \omega')\) may touch the surface
\[
ax^2 + by^2 + cz^2 + dw^2 = 0
\]
is
\[
\Sigma ab(\xi \eta' - \xi' \eta) = 0,
\]
the summation extending to the binary combinations of \(a, b, c, d\)."

But none of all these formulae appear readily to conduct to the transcendental equations connecting \( \theta, \phi \) with \( p, q ; p', q' \).
Dr. F. Wrightson's Remarks on
Reasoning from analogy, it would seem that there exist transcendental equations
\[ \pm \Pi \theta \pm \Pi \phi \pm \Pi p \pm \Pi p' = \text{const.} \]
\[ \pm \Pi \theta \pm \Pi \phi \pm \Pi p \pm \Pi p' = \text{const.}, \]
or the similar equations containing \( q, q' \), instead of \( p, p' \), into which these are changed by means of the transcendental equations between \( p, q, p', q' \). If in these equations we write \( \theta', \phi' \) instead of \( \theta, \phi \), it would appear that the functions \( \Pi p, \Pi p', \Pi_1 p, \Pi_1 p' \) may be eliminated, and that we should obtain equations such as
\[ \pm \Pi \theta \pm \Pi \phi \pm \Pi \theta' \pm \Pi \phi' = \text{const.} \]
\[ \pm \Pi \theta \pm \Pi \phi \pm \Pi \theta' \pm \Pi \phi' = \text{const.} \]
to express the relations that must exist between the parameters \( \theta, \phi \), and \( \theta', \phi' \) of the extremities of a chord of the surface
\[ x^2 + y^2 + z^2 + w^2 = 0, \]
in order that this chord may touch the two surfaces
\[ k(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0, \]
\[ k'(x^2 + y^2 + z^2 + w^2) + ax^2 + by^2 + cz^2 + dw^2 = 0. \]
The quantities \( k, k' \), it will be noticed, enter into the radical of the integrals \( \Pi x, \Pi_1 x \). This is a very striking difference between the present theory and the analogous theory relating to conics, and leads, I think, to the inference that the theory of the polygon inscribed in a conic, and the sides of which touch conics intersecting the conic in the same four points, cannot be extended to surfaces in such manner as one might be led to suppose from the extension to surfaces of the much simpler theory of the polygon inscribed in a conic, and the sides of which touch conics having double contact with the conic. (See my paper "On the Homographic Transformation of a surface of the second order into itself.")

The preceding investigations are obviously very incomplete; but the connexion which they point out between the geometrical question and the Abelian integral involving the root of a function of the sixth order, may, I think, be of service in the theory of these integrals.

LXIV. Remarks on Professor Williamson's Othyle Theory.

By Dr. F. Wrightson.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE only within the last few days had an opportunity of observing a communication in the September Number of the Philosophical Magazine by Professor Williamson, making my
investigation* "Upon the Atomic Weight and the Constitution of the Alcohols" the subject of his criticism. Prof. Williamson writes:—"In the August Number of the Philosophical Magazine I observe a paper by Dr. Wrightson, describing the action of an alcoholic solution of potash-hydrate upon a mixture of the cyanides of æthyle and amyle, and employing the result of that experiment as an argument against the othyle theory;" he goes on to show the inadmissibility of such a deduction, derived from the above-named experiments, as adverse to the othyle theory. Now will Professor Williamson do me the favour to indicate where I have used the result of the above experiments as an argument against the othyle theory? I have never drawn any conclusion whatever as to the othyle theory from the results I obtained. My investigation, as its title implies, was directed to prove or disprove the hypothetical substitution of certain alcohol and æther radicals; and Professor Williamson might certainly have abstained from so gratuitously forcing his othyle hypothesis upon me to assist in the verification of his alcohol theory, considering my very decided objection thereto, expressed at the outset before alluding to my experiments, in the following words (Phil. Mag. August Number, p. 91):—

"To show, however, the relation of the othyle theory to the before-named important facts, and convey the conviction that it is no longer fairly tenable," &c.

The experiments of Kolbe and Frankland, of Dumas, &c., upon the facile transformation of the cyanides into the acids, and vice versa, the transformation of the ammonia salts of the acids into the cyanides; the decomposition of valerianic and acetic acids by the galvanic stream, were the grounds upon which I rejected the othyle hypothesis, and based my experiments upon the generally accepted formula for this class of acids,

\[ C^n H^{n+1} \cdot C^2 O^3 \]

commencing at formic acid and ascending to capric acid or beyond. I must therefore decidedly object to Professor Williamson's mode of argument complaining of my unreasonableness in desiring two æther radicals in the quasi acids, while his othyle theory denied the existence of one. Certainly, upon the othyle hypothesis or 'assumption,' no intermediate acids could exist between those already known, and therefore, I repeat, I was careful to show at the outset I could not proceed upon such an assumption.

But according to the view I took, which considers acetic acid as containing one equivalent or two atoms of methyle, we had

* Phil. Mag. August 1853.
every reason to expect the existence of such intermediate acids. Therefore, without further reference to the othyle theory, the inquiry I had in view may be thus simply stated. In propionic acid,

$$\{C_3\text{H}_5\text{O}_3\text{H} \cdot C_2\text{H}_5\text{O} \}$$

can one atom of the æthyle radical, conformable to Williamson’s alcohol and æther theory, be substituted by any other alcohol radical?

The experiments to satisfy this inquiry furnished me with conclusions which I applied to the alcohol and æther theory alone. These conclusions represented Professor Williamson’s theory of the substitution of the æther radicals as much less probable than before, especially as it was unsupported by any decisive facts, but by no means represented that theory as overturned. To the othyle hypothesis they did not refer, except in so far as the latter is dependent upon the former. Without further discussing the great difficulties presented by the latter,

I remain, Gentlemen,

Yours faithfully,

Birmingham, Oct. 24, 1853.

F. Wrightson.

LXV. On the Origin of Tar in Olefiant Gas. By G. Magnus.*

Upwards of sixty years have passed since gas was first applied for the purposes of illumination; but notwithstanding this, the property to which its illuminating power, and consequently its entire application, is due, namely, the yielding up of carbon at a red heat, is not yet quite understood.

The Dutch physicists who discovered olefiant gas, Dieman, Van Troostwyk, Lawrenberg and Bondt, alluded in their earliest communication in 1795 to the separation of carbon during its combustion. Since that time various investigations have been published on this gas, as well as on that derived from stone-coal. Besides the repetition of the Dutch experiments by Vauquelin and Hecht at the request of the Philomathic Society of Paris, Berthollet, Th. Thomson, Th. De Saussure, Dr. Henry,**

† Journal de Physique, vol. xlv, p. 246.
‡ Annales de Chimie, vol. xxi, p. 64.
** Philosophical Transactions for 1808, p. 232; and for 1821, p. 136.
Brande*, J. Davy†, and G. Bischoff‡ of Bonn, have engaged in inquiries on coal-gas, marsh-gas, and the different carburets of hydrogen.

To these have been lately added Frankland's§ excellent contributions to our knowledge of the manufacture of gas, the object of which is to prove that the process of White is to be preferred, which consists in driving the gases forth from the red-hot retorts by the introduction of other gases produced by the action of steam upon charcoal. As, however, Frankland has only occupied himself with coal-gas, which is a mixture of different gases, the decomposition of the pure olefiant gas was not in his case further looked into.

An investigation which specially treats of the decomposition of this gas was carried on by Marchand||. He conducted olefiant gas, which was obtained from the action of sulphurous acid upon alcohol, through milk of lime and sulphuric acid in order to purify it, afterwards over caustic potash to dry it, and through a red-hot tube which was filled with copper wire. The gas which issued from this tube he analysed by conducting it through a red-hot tube containing oxide of copper, and determining the quantity of water and of carbonic acid formed. He found that at the commencement it was almost pure marsh-gas, but after the tube had attained a complete white heat, the issuing gas was almost pure hydrogen. All the carbon had been separated.

The olefiant gas generated from sulphuric acid and alcohol, after it has been freed as far as possible from sulphurous acid, aether, and the vapours of wine-oil, possesses a peculiar odour. On conducting a quantity of such gas through a red-hot glass tube, I remarked that this odour had disappeared, and that the gas had assumed the smell of coal-tar. This observation was the origin of the following experiments.

In a former communication on the sulphate of carbyle, I mentioned that olefiant gas might be prepared by mixing sulphuric acid in a spacious glass vessel with about the sixteenth of its weight of alcohol, warming the mass until it begins to colour, and then pouring through a Welter's funnel, which enters through the cup of the vessel, small quantities of alcohol into the latter. Since then M. Mitscherlich has proposed to introduce the alcohol in the state of vapour, by boiling it beforehand in a separate vessel, and conducting the vapour into the sulphuric acid, which has been diluted with 30 per cent. water and heated to 165° C.

* Philosophical Transactions for 1820, p. 11.
§ Annalen der Chemie und Pharmacie, vol. lxxxi.
M. Mitscherlich indeed says, that, in order to prepare the gas quickly and conveniently, it is well to make use of the original method, and to four parts of sulphuric acid to add at once one part of alcohol and then to warm cautiously; I have found, however, that the method according to which the alcohol is introduced as vapour into the sulphuric acid, both as regards quickness and purity of gas, is preferable to all others, and I have therefore always followed this method.

In order to free the gas from all wine-oil and aëther, as also from any trace of sulphurous acid that might be present, which, however, was never observed, it was in the first place conducted through several vessels containing concentrated sulphuric acid and afterwards through caustic potash. In order to be certain that no foreign gas was present, a small quantity of the gas was collected over mercury, and fuming sulphuric acid allowed to act on it. In some cases a quantity was collected over water, mixed with chlorine, a specimen of which was completely absorbed by caustic potash. After the Dutch oil had formed, caustic potash was added in order to take away the excess of chlorine gas. Even when the olefiant gas prepared in this manner, and recognized as pure, was heated to redness in a glass tube, the production of tar was the consequence.

It might be imagined, perhaps, that the gas still contained slight traces of oxygen, as, according to the investigation of Schrötter on the emission of light by phosphorus, traces of oxygen are found in gases which have been purified in various ways. To this, indeed, the pretty considerable quantity of tar obtained could not be due; but in order to be certain that the tar is formed even when the gas is completely free from oxygen, olefiant gas was conducted through a long glass tube, at one end of which was placed a bit of phosphorus while the other end dipped into mercury. After the gas had streamed through the tube for a considerable time, and it might be assumed that all atmospheric air was removed, the phosphorus was melted and the gas thus freed from the least trace of oxygen. When the tube was heated at a distance from the phosphorus, tar was immediately produced.

In a similar manner a curved glass tube closed at one end was filled with olefiant gas and the communication shut off with mercury; a portion of phosphorus was then brought into the bent portion and there melted. The tube was then heated to redness at another place, when the tar soon began to be condensed upon the inside of the tube. It is therefore placed beyond doubt, that tar is produced, even when olefiant gas, which is totally free from oxygen, is exposed to a red heat, and that hence the tar can contain nothing else than the constituents of this gas, namely, carbon and hydrogen.
When the upper portion of a curved tube or receiver filled with pure gas and shut off by mercury was heated in a bath of Rose's metal, the temperature of which was a little higher than that of boiling mercury, the gas did not assume the smell of tar, but retained its original odour. The formation of tar from olefiant gas begins therefore at a temperature at all events higher than 360° C., and it appears not to occur at a temperature under that of a red heat.

At a dark red heat, however, the tar is formed in such quantities that it collects in drops. It is only partially volatile; for if we attempt to distil it, a quantity of carbon always remains behind. It is not always of the same character, for it is sometimes lighter and sometimes darker.

When the tar is formed, the volume of the gas from which it comes is diminished. This diminution is different according to the decomposition to which the tar itself is subjected. In different determinations the volume of the gas which remained at the temperature and pressure of the original gas amounted to—

<table>
<thead>
<tr>
<th>Volume</th>
<th>per cent.</th>
</tr>
</thead>
<tbody>
<tr>
<td>84·4</td>
<td></td>
</tr>
<tr>
<td>93·6</td>
<td></td>
</tr>
<tr>
<td>92·1</td>
<td></td>
</tr>
<tr>
<td>89·4</td>
<td></td>
</tr>
<tr>
<td>88·9</td>
<td></td>
</tr>
</tbody>
</table>

Mean ... 89·7 of the latter.

The volumes of the remaining gas differ from each other, and so do the compositions of the different remaining portions. It always consisted for the most part of marsh-gas, but contained also a quantity of undecomposed olefiant gas, the vapours of tar and hydrogen. The quantities, however, in which these substances were found were different. The remaining gas burnt with a tolerably luminous flame, about the same as that of ordinary coal-gas.

When the olefiant gas was exposed to a full white heat, its volume was not diminished but augmented. A porcelain tube was closed at one end, and the other end was so connected with a receiver shut off with mercury that the gas might proceed without hindrance from the tube to the receiver, and from the latter could return to the tube. After the tube and the receiver had been filled with olefiant gas, the former was introduced into a blast-furnace and heated to whiteness for a distance of eighteen inches. In order that the gas might have full liberty to expand, the receiver which dipped pretty deep into the mercury was slowly lifted out of it, and afterwards during the cooling of the porcelain tube permitted to sink again. When at the conclusion of the experiment the gas was brought under the original pres-
sure and to the original temperature, its volume was found to be doubled. The investigation showed that it consisted almost wholly of pure hydrogen, which possessed scarcely the odour of tar. The olefiant gas behaved in a similar manner when it was conducted very slowly through a porcelain tube heated to whiteness. No tar was formed, a great quantity of carbon was, on the contrary, separated, and the gas that issued from the tube was composed almost wholly of pure hydrogen, free from the smell of tar.

From this it may be inferred that tar is formed only when the heat is that of redness, and by the action of a white heat is decomposed into carbon and hydrogen.

A nearer acquaintance with the process of its formation was only to be obtained by the examination of the composition of the tar. To obtain a quantity sufficient for analysis, the olefiant gas, purified as above, was conducted through a glass tube which had a diameter of 0.4 of an inch, and a length of which equal to 15 inches was maintained at a red heat. To condense the vapour of tar thus produced, it was conducted, in company with the decomposed gases, through a long thin glass tube whose diameter was only 0.2 of an inch. In this manner, in the course of a day, a quantity of tar sufficient for analysis was obtained, about 0.3 of a gramme.

When a non-volatile organic body is exposed to an invariable temperature at which it is decomposed, during the whole time of heating volatile products are formed, until a non-volatile compound remains behind. If, however, the temperature be increased, the latter compound will be also decomposed, and another non-volatile body remains. This process is repeated until at length carbon alone remains. If, therefore, the temperature be not completely constant, the formation of different volatile products is the consequence of its various changes, and such products also occur when the heated substance does not possess the same temperature throughout. This is, however, almost always the case, for at the sides of the containing vessel the heat is greater than at the centre. Hence in dry distillation different products of decomposition appear simultaneously; the more so, as not only the non-volatile remains of the substance are decomposed with increasing temperature, but also the volatile portions, if they are decomposable. This is the reason why the tar produced from olefiant gas is a mixture of various products of decomposition; for it is not possible to maintain the temperature of the red-hot tube, in which the tar is generated, perfectly constant. Besides this, the sides of such a tube are always hotter than the middle, which is cooled by the gas passing through the tube. Finally, the tar-vapour formed on entering
the tube is longer exposed to the heat and assumes a higher temperature than that formed at another portion of the tube, for example, near the end at which the gas leaves the tube.

Not always, therefore, was the tar found to possess the same composition; sometimes, as already remarked, it was darker, sometimes lighter; sometimes it was so volatile that it could not be weighed in an uncovered vessel. After having been some time exposed to the air, a portion was volatilized, and it was possible to weigh the remainder in a small porcelain vessel without a cover.

When volatile substances are decomposed, it is not necessary that the products of decomposition should be also volatile; and in the decomposition of olefiant gas, non-volatile products are also formed, for, as has been already remarked, the tar cannot be wholly distilled.

It would certainly have been desirable to ascertain the composition of each separate product of decomposition. But as we are not yet in a position to separate these products, I must be content with an analysis of the entire tar. It is, however, evident that it was not possible to obtain it always of the same character for analysis, and hence arise the divergences in the results.

The analyses were so conducted that the substance was weighed in a small porcelain vessel, and with the latter was introduced into a tube filled with the oxide of copper. During the combustion oxygen was conducted over the oxide. Tar possesses the disagreeable property of readily creeping, when warm, over the edge of the vessel which contains it, and then spreading itself over the interior of the hinder portion of the tube. To prevent this a further quantity of the oxide of copper was placed behind the porcelain vessel, which during the combustion was preserved at a red heat, so that the tar was immediately decomposed as soon as it came into contact with this oxide.

The analyses gave the following composition for the tar:

<table>
<thead>
<tr>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>I.</td>
<td>94·106</td>
<td>6·066</td>
<td>...</td>
<td>100·172</td>
</tr>
<tr>
<td>II.</td>
<td>92·461</td>
<td>6·652</td>
<td>0·887</td>
<td>100·000</td>
</tr>
<tr>
<td>III.</td>
<td>92·403</td>
<td>6·808</td>
<td>...</td>
<td>100·211</td>
</tr>
</tbody>
</table>

This composition agrees pretty well with that of naphthaline, which consists of:

Carbon . . . 93·75 per cent. = 5 C
Hydrogen . .  6·25       ...  2 H

\[
\text{100·00} = 5\ C + 2\ H.
\]

The odour of tar is also quite similar to that of naphthaline;

and I sometimes found, particularly when the more volatile portion had disappeared, small white crystals within the tar which were manifestly nothing else than naphthaline. We can therefore regard tar as a mixture of several hydrocarbons which are isomeric with naphthaline, or as a solution of naphthaline in such isomeric compounds.

If we assume that olefiant gas is resolved only into naphthaline and marsh-gas, then eight volumes of olefiant gas are necessary to form six volumes of marsh-gas and one equivalent of naphthaline.

\[ 1 \text{Naphthaline} = 5C + 2H \]
\[ 6 \text{Marsh-gas} = 3C + 6H. \]

Hence six-eighths, or 75 per cent. of the volume of the olefiant gas made use of, must remain as marsh-gas. In the experiments above cited, the quantity was always something more than 75 per cent. But, as already remarked, the whole of the olefiant gas was not decomposed; and a portion, although a small one, of the tar was also decomposed into carbon and hydrogen.

Olefiant gas alone furnishes a tar. Marsh-gas, on the contrary, remains quite unchanged by a temperature sufficient to soften the most stubborn Bohemian glass. At a white heat, however, it splits into carbon and hydrogen.

It may be concluded from this that the decomposition of the olefiant gas is so effected, that at a red heat it is decomposed into tar and marsh-gas; and that both of these, the tar as well as the marsh-gas, are again decomposed into carbon and hydrogen at a white heat.

Marsh-gas therefore may be regarded as a product of the decomposition of olefiant gas. But olefiant gas itself is always obtained by a decomposition, and is therefore to be regarded as a product thereof. That this body, however, which in its chemical composition and in its physical properties diverges so widely from organic bodies which furnish tar, should produce the same substance, was to me very surprising.

It is a singular circumstance that the generation of tar in olefiant gas has never before been observed; the more so, as among the numerous persons who exposed it to a high temperature several perceived an empyreumatic odour. The physicists of Holland mention it*, and Berthollet corroborates the statement†. G. Bischoff also ‡, on conducting the gas of a coal-mine through a red-hot porcelain tube, remarked an empyreumatic odour, which he states to have been similar to that of turpentine. He

also observed white vapours which coloured concentrated sulphuric acid brown, and found even some drops of a green tough oil. But he did not follow the subject further. Marchand, on the contrary, conducted the products of decomposition directly over oxide of copper, and determined them as carbonic acid and water, but could perceive nothing of the formation of tar.

It might indeed be assumed, that, considering the extensive application of this gas, the generation of tar from it must have been often observed. The reason why this has not been the case is, perhaps, to be referred partly to the circumstance that tar is formed at a red heat only, and at higher temperatures furnishes only carbon and hydrogen; partly to the fact that coal-gas itself always contains a slight admixture of the vapour of tar, so that even when fresh tar is generated at a red heat it escapes observation.

The origin of tar from olefiant gas shows that this is much more closely allied to the organic compounds, consisting of carbon, hydrogen and oxygen, particularly to those which at a high temperature form empyreumatic oils or tars, than marsh-gas. The easier decomposition of olefiant gas is also the reason why it enters into composition with other bodies, as chlorine and anhydrous sulphuric acid, more readily than marsh-gas.

As regards the fabrication of coal-gas, the experiments lead to the conclusion that tar, which always appears in company with this gas, is formed in two different ways; partly by the decomposition of the olefiant gas already generated, partly at the same time as the olefiant gas, directly from the substance of the coal. For even if the coal were not suited to the formation of olefiant gas, it would, notwithstanding, like most vegetable substances, as wood, cellulose, turf, sugar, and many others, form a tar or empyreumatic oil. This latter portion contains nitrogen, inasmuch as coal contains it, and furnishes the compounds which have recently grown to such importance, aniline, leucoline, &c. The tar produced by the decomposition of olefiant gas is free from nitrogen, and furnishes principally naphthaline.

LXVI. On a Property of the Caustic by Refraction of the Circle.

By A. Cayley, Esq.*

M. ST. LAURENT has shown (Gergonne, vol. xviii. p. 1), that in certain cases the caustic by refraction of a circle is identical with the caustic of reflexion of a circle (the reflecting circle and radiant point being, of course, properly chosen), and a very elegant demonstration of M. St. Laurent's theorems is

* Communicated by the Author.

2 F 2
Mr. A. Cayley on a Property of the Caustic

given by M. Gergonne in the same volume, p. 48. A similar method may be employed to demonstrate the more general theorem, that the same caustic by refraction of a circle may be considered as arising from six different systems of a radiant point, circle, and index of refraction. The demonstration is obtained by means of the secondary caustic, which is (as is well known) an oval of Descartes. Such oval has three foci, any one of which may be taken for the radiant point: whichever be selected, there can always be found two corresponding circles and indices of refraction. The demonstration is as follows:

Let $c$ be the radius of the refracting circle, $\mu$ the index of refraction; and taking the centre of the circle as origin, let $\xi, \eta$ be the coordinates of the radiant point, the secondary caustic is the envelope of the circle

$$\mu^2(x - a^2 + y - \beta^2) - (\xi - a^2 + \eta - \beta^2) = 0,$$

where $a, \beta$ are parameters which vary subject to the condition

$$a^2 + \beta^2 - c^2 = 0.$$

The equation of the variable circle may be written

$$\{\mu^2(a^2 + y^2 + c^2) - (\xi^2 + \eta^2 + c^2)\} - 2(\mu^2x - \xi)x - 2(\mu^2y - \eta)\beta = 0,$$

which is of the form

$$C + A\alpha + B\beta = 0;$$

the envelope is therefore

$$C^2 = c^2(A^2 + B^2).$$

Or substituting, we have for the equation of the envelope, i.e. for the secondary caustic,

$$\{\mu^2(a^2 + y^2 + c^2) - (\xi^2 + \eta^2 + c^2)\}^2 = 4c^2\{(\mu^2x - \xi)^2 + (\mu^2y - \eta)^2\},$$

which may also be written

$$\{\mu^2(a^2 + y^2 - c^2) - (\xi^2 + \eta^2 - c^2)\}^2 = 4c^2\mu^2(x - \xi)^2 + y - \eta^2;$$

and this may perhaps be considered as the standard form. To show that this equation belongs to a Descartes' oval, suppose for greater convenience $\eta = 0$, and write

$$\mu^2(a^2 + y^2 - c^2) - \xi^2 + c^2 = 2c\mu \sqrt{(x - \xi)^2 + y^2}.$$

Multiplying this equation by $1 - \frac{1}{\mu^2}$, and adding to each side

$$c^2\left(\mu - \frac{1}{\mu}\right)^2 + (x - \xi)^2 + y^2,$$

we have
by Refraction of the Circle.

\[
\left(1 - \frac{1}{\mu^2}\right)\left\{\mu^2(x^2 + y^2 - c^2) - \xi^2 + c^2\right\} + (x - \xi)^2 + y^2 + c^2\left(\mu - \frac{1}{\mu}\right)^2 = (x - \xi)^2 + y^2 + 2c\left(\mu - \frac{1}{\mu}\right)\sqrt{(x - \xi)^2 + y^2 + c^2\left(\mu - \frac{1}{\mu}\right)^2};
\]

or reducing,

\[
\mu^2\left\{\left(x - \frac{\xi}{\mu^2}\right)^2 + y^2\right\} = \left(\sqrt{(x - \xi)^2 + y^2 + c\mu - \frac{1}{\mu}}\right)^2.
\]

Again, multiplying the same equation by \(\frac{1}{\mu^2}\left(1 - \frac{c^2}{\xi^2}\right)\), and adding to each side \(\frac{\xi^2}{\mu^2}\left(1 - \frac{c^2}{\xi^2}\right) + \frac{c^2}{\xi^2}(x - \xi)^2 + y^2\), we have

\[
\frac{1}{\mu^2}\left(1 - \frac{c^2}{\xi^2}\right)\left\{\mu^2(x^2 + y^2 - c^2) - \xi^2 + c^2\right\} + \frac{c^2}{\xi^2}(x - \xi)^2 + y^2
\]

or reducing,

\[
\left(x - \frac{c^2}{\xi}\right)^2 + y^2 = \left\{\frac{c}{\xi}\sqrt{(x - \xi)^2 + y^2} + \frac{\xi}{\mu}\left(1 - \frac{c^2}{\xi^2}\right)\right\}^2.
\]

Hence, extracting the square roots of each side of the equations thus found, we have the equation of the secondary caustic in either of the forms

\[
\sqrt{(x - \frac{\xi}{\mu^2})^2 + y^2} = \frac{1}{\mu} \sqrt{(x - \xi)^2 + y^2} + \frac{c}{\mu}\left(\mu - \frac{1}{\mu}\right),
\]

\[
\sqrt{(x - \frac{c^2}{\xi})^2 + y^2} = \frac{c}{\xi} \sqrt{(x - \xi)^2 + y^2} + \frac{1}{\mu}\left(\xi - \frac{c^2}{\xi}\right);
\]

to which are to be joined

\[
\sqrt{(x - \frac{c^2}{\xi})^2 + y^2} = \frac{c\mu}{\xi} \sqrt{(x - \xi)^2 + y^2} + \frac{\xi}{\mu} - \frac{c^2\mu}{\xi},
\]

\[
c\left(\mu - \frac{1}{\mu}\right)\sqrt{(x - \frac{c^2}{\xi})^2 + y^2} + \left(-\xi + \frac{c^2}{\xi}\right)\sqrt{(x - \frac{\xi}{\mu^2})^2 + y^2}
\]

or

\[
\frac{\xi}{\mu} - \frac{c^2\mu}{\xi}\right) \sqrt{(x - \xi)^2 + y^2} = 0.
\]

Write successively,
Mr. A. Cayley on a Property of the Caustic

\[ \xi' = \xi \quad c' = c \quad \mu' = \mu \]  \hspace{1cm} (1)

\[ \xi' = c^2 = \xi \mu' = \frac{c}{\xi} \]  \hspace{1cm} (\alpha)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c' = \frac{c}{\mu} \quad \mu' = \frac{1}{\mu} \]  \hspace{1cm} (\beta)

\[ \xi' = \xi \quad c' = \frac{\xi}{\mu} \quad \mu' = \frac{\xi}{c} \]  \hspace{1cm} (\gamma)

\[ \xi' = c^2 = \frac{c^\mu}{\xi} \]  \hspace{1cm} (\delta)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c' = \frac{\xi}{\mu} \quad \mu' = \frac{\xi}{c^\mu} \]  \hspace{1cm} (\epsilon).

Or, what is the same thing,

\[ \xi = \xi' \quad c = c' \quad \mu = \mu' \]  \hspace{1cm} (1)

\[ \xi = \frac{\xi^2}{\mu^2} \quad c = \frac{c}{\mu} \quad \mu = \frac{\xi}{c^\mu} \]  \hspace{1cm} (\alpha)

\[ \xi = \frac{\xi^2}{\mu^2} \quad c = \frac{c}{\mu} \quad \mu = \frac{1}{\mu} \]  \hspace{1cm} (\beta)

\[ \xi = \frac{\xi^2}{\mu^2} \quad c = \frac{c}{\mu} \quad \mu = \frac{\xi}{c^\mu} \]  \hspace{1cm} (\gamma)

\[ \xi = c^2 \quad c = \frac{c}{\mu} \quad \mu = \frac{c^\mu}{\xi} \]  \hspace{1cm} (\delta)

\[ \xi = \frac{\xi^2}{\mu^2} \quad c = \frac{c}{\mu} \quad \mu = \frac{c^\mu}{\xi} \]  \hspace{1cm} (\epsilon).

Or, again,

\[ \xi' = \xi \quad c^2 = \frac{c^2}{\mu^2} \quad \frac{c^2}{\mu^2} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (1)

\[ \xi' = c^2 \quad c^2 = \frac{c^2}{\xi} \quad \frac{c^2}{\xi} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (\alpha)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c^2 = \frac{c^2}{\mu^2} \quad \frac{c^2}{\mu^2} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (\beta)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c^2 = \frac{c^2}{\mu^2} \quad \frac{c^2}{\mu^2} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (\gamma)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c^2 = \frac{c^2}{\mu^2} \quad \frac{c^2}{\mu^2} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (\delta)

\[ \xi' = \frac{\xi^2}{\mu^2} \quad c^2 = \frac{c^2}{\mu^2} \quad \frac{c^2}{\mu^2} = \frac{\xi}{\mu^2} \]  \hspace{1cm} (\epsilon).

Then, whichever system of values of \( \xi' \), \( c' \), \( \mu' \) be substituted for
by Refraction of the Circle.

We have in each case identically the same secondary caustic, the effect of the substitution being simply to interchange the different forms of the equation, and we have therefore identically the same caustic. By writing

\[(\xi', c', \mu') = (\xi, c, \mu) = \alpha(\xi, c, \mu), \&c.,\]

\(\alpha, \beta, \gamma, \delta, \epsilon\) will be functional symbols, such as are treated of in my paper "On the Theory of Groups as depending on the symbolic equation \(\theta^n = 1\)," and it is easy to verify the equations

\[
\begin{align*}
1 &= \alpha \beta = \beta \alpha = \gamma^2 = \delta^2 = \epsilon^2 \\
\alpha &= \beta^2 = \delta \gamma = \epsilon \delta = \gamma \epsilon \\
\beta &= \alpha^2 = \epsilon \gamma = \gamma \delta = \delta \epsilon \\
\gamma &= \delta \alpha = \epsilon \beta = \beta \delta = \alpha \epsilon \\
\delta &= \epsilon \alpha = \gamma \beta = \alpha \gamma = \beta \epsilon \\
\epsilon &= \gamma \alpha = \delta \beta = \beta \gamma = \alpha \delta.
\end{align*}
\]

Suppose, for example, \(\xi = -c\), i.e. let the radiant point be in the circumference; then in the fourth system \(\xi' = -c\), \(c' = -\frac{c}{\mu}\), (or, since \(c'\) is the radius of a circle, this radius may be taken \(\frac{c}{\mu}\)), \(\mu' = -1\), or the new system is a reflecting system. This is one of M. St. Laurent's theorems, viz.

**Theorem.** The caustic by refraction of a circle when the radiant point is on the circumference, is the caustic by reflexion for the same radiant point, and a concentric circle the radius of which is the radius of the first circle divided by the index of refraction.

Again, if \(\xi = -c\mu\), the fifth system gives \(\xi' = \frac{c^2}{\xi}, c' = c, \mu' = -1\), or the new system is in this case also a reflecting system. This is the other of M. St. Laurent's theorems, viz.:

**Theorem.** The caustic by refraction of a circle when the distance of the radiant point from the centre is equal to the radius of the circle multiplied by the index of refraction, is the caustic by reflexion of the same circle for a radiant point the image of the first radiant point.

Of course it is to be understood that the image of a point means a point whose distance from the centre = square of radius + distance.

2 Stone Buildings, Nov. 2, 1853.
LXVII. **On a proposed Barometric Pendulum, for the Registration of the Mean Atmospheric Pressure during long Periods of Time.**

By W. J. MACQUORN RANKINE, C.È., F.R.SS. Lond. and Edinb. &c.*

Introductory Remarks.

**WHEN** this paper was read, I was not aware that a barometric pendulum formed part of an instrument sent by Mr. G. F. Hall to the Great Exhibition of 1851. There is this difference, however, between Mr. Hall's pendulum and that here proposed, that the former oscillates in a plane, while the latter is intended to revolve. It appears to me that the revolving pendulum will possess a considerable advantage, inasmuch as the mercury will remain steady in the tube during each revolution, instead of rising and falling with each vibration, as it would do in the oscillating pendulum.

It has been objected to the proposed barometric clock, that its introduction would be superfluous, owing to the great accuracy which has been attained in the photographic method of registration. With all due deference to the well-known skill and experience of Mr. Welsh, with whom I understand that this objection originated, I must decline to admit its relevancy; for I cannot assent to the principle, that the excellence of an existing mode of recording natural phenomena ought to form a bar to the introduction of any new mode in other respects unobjectionable. It appears to me, on the contrary, that much instruction is to be gained by recording the same phenomena by different processes. There is this important difference between the photographic and the chronometric methods of ascertaining the mean barometric pressure during long periods,—that an integration, which in the former process has to be made by measuring and calculating the contents of a curvilinear figure described on a series of photographic plates, is performed in the latter process by the barometric clock itself.

Glasgow, Oct. 27, 1853.

W. J. MACQUORN RANKINE.

The influence of temperature on the rate of clocks and watches has naturally suggested to more than one person the idea of employing the variations of the rate of a clock, regulated by a pendulum of suitable expansibility, to register the mean temperature during long periods of time.

I have now to propose another and a somewhat similar appli-

* Communicated by the Author, having been read to the British Association for the Advancement of Science, Section A, at Hull, September 10, 1853.
On a proposed Barometric Pendulum.

The demonstration of the variations of the rate of a clock, viz. to determine the mean barometric pressure during long periods.

For this purpose the clock should be regulated by a centrifugal or revolving pendulum, part of which should consist of a siphon barometer. The rising and falling of the mercury would evidently affect the rate of the clock; so that from the number of revolutions of the pendulum in a given time might be deduced approximately the mean height of the mercurial column during that period.

The formula applicable to this calculation are the following, the demonstrations of which are given in an Appendix.

The suffix 0 being used to designate a certain normal condition of the barometric pendulum as to temperature and pressure, to be employed as a standard of comparison, let

\[ Y_0 \] represent the distance, in this condition, of the centre of gravity of the pendulum from the centre of suspension;

\[ \lambda_0 \] the distance of the centre of oscillation from the centre of suspension, or the length of the equivalent simple pendulum;

\[ F_0 \] the distance from the centre of suspension to the lowest part of the central line of the siphon;

\[ L_0 \] the sum of the lengths of the mercurial columns in the two legs of the siphon, measured from the same point;

\[ H_0 \] the difference of those lengths, which may be called the oblique height of the barometric column;

\[ l_0 \] the length of a column of mercury, of sectional area equal to that of the column in the siphon, whose weight would be equal to the whole weight of the pendulum.

Let \( \Delta \) be used to designate deviations from this normal condition.

Then the deviation of the square of the oblique height is connected by the following equation with the deviation of the length of the equivalent simple pendulum

\[ \Delta, \bar{H} = \frac{4l_0Y_0\Delta\lambda}{\lambda_0 + \Delta\lambda - 2F_0 + L_0} \]  

(1)*

If the weight and dimensions of the barometric pendulum be so adjusted that the deviations of the length of the equivalent simple pendulum are always very small compared with the normal length, then the above formula may be used to calculate the mean deviation of the square of the oblique height \( \bar{H} \) by means of the mean deviation of the length of the equivalent simple pendulum.

The latter deviation is to be determined as follows:—

Let \( n_0 \) be the number of revolutions of the pendulum during a given period in the normal condition;

\( n_0 + \Delta n \) the actual number of revolutions;

* See Appendix 1.
then the mean deviation of the length of the equivalent simple pendulum is given approximately by the formula

$$\Delta \lambda = -2\frac{\lambda_0}{n_0} \Delta n \text{ nearly; . . . . (2)}$$

which value being substituted in equation 1, transforms it to the following,

$$\Delta . H^2 = \frac{-8\lambda_0 \Delta n}{n_0 \left(1 - \frac{2F_0 - L_0}{\lambda_0}\right) + 2\Delta n} \text{ nearly; . . . . (3)}$$

and gives the mean deviation of the square of the actual oblique barometric height from the square of the normal oblique barometric height.

We thus obtain the mean of the squares of the oblique height during the period of registration, which may be represented thus:

$$m . H^2 = H_0^2 + \Delta . H^2 . . . . . . \ (4)$$

The square root of this may be taken as a first approximation to the mean value of the oblique height.

To obtain a nearer approximation, let $m . h$ be the isotropic mean of the maximum deviations upwards and downwards, from the first approximate value of the mean height, which deviations may be ascertained by other instruments; then a second approximation to the mean oblique height is

$$m . H = \sqrt{m . H^2 - \frac{1}{2} (m . h)^2} . . . . . (5)$$

The several quantities denoting lengths in these formulae are supposed to have been corrected for expansion; the mean deviation from the normal temperature having been ascertained by a thermometric pendulum, or otherwise.

The quantity $m . H$, thus ascertained and corrected for temperature, has finally to be corrected for the obliquity of the position of the barometer, and for centrifugal force, by the following formula:

Let $\alpha$ denote the angle made by the pendulum with the vertical;

$T$, the mean time of one revolution;

$m . H'$, the corrected mean height of the barometric column;

then

$$m . H' = m . H \left\{ \cos \alpha + \frac{2\pi^2 \sin^2 \alpha}{gT^2} (2F - L) \right\} . . . . . (6)$$

The first term between the brackets represents the effect of obliquity; the second, that of centrifugal force.

* See Appendix 2.
† See Appendix 3.
for the Registration of the Mean Atmospheric Pressure.

In Appendix 4, it is shown how the foregoing formulæ may be simplified under certain special circumstances.

I shall not at present attempt to assign the best proportions for the barometric pendulum, nor the best manner of constructing it, for those particulars will be most readily fixed by practical trials. When this has been accomplished, it appears to me probable that the method here proposed of ascertaining the mean barometric pressure during long periods of time may probably be found more accurate than any other, as well as less laborious.

Glasgow, July 9, 1853.

APPENDIX.

INVESTIGATION OF THE FORMULÆ.

1. Relation between the deviations of the oblique height of the mercurial column and those of the centre of oscillation.

The point of suspension being taken as the origin of $x$, measured along the pendulum, let $x_1$ denote the position of the surface of the mercury in the closed leg of the siphon, and $x_2$ its position in the open leg, in the normal condition as to pressure and temperature. Then

$$x_2 + x_1 = 2F_0 - L_0; \quad x_2 - x_1 = H_0;$$

$$\Delta x_1 = -\frac{1}{2} \Delta H; \quad \Delta x_2 = +\frac{1}{2} \Delta H.$$

If $d\mu$ denote an element of the mass of the pendulum, and $\mu$ its entire mass, the length of the equivalent simple pendulum revolving at the same angle with the vertical is known to be

$$\lambda = \frac{\int x^2 d\mu}{\int x d\mu} = \frac{\gamma_0 \lambda_0 \mu + \Delta \int x^2 d\mu}{\gamma_0 \mu + \Delta \int x d\mu}.$$

Now, $M$ being the mass of unity of volume of mercury, and $\omega$ the sectional area of the siphon,

$$\Delta \int x d\mu = M\omega \left\{ \frac{1}{2} \int_{x_1}^{x_2} - \frac{1}{2} \int_{x_2}^{x_3} \Delta H \right\} (x dx)$$

$$= -M\omega \left\{ \frac{1}{2} (x_2 - x_1) \Delta H + \frac{1}{4} (\Delta H)^2 \right\} = -\frac{M\omega}{4} \Delta . H^2$$

$$\Delta \int x^2 d\mu = M\omega \left\{ \frac{1}{2} \int_{x_1}^{x_2} - \frac{1}{2} \int_{x_2}^{x_3} \Delta H \right\} (x^2 dx)$$

$$= -M\omega \left\{ \frac{1}{2} (x_2^2 - x_1^2) \Delta H + \frac{1}{4} (x_2 + x_1)(\Delta H)^2 \right\}$$

$$= -\frac{M\omega}{4} (2F_0 - L_0) \Delta . H^2.$$
Consequently, making
\[ \frac{\mu}{M \omega} = l_\omega \]
we have
\[ \lambda = \lambda_0 + \Delta \lambda = \frac{\gamma_0 \lambda_0 - \frac{1}{4l_0} (2F_0 - L_0) \Delta \cdot H^2}{\gamma_0 - \frac{1}{4l_0} \Delta \cdot H^2}, \quad (B) \]
from which is immediately deduced equation 1 of the paper.

2. Periodic Time of Revolution.

This quantity is well known to be expressed by
\[ T = \sqrt{\left(\frac{4\pi^2}{g} \lambda \cos \alpha\right)}, \quad \ldots \ldots \quad (C) \]
consequently the number of revolutions in a given time \( \Theta \) is
\[ n = \frac{\Theta}{T} = \frac{\Theta}{\sqrt{\left(\frac{4\pi^2}{g} \lambda \cos \alpha\right)}}. \quad \ldots \ldots \quad (D) \]
We have therefore
\[ \frac{n^2}{n_0^2} = \frac{\lambda_0}{\lambda}, \quad \ldots \ldots \ldots \ldots \quad (E) \]
and consequently, when \( \Delta \lambda \) is very small,
\[ \frac{\Delta \lambda}{\lambda_0} = -2 \frac{\Delta n}{n_0} \text{ nearly}, \]
being equivalent to equation 2 of the paper.

3. Condition of Equilibrium of the Mercury.

\( H' \) being the vertical height of a column of mercury whose weight is equivalent to the atmospheric pressure, may be used to express the action of the air on the open surface of the mercury.

The effect of gravity will be proportional to
\[ H \cos \alpha, \]
and that of centrifugal force to
\[ \frac{4\pi^2 \sin^2 \alpha}{gT^2} \int_{x=\frac{1}{2} \Delta H}^{x=-\frac{1}{2} \Delta H} x dx = H \cdot \frac{2\pi^2 \sin^2 \alpha}{gT^2} (2F_0 - L_0); \]
consequently the condition of equilibrium of the mercury is expressed by the following equation,
for the Registration of the Mean Atmospheric Pressure.

\[ H' = H \left( \cos \alpha + \frac{2\pi^2 \sin^2 \alpha}{gT^2} (2F_0 - L_0) \right), \]  

of which equation 6 of the paper is a particular case.

4. Simplification of the Formulae by particular adjustments.

If we make \(2F_0 = L_0\); that is to say, if we so place the siphon barometer that the normal position of a point midway between the surfaces of the mercury in its two legs shall be the centre of suspension of the pendulum, then equation 1 becomes

\[ \Delta \cdot H^2 = 4d \gamma_0 \cdot \frac{\Delta \lambda}{\lambda_0 + \Delta \lambda}. \]

Now

\[ \frac{\Delta \lambda}{\lambda_0 + \Delta \lambda} = \frac{\lambda - \lambda_0}{\lambda} = 1 - \frac{\lambda_0}{\lambda} = 1 - \frac{n^2}{n_0^2} = -\frac{\Delta \cdot n^2}{n_0^2}; \]

consequently

\[ \Delta \cdot H^2 = -4d \gamma_0 \cdot \frac{\Delta \cdot n^2}{n_0^2}. \]  

(G)

Should the mean oblique barometric height be known approximately previously to the construction of the pendulum, let this first approximate value be taken for the normal oblique height \(H_0\), and let the weight and the centre of gravity of the pendulum be so adjusted that

\[ H_0^2 = 4d \gamma_0; \]

then we shall have

\[ \frac{\Delta \cdot H^2}{H_0^2} = -\frac{\Delta \cdot n^2}{n_0^2}; \]  

(H)

and the following formula will be very nearly exact,

\[ \Delta H = -\frac{H_0 \Delta n}{n_0}. \]  

(K)

So that a second and very close approximation to the mean oblique height, when the adjustments above described have been made, may be found by the following formula,

\[ m \cdot H = H_0 \left( 1 - \frac{\Delta n}{n_0} \right). \]  

(L)

As the correction for centrifugal force in this case disappears, equation 6 is reduced to

\[ m \cdot H' = m \cdot H \cos \alpha. \]  

(M)

Our knowledge of cyclones is limited to that mature condition of the phenomenon in which it has become a hurricane: nothing is known of their origin, and very little of their gradual development. It might be anticipated that some light would be thrown on these important points by investigating and comparing the corresponding phases of the apparently analogous phenomena of waterspouts, tornados, &c. This comparison seems to have been somewhat too hastily abandoned by cyclonologists on meeting with some cases in which the law of rotation appeared to be violated. If this discrepancy had been well established, it might be thought fatal to the hypothesis of the homogeneity of the two meteors; but the observations of the direction of rotation of waterspouts are both too few-and too uncertain to outweigh the numerous remarkable coincidences which tend to show that cyclones and waterspouts differ only in degree; a waterspot being either a cyclone in miniature, or an embryo cyclone. In several instances two or three waterspouts have been seen within the area of one cyclone, which confirms the supposition that both meteors are produced by the same physical agent. Each has a motion of rotation about a vertical axis, and another of translation along the surface of the earth. Both arise after extreme heat, and travel towards a colder region. The central portions of both are characterized by an excessively low temperature and extreme rarefaction; by copious falls of rain, snow and hail; by peculiar noises; by lightning, and other manifestations of the presence of electricity. Further research will disclose more points of close resemblance. An inquiry into the origin and nature of waterspouts, tornados, and other apparently cognate phenomena, may therefore lead to valuable suggestions respecting the more important but less known phenomenon called a cyclone.

The following appear to be the main facts which are available as a basis for a theory which shall comprehend all the meteors in question.

1. The eruption of a submarine volcano has produced waterspouts.

"During these bursts the most vivid flashes of lightning continually issued from the densest part of the volcano, and the columns of smoke rolled off in large masses of fleecy clouds, gradually expanding themselves before the wind in a direction
nearly horizontal, and drawing up a quantity of waterspouts.”—Capt. Tilland’s description of the upheaval of Sabrina Island in June 1811, Phil. Trans.

With this significant fact may be compared the following analogous ones:

“In the Aleutian Archipelago a new island was formed in 1795. It was first observed after a storm, at a point in the sea from which a column of smoke had been seen to rise.”—Lyell, Principles of Geology.

“Among the Aleutian Islands a new volcanic island appeared in the midst of a storm, attended with flames and smoke. After the sea was calm, a boat was sent from Unalaska with twenty Russian hunters, who landed on this island on June 1, 1814.”—Journal of Science, vol. vii.

“On July 24, 1818, a submarine eruption broke out between the mainland of Orkney and the island of Stronsa. Amid thunder and lightning a very dense jet-black cloud was seen to rise from the sea, at a distance of five or six miles, which travelled towards the north-east. On passing over Stronsa, the wind from a slight air became a hurricane, and a thick, well-defined belt of large hailstones was left on the island. The barometer fell two inches.”—Trans. Roy. Soc. Edinb. vol. ix.

2. Hurricanes, whirlwinds, and hailstones accompany the paroxysms of volcanos.

“1730. A great volcanic eruption at Lancerote Island, and a storm, which was equally new and terrifying to the inhabitants, as they had never known one in the country before.”—Lyell, Principles of Geology, vol. ii.

“1754. In the Philippine Islands a terrible volcanic eruption destroyed the town of Taal and several villages. Darkness, hurricanes, thunder, lightning, and earthquakes alternated in frightful succession.”—Edinb. Phil. Journal.

“In 1805, 1811, 1813 and 1830, during eruptions of Etna, caravans in the desert of Africa perished by violent whirlwinds. In 1807, while Vesuvius was in eruption, a whirlwind destroyed a caravan.”—Rev. W. B. Clarke in Tasm. Journal.

“1815. Java. A tremendous eruption of the Tombow Mountain. Between 9 and 10 P.M. ashes began to fall, and soon after a violent whirlwind took up into the air the largest trees, men, horses, cattle, &c.”—Raffles’s History of Java.


“1843, Jan. Etna in eruption. Violent hurricanes at Genoa, in the Bay of Biscay, and in Great Britain.

“1843, Feb. Destructive earthquakes in the West Indies, a volcanic eruption at Guadaloupe, followed by hurricanes in the Atlantic.


“1847, March 20. Volcanic eruption and earthquake in Java; and on the 21st of March and 3rd of April violent hurricanes.”—Java Courant.

“1851, Aug. 5. A frightful eruption of the long dormant volcano of the Pelee Mountain, Martinique. Aug. 17, hurricane at St. Thomas's, &c.; earthquake at Jamaica, &c.

“1852, April 14. Earthquake at Hawaii, and on the 15th a great volcanic eruption. On the 18th a gale of unusual violence lasted thirty-six hours, and did great damage.”—The Polynesian, April 22, 1852.

3. In volcanic regions, earthquakes and hurricanes often occur almost simultaneously, but in no certain order, and without any volcanic eruption being observed.

In 1712, 1722, 1815 and 1851, earthquakes and hurricanes occurred together at Jamaica. In 1762 at Carthagena; in 1780 at Barbadoes; in 1811 at Charleston; in 1847 at Tobago; in 1837 and 1848 at Antigua; in 1819 an awful storm at Montreal, rain of a dark inky colour, and a slight earthquake. People conjectured that a volcano had broken out. In 1766 the great Martinique hurricane, a waterspout burst on Mount Pelee and overwhelmed the place. Same night, an earthquake.

1843, Oct. 30. Manilla. Twenty-four hours' rain and two heavy earthquakes. 10 p.m. a severe hurricane.

“1852, Sept. 16. Manilla. An earthquake destroyed a great part of the city; many vessels wrecked by a great hurricane in the adjacent seas between the 18th and 26th of September.”—Singapore Times.

“1737, Oct. Calcutta. Furious hurricane and violent earthquake; 300,000 lives lost.


“1800. Ongole, India, and in 1815 at Ceylon, a hurricane and earthquake shocks.”—Piddington.
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"1848. Cyprus. An earthquake and a frightful hurricane."
—Hecker.

"1819. Bagdad. An earthquake and a storm, an event quite unprecedented.


"1836, Oct. At Valparaiso a destructive tempest and severe earthquakes."—Naut. Mag. 1848.

When an earthquake of excessive intensity occurs, as at Lisbon in 1755, the volcanic craters, which act as the safety-valves of the regions in which they are placed, are supposed to be sealed up; and it is a remarkable and highly suggestive fact, that no hurricane follows such an earthquake. The number of instances of the concurrence of ordinary earthquakes and hurricanes might easily be increased, but the preceding suffice to show the generality of this coincidence both as to time and place.

4. The breaking of waterspouts on mountains sometimes accompanies hurricanes.

In 1766, during the great Martinique hurricane, before cited.

"1826, Nov. At Teneriffe, enormous and most destructive waterspouts fell on the culminating tops of the mountains, and a furious cyclone raged around the island. The same occurred in 1812 and in 1837."—Espy and Grey's Western Australia.

"1829. Moray. Floods and earthquakes, preceded by waterspouts and a tremendous storm."—Sir T. D. Lauder.


5. The fall of an avalanche sometimes produces a hurricane.

"1819, Dec. A part (360,000,000 cubic feet) of the glacier fell from the Weisshorn (9000 feet). At the instant when the snow and ice struck the inferior mass of the glacier, the pastor of the village of Randa, the sacristan, and some other persons observed a light. A frightful hurricane immediately succeeded."

6. Waterspouts occur frequently near active volcanos.

This is well known with regard to the West Indies and the Mediterranean. The following notices refer to the Malay Archipelago and the Sandwich Islands:

to Singapore. In Oct. 1841, I saw six in action attached to one cloud. In Aug. 1838 one passed over the harbour and town of Singapore, dismasting one ship, sinking another, and carrying off the corner of the roof of a house in its passage landward.”—Journal of Indian Archipelago.

“1809. An immense waterspout broke over the harbour of Honolulu. A few years before one broke on the north side of the island (Oahuan), washed away a number of houses, and drowned several inhabitants.”—Jarves's Hist. of Sandwich Islands.

7. Cyclones begin in the immediate neighbourhood of active volcanos.

The Mauritius cyclones begin near Java; the West Indian near the volcanic series of the Caribbean Islands; those of the Bay of Bengal near the volcanic islands on its eastern shores; the typhoons of the China sea near the Philippine Islands, &c.

8. Within the tropics, cyclones move towards the west; and in middle latitudes, cyclones and waterspouts move towards the N.E., in the northern hemisphere, and towards the S.E. in the southern hemisphere.

9. In the northern hemisphere, cyclones rotate in a horizontal plane in the order N.W.S.E.; and in the southern hemisphere, in the order N.E.S.W.

By applying the principles of electro-dynamics to the electricity of the atmosphere, I shall endeavour to connect and explain the preceding well-defined facts. The continuous observations of Quetelet on the electricity of the atmosphere from 1844 to 1849 (Literary Journal, Feb. 1850), show that it is always positive, and increases as the temperature diminishes. It therefore increases rapidly with the height above the earth's surface. We may consequently regard the upper and colder regions of the atmosphere as an immense reservoir of electric fluid enveloping the earth, which is insulated by the intermediate spherical shell formed by the lower and denser atmosphere. Now, whenever a vertical column of this atmosphere is suddenly displaced, the surrounding aqueous vapour will be immediately condensed and aggregated, and the cold rarefied air and moisture of the column will form a vertical conductor for the descent of the electric fluid. This descent will take place down a spiral, gyrating in the order N.W.S.E. in the northern hemisphere, since the electric current is under the same influence as that of the south pole of a magnet; and in the order N.E.S.W. in the southern hemisphere. The air exterior to the conducting cylinder will partake of the violent revolving motion, and a tornado or cyclone will be produced. The facts marked 9 are thus accounted for, as well as the numerous circumstances indicative of intense electrical action,
such as the appearance of fiery meteors, the loss of sight, the deranged action of the needle, &c.

As long as the integrity of the conducting column is maintained and the supply of electric fluid continued, the aqueous vapour through which a cyclone passes will be condensed and accumulated. Hence arises the immense fall of water (always fresh) when a waterspout breaks, and the excessive rains that accompany the passage of the central space of a cyclone. When the condensation is sufficiently sudden and intense, hail and even ice may be formed. The fact marked 4 is thus explained.

A physical mechanism adequate to the formation of the conducting cylinder presents itself in the form of an avalanche (fact 5), and in the sudden ejection from a volcanic crater of long pent-up and highly elastic gases (facts 1, 2, 6 and 7). It follows, moreover, that a waterspout, a tornado, or even a cyclone, may be produced by anything which tends to form a vertical column of considerably less density, or of much greater humidity, than the surrounding atmosphere. Hence the well-known fact that storms follow great battles, great bush fires, &c.

When an earthquake and a cyclone occur nearly together in the neighbourhood of a volcano, the earthquake indicates the activity of the volcanic forces, and the cyclone bears evidence of a sudden ejection of gases from the crater, although no eruption of flame or lava may have taken place or been observed. (Fact 3.) The atmosphere will be most highly charged with electricity, and therefore the tendency to a violent restoration of electrical equilibrium will be most powerful just after the season of greatest heat, during which the processes of evaporation and vegetation have been most active, and therefore the development and accumulation of atmospheric electricity most rapid. Accordingly it is found that cyclones generally originate in tropical regions; that the hurricane season in any locality is the same as the season of greatest heat; and that waterspouts, tornados, and hailstorms occur on very hot days. The terrestrial electric currents flow towards the magnetic west, and must therefore decline towards the south in passing from Java towards the Mauritius, and towards the north in passing from the Caribbean Isles (Barbadoes, Martinique, &c.) towards Cuba. This explains the directions of the two great cyclone tracts in the South Indian Ocean and in the West Indies, up to the points where they are found to recurve, but the facts enunciated in (8) concerning the direction of a cyclone’s track in middle latitudes remain unaccounted for. With this exception, the electro-dynamical theory of cyclones here proposed will account for every important and definite fact connected with the phenomenon, and will likewise embrace the obviously allied phenomena of waterspouts, tornados, hail-storms, &c.
It follows from this theory, that hurricanes are most likely to be produced by such volcanic eruptions as succeed seasons of great heat, and that the eruption of a low volcano is more likely to produce a hurricane than that of a high one, since the whole of the insulating atmospheric shell is pierced through in the former case, and only the upper portion of it in the latter.

April 18, 1853.

LXIX. Fragment on Multiplicity of Values. By James Cockle, M.A., of Trinity College, Cambridge; Barrister-at-Law of the Middle Temple*.

I. In the Introduction to his Mémoire sur le nombre de valeurs que peut prendre une fonction quand on y permute les lettres qu'elle renferme, published in Liouville's Journal for January 1850, M. J. A. Serret has given an outline of the history of the question. Were it sought to add to the learned analyst's sketch indications of collateral, but allied, researches, we should have to make mention of the names of Descartes, Hudde, Saunderson, Le Sueur, Euler, Foncecenx, Vandermonde; Lagrange, Laplace, Malfatti, Abel, Sir W. R. Hamilton, Badanjo, Bronwin, and G. B. Jerrard, as well as of others††.

II. Some suggestions may perhaps arise from the following mode of considering the subject. Let

\[ a_1(y) + a_2(y) + \cdots + a_m(y) = a(y) \]
\[ e_1(y) + e_2(y) + \cdots + e_m(y) = c(y) \]
\[ a_1(y) + a_2(y) + \cdots + a_m(y) = \omega(y), \]

* Communicated by the Author.
‡ Crelle's Journal, vol. i. pp. 65–84. (Berlin, 1826.)
§ Trans. Royal Irish Acad. vols. xviii. (pp. 171–259) and xix. The latter contains (pp. 329–376) the criticism on Badano's admirable researches.
** Phil. Mag. Suppl. for June 1845 (S. 3. vol. xxvi. pp. 545–574); for January 1846 (S. 3. vol. xxvii. p. 63); for February 1852 (S. 4. vol. iii. pp. 112–116); for June 1852 (Ibid. pp. 457–460); for December 1852 (S. 4. vol. iv. p. 434). I have pointed out certain objections which, as it appeared to me, might be taken to Mr. Jerrard's views (see Phil. Mag. for December 1849, pp. 436, 437), and I have further pursued the subject of quintics at pp. 505–507 of vol. lv. of the Mechanics' Magazine. I may mention that in the last line but one of the second column of the page last mentioned (507) \( x^{n-r} \) should be put in place of \( x^n \).
†† Woolhouse, Kirkman, Mease, Cayley, Salmon, and, I think, others.
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where
\[ a_{1}(y) = a_{1}(y) \times e_{1}(y), \quad a_{2}(y) = a_{2}(y) \times e_{2}(y), \ldots, \]
\[ a_{m}(y) = a_{m}(y) \times e_{m}(y), \]
and \( a \) and \( e \) are functions of one or more of the \( n \) quantities \( y, y_2, \ldots, y_n \).

III. Suppose that any one of the functions, say \( a_p(y) \), can be derived from any other of them, say \( a_1(y) \), by some change which we may represent* by

\[ (1, 2, \ldots, n) \]
\[ (a_p, \beta_p, \ldots, y_p), \]
then I apply to the functions \( a \) the term *commetric.*

IV. When the functions \( a(y) \) and \( e(y) \) are symmetric, and the functions \( a(y), e(y) \), and \( a(y) \) are commetric, I term \( \varphi(y) \), if it be not symmetric, an *epimetric* function.

V. Let the function \( a(y) \) have only \( m \) values, then I call \( \varphi(y) \) a *pure* epimetric function; and the application to it of any change will produce another pure epimetric function, in which the values of \( a(y) \) will recur in a different order.

VI. We may in such case consider the functions \( a(y) \) and their order as permanent, and, without regarding the portion \( a \) of \( \varphi \), confine our attention to the changes which can take place in the portion \( e \), and which we may term the *epimetric interchanges.*

VII. By an interchange I mean an operation which introduces no *new* value of \( y \) into a function. I denote it by the peculiar brackets \( \{ \} \). An epimetric interchange, which operates on the functions \( e \) *successively*, I represent by \( e\{ \} \). Among the number of interchanges (or rather of interchangeable values) the primitive function is included.

VIII. The number of values which a pure epimetric function can take is a function of the number of epimetric interchanges. In other words, if we denote by \( f(\varphi) \) the number of values of \( \varphi(y) \), supposed pure, and by \( e_r \) the number of interchangeable values of \( e_r(y) \), we have

\[ f(\varphi) = \phi(e) = e\psi(e), \]

\( e_r \) being the same for all values of \( r \), and \( \psi(e) \) representing the number of values which, \( e_r \) remaining unchanged, the residue of the function can take.

IX. Without seeking now to determine \( \phi \) in all its generality, let us consider the function \( U_4 \) which occurs in my application of the method of symmetric products to equations of the fifth degree (Phil. Mag. for March 1853, p. 174).

X. If in place of \( U_4 \) we write \( -5u \), and make

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\[ a_r(y) = y_r^2, \quad e_1(y) = y_3y_5 + y_3y_4 \]
\[ e_2(y) = y_3y_5 + y_3y_4, \quad e_3(y) = y_1y_5 + y_2y_4 \]
\[ e_4(y) = y_1y_2 + y_3y_5, \quad e_5(y) = y_1y_4 + y_2y_3 \]

these relations, which result in

\[ \alpha(y) = u \quad \text{and} \quad 5 = m = n, \]

indicate that \( u \), and consequently \( U_4 \), is a pure epimetric function.

XI. It would seem that \( \Psi \) can be determined by ascertaining the number of values which, \( e_1 \) being permanent, any one of the other functions \( e \) can take. But,

for \( e_2 \) and \( e_5 \) \( \left( ^{34} \right) \) and for \( e_3 \) and \( e_4 \) \( \left( ^{25} \right) \)

are the only admissible changes. Hence, whichever of these four functions we consider, the number of interchanges is the same, and we have

\[ \epsilon = 3, \quad \Psi(e) = 2 = \epsilon - 1 \]
\[ \phi(e) = e \Psi(e) = e(\epsilon - 1) = 3 \times 2 = 6, \]

and the function \( u \) has only six values, a result arrived at by M. J. A. Serret (Liouville, vol. xv. p. 62).

XII. In fact, there exist the relations*

\[ u \left( ^{25} \right) = u \left( ^{34} \right) = u \left\{ \begin{array}{c} 2345 \\ 5432 \end{array} \right\}, \]

the last (epimetric) interchange being applied to the functions \( e \) successively; and, since any change applied to \( u \) will affect all but one of the five functions \( e \), the proper epimetric interchange must have the same effect. Now there is only one such interchange, that just given, consequently \( \Psi(e) \) is equal to 2.

XIII. From any other of the functions

\[ a_r(y) \quad \text{or} \quad y_r^q(y_3y_q + y_3y_t), \]

we might have obtained the relations

\[ u \left( ^{pq} \right) = u \left( ^{st} \right) = u \left\{ \begin{array}{c} pqst \\ qpts \end{array} \right\}, \quad \epsilon = 3, \]

and have observed that the portions \( p, q \) and \( s, t \) of the epimetric interchange have no effect on \( a_p, a_q \) and \( a_s, a_t \) respectively.

XIV. The foregoing must be regarded as the merest opening of the subject, in reference to a particular case. It will, however, be at once remarked, that no such pure epimetric as that just considered can occur when \( n \) is even. I shall now proceed to view the subject of multiplicity under another aspect.

XV. Let \( w \) be a function of the \( n \) quantities \( y \) and of the \( n \)th degree, or, in the nomenclature of my Analysis of the Theory of

\[ * u \left( ^{25} \right) = (a_3(y) + a_4(y)) \left( ^{25} \right) + (a_5(y) + a_6(y)) \left( ^{34} \right). \]
Equations*, an \( n \)-ary \( m \)-ic function. Also let

\[
\frac{dw}{dy_1} + \frac{dw}{dy_2} + \ldots + \frac{dw}{dy_n} = \sigma(w),
\]

and, in general,

\[
\left( \frac{d}{dy_1} + \frac{d}{dy_2} + \ldots + \frac{d}{dy_n} \right)^w = \sigma^r(w).
\]

Then if \( \sigma(w) \) be symmetric, I call \( w \) a \textit{hyposymmetric} function. If \( \sigma^r(w) \) be the first of the functions symmetric with respect to \( y \), I call \( w \) an \textit{‘unsymmetric} function of the \((r-1)\)th class\textit{.’} When none of the functions \( \sigma \) is symmetric, \( w \) is \textit{‘unsymmetric’}.

XVI. Let \( w' \) be what \( w \) becomes when each of the quantities \( y \) receives an equal increment \( y' \), and let

\[
\chi(y') = \sigma(w)y'' + \sigma^2(w)\frac{y'^2}{1.2} + \text{&c}.;
\]

then

\[
w' = w + \chi(y'), \quad w = w' - \chi(y').
\]

XVII. If the substitution of \(-y_r\) for \( y' \) change \( w \) into \( w_r \), we have

\[
w = w_r - \chi(-y_r) = \rho(r),
\]

for all values of \( r \) from 1 to \( n \), both inclusive. Hence

\[
w = \rho(1) = \rho(2) = \ldots = \rho(n).
\]

XVIII. But, when \( w \) is \textit{hyposymmetric}, \( \chi \) and \( w_r \) and \( \rho \) are respectively \textit{commetric}; and we see that, in such case, the values of \( w \) are distributed in groups each involving \( n \) equal functions. The \( n \) identical expressions \( \rho(1), \rho(2), \&c. \) thus become representatives of \( n \) changes of the same function \( w \), and consequently we know that the number of values of a \textit{hyposymmetric} function

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* The First and Second Parts of that Analysis appeared in the 32nd and 37th volumes respectively of the third series of this Journal. In Part I. (p. 367) the reference to “vol. ii.” (of the Camb. Math. Journ.) should be changed to vol. i. In Part II. art. (19.) (p. 503) reference is made to page 34. of vol. I. of the Mechanics’ Magazine. The “First Solution” of the page last cited should be corrected by means of the investigations in paragraph XXXI. of the last of my series of papers on the Method of Vanishing Groups (see Camb. and Dub. Math. Journ. for February 1853, pp. 55, 56), and, for the purposes of that solution, though not of the \textit{‘Second’,} and \textit{‘Third,’} the given quadratics must be considered as of the \textit{nineth} order. Each Part of the Analysis was given in the form of a letter to the late eminent geometer Thomas Stephens Davies. To the first, Professor Davies appended notes of his own, which enhance such value as it may happen to possess.

My \textit{‘Notes on the Theory of Algebraic Equations,’} adverted to in the Analysis, are comprised in three series, of which the first appeared in the 46th volume of the Mechanics’ Magazine. The second is contained in the 48th and 49th volumes of the same journal, and the third and concluding series will be found in its 52nd, 53rd and 55th volumes.

My \textit{Horae Algebrae} are printed in the 47th, 48th, 49th, and 50th volumes of the Mechanics’ Magazine.
of $n$ quantities cannot exceed

$$(n-1) \times (n-2) \ldots 3 \times 2 \times 1.$$  

XIX. Further, in examining these latter values we may confine our attention to any one of the functions $\rho$, $\rho(r)$ for instance, and consider $y_r$ as fixed. The determination of the number of values of an $n$-ary hyposymmetric function is, in fact, reduced to the corresponding determination of an $(n-1)$-ary function.

XX. The functions $U_4$ and $u$ are hyposymmetric, and

$$\sigma(u) = \sum y_1^2 y_2 y_3 + 2\sum y_1 y_2 y_3 y_4.$$  

And, if we make

$$y_r - y_5 = z_r,$$

we find

$$u_5 = z_1^2 z_2 z_4 + z_2^2 z_1 z_3 + z_3^2 z_2 z_4 + z_4^2 z_1 z_3,$$

and (see VIII. and XVII.)

$$f(u) = f(u_5).$$

XXI. If all the values of $w$ are known, $w'$ may be made to vanish by means of an equation of the $m$th degree in $y'$ (XVI.). Were we to call the function of $z$ the reducee of that of $y$, it would be well to inquire whether any relations of value or (hypo- or un-) symmetry exist among successive reducees. The relations between epimetry and hyposymmetry would be another topic of inquiry. The hyposymmetry of the pure epimetric $u$ would no longer subsist if in $a_r(y)$ we replaced $y_r^2$ by $y_r^s$, $s$ being greater than 2. Were $s$ equal to 3, $\sigma(u)$ would consist of a symmetric and a hyposymmetric portion.

Temple, November 3, 1853.

LXX. Intelligence and Miscellaneous Articles.

REMARKS ON LUNAR CIRCLES. BY A. QUETELET.

During the evening of the 19th of January 1853, towards half-past eight, the moon being nearly 60° above the horizon, M. Bouvy, Assistant at the Observatory, saw at the north of the moon two circular arcs of a white colour, the one on this side and the other beyond the zenith. The former was concentric with the moon and about 22° in radius; the interior part was of a red colour. The second, parallel to the horizon of a very bright silvery white colour, and not iridescent; it would have been passed by the moon if it had been complete.

Although this phenomenon belongs to the class of halos, and these circles are described in special treatises, the former under the name of the concentric circle, and the second under that of the paraseleinenic circle, they nevertheless presented an appearance sufficiently singular to be remarked.

The moon was at the moment at a considerable elevation above the horizon; the diameter of the paraseleinenic semicircle was not
greater than that which surrounded the moon, and the two arcs of the circles almost joining at their extremities, formed towards the zenith an immense crescent, the effect of which was rendered still more remarkable at the moment by the favourable aspect of the clouds; within the space comprised between the two semicircles the sky presented only faint striated vapours, while at the exterior parts it was covered for the most part by small woolly clouds.

Professor Kämmtz, in his 'Meteorology,' remarks that "as the halos appear most frequently when the barometer falls, they are likely to be followed by rain;" and in the present instance, on the morning of the 20th of January, the barometer fell more than six millimetres, and a heavy rain fell which continued during the day.

ON THE MOTION OF ELASTIC FLUIDS, AND THE THEORY OF WIND INSTRUMENTS. BY A. MASSON.

The author's memoir treats of the flow of elastic fluids through circular orifices pierced in metallic plates; the acoustic phenomena produced by the flow of air through circular orifices adapted to cylindrical pipes; numerous experiments on pipes of different material and length; an examination of the various theories proposed as explanatory of the motion of air in acoustic pipes, together with some remarks on the function of the apertures of organ pipes.

Flow of air through circular orifices.—A rectangular deal box was suitably placed upon the reservoir of an organ bellows, and on its upper surface were adjusted metal discs pierced at their centres with a circular orifice. A manometer—consisting of a narrow inclined tube communicating at one part with the conduit pipe and at the other with a large flask filled with water—indicated the aërial pressure during the experiment. This differential manometer, capable of being inclined in any direction, gives the true pressure by multiplying the variations of the column of water by the sine of the respective inclinations. Several applications of this new apparatus are pointed out in the memoir; its great sensibility was a main condition of the success of the experiments, which required the application of very minute degrees of pressure, frequently less than that of one millimetre of water.

The air in flowing through the circular orifices in the metallic plate produces a sound which rises to a higher pitch in a continuous manner with the increase of pressure, as in the syren.

The sounds which can be obtained from the same orifice are comprised within two limited pressures, which depend upon its diameter and the thickness of the plate.

Elastic fluids in passing through narrow apertures acquire a vibratory condition; the number of vibrations which they perform in any case is proportional to the square root of the pressure or to the velocity of their flow, and is not dependent upon the diameter of the orifices.

Flow of air through orifices surmounted by acoustic pipes.—The periodical motion of the air which flows through the orifices does not always exercise upon the organ of hearing an action sufficiently energetic to give rise to the sensation of sound.
The stoppage of the vibrations by the mass of exterior air, the form, still unknown, of the gaseous vein, the too feeble elasticity of the fluid, may contribute together or separately to the prevention of any acoustic impression.

In order to strengthen the sounds originally produced at the orifice from which the flow takes place, for the purpose of submitting to a further investigation the laws of the vibrations of columns of air, a cylindrical pipe of wood was fixed upon the metallic plate, so that its axis passed through the centre of the acoustic aperture.

When air was forced through by the bellows a sound was produced, and the vibrations were communicated to the pipe, which gave out a series of harmonics comparable in point of purity and intensity with the finest sounds of the organ.

The results of these experiments upon the motion of air in tubes may be expressed as follows:—

1. Air in flowing from an orifice, acquires a vibratory condition capable of producing sound in gaseous columns.

2. The acoustic phenomena are not altered by placing the orifice on the upper or under part of the pipe through which the air flows, or by forcing or drawing the air through it.

3. The sounds which any one pipe is capable of giving depend only on the pressure of the air and not on the diameter of the orifices. The number of vibrations appear to be, for a constant aërial pressure, proportional to the thickness of the plates.

4. The different harmonics of a pipe vibrated by air issuing through a circular orifice may be thus classified:—
   a. Several sounds deeper than the fundamental sound of the pipe.
   b. Sounds of the open pipe, agreeing with theory.
   c. Sounds of the closed pipe, agreeing with theory.
   d. Indeterminate sounds.
   e. Harmonic sounds of the theoretic wave.

5. The space comprised between two ventral segments or two nodes of vibration is always conformable to theory, with the exception of a portion of the pipe near the plate.

Terminated by two ventral segments, or by one ventral segment and one node, this portion is generally much smaller than an actual wave.

6. The acoustic wave, situated at the extremity of the pipe, which is the seat of the initial vibratory motion, and the real or theoretic wave at a distance from this, always vibrate in unison, and their lengths bear a simple and harmonic ratio to each other.

7. For the same orifice in the same pipe a sound may be produced by very different pressures, but they still preserve an harmonic proportion to each other.

8. For any one sound the pressure varies within certain limits, without the tone of the pipe manifesting the slightest alteration; it is only the intensity of the sounds which increases or decreases with the pressure.

9. One pipe may give several sounds simultaneously.

10. To a given mouth-piece, a pipe possessing the property of
rendering a sound distinct always corresponds, notwithstanding the variations of atmospheric pressure*.

Every possible means have been adopted for verifying the consequences of the principal facts which have been announced above, and the greatest care and attention must be paid to the determination of the ventral segments of vibration. It was by piercing or cutting the pipe that the author has succeeded in determining the points at which the air retains its natural state throughout the entire time of vibration. He has thus ascertained that the two open extremities of the pipe are always ventral segments, and that the part near the mouthpiece may be comprised between two or between one node and one ventral segment. It is possible without altering the sound to remove the whole of that portion of the tube situated above the extreme wave.

In the series of sounds represented by the formula which characterizes closed pipes, the half-wave near the acoustic orifice is always comprised within two ventral segments; this circumstance sufficiently distinguishes this series from those of Bernoulli.

In addition to the remarkable fact, that a column of air comprised between two ventral segments without interposition of a node gives a deeper sound than the fundamental sound determined by its length, the author states that the exceptional wave may sound under two very different pressures; the greatest is necessary to produce the sound when the pipe is restored to its original length.

In all the experiments the volume of the reservoir of air or the conduit pipe was changed several times without any recognizable alteration in the phenomena being produced.

All pipes, whatever their dimensions of material, conform to the same laws. Pipes of wood, gutta percha, glass and metal were employed; the ratio of the length and diameter has varied from 4 to 40, and no exceptions to the principles laid down by the author were met with.

By placing the acoustic orifice between two tubes of the same diameter and length which bear a simple proportion to each other, it was found that the two columns vibrated in unison when by their division they were capable of giving rise to subdivisions of the same length, the exceptional parts situated at each side of the plate being equal to each other or to an octave. In every other case the note of one pipe alone is heard.—Comptes Rendus, Feb. 1853.

**OBSERVATIONS ON SOME EFFECTS PRODUCED BY ELECTRIC CURRENTS.** BY A. MASSON.

In producing the decomposition of water with the ingenious apparatus of M. Ruhmkorff, Mr. Grove† found the extremities of the Wollaston conductors perfectly luminous; like his predecessor, he obtained detonating mixtures at both poles, sometimes with an

* The practical value of these principles has already been shown in a work, published by M. Louyet and the author, upon the theory of wind instruments and of the voice (Traité de Physiologie, par F. A. Louyet, tome i. fasicule 3).
† Phil. Mag. March 1853.
excess of hydrogen, sometimes of oxygen. The English author indicates the laws of these decompositions, which appear to M. Masson to be explained by some phenomena which he regards as incontestable, although some physicists hesitate about admitting them. He thinks that—

1. Platinum, and probably all incandescent bodies, have the power of decomposing liquids as they decompose gases*.

2. Gases may be polarized as well as solids and fluids†.

3. Gases as well as solids and fluids conduct electricity, and behave like the latter in all electrical actions.

4. Conductors of equal section placed in the same circuit become more heated in proportion as they are bad conductors.

5. When a negative current passes from a good conductor to a bad one, the point of junction becomes much more heated than if the current passed from the bad conductor to the good.

6. The electric light or spark is caused by the incandescence of the medium through which it passes.

The author shows that two modes of electrical decomposition must be admitted; one which he calls polar, in which the elements of the body submitted to electrolyzation are transported to the two poles, and a second which he terms calorific decomposition. This acts principally in the decomposition of gases and liquids; it is very active at the incandescent poles of an electric current, or at each point of a spark.

The principal object of his experiments has been to isolate these two decompositions.

He first decomposed water with two Wollaston's conductors, and found at each pole a detonating mixture, which was more abundant at the negative, or more incandescent pole, than at the positive pole. There was always an excess of hydrogen at the negative, and of oxygen at the positive pole.

The oxyhydrogen mixture is produced by the calorific decomposition; the excess of the gases is attributed to polar decomposition. This is proved by the following experiments.

By taking as poles one pointed conductor, whilst the other is terminated by a ball of platinum of 1 to 2 millimetres in diameter, the luminous point becomes the centre of an abundant decomposition. Scarcely any gas is evolved on the ball. Mr. Grove has observed the same fact in employing a plate. The gas disengaged by the point is a mixture of oxygen and hydrogen, with an excess of one or the other, according as it is positive or negative; the ball gives pure oxygen or hydrogen gas according to its polar position.

If the point be kept out of the liquid and in the air so as to obtain a series of sparks, the ball plunged in the liquid, which, in these experiments, was water acidulated with phosphoric acid, scarcely produces any decomposition, and furnishes oxygen or hydrogen according as it is positive or negative. In this case the decomposition is polar and very feeble. In these experiments the decomposition of the glass forming the extremity of the conducting tube placed in the air was ascertained.

* Grove, Bakerian Lecture. † Faraday, Researches on Electricity.
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Alcohol presents the same phenomena as water. The calorific decomposition is much more active in alcohol than in water. When a point and a ball are immersed in the liquid, no decomposition appears to take place on the ball if it be positive, the oxygen probably combining with the alcohol; the decomposition is very weak if it be positive, and the gas obtained appeared to consist of bicarbonate hydrogen. The product of the calorific action appeared to be a mixture of hydrogen and carbonic oxide, which would be very readily explained by the composition of hydrated alcohol.

The author had observed in his eudiometric analyses that the two wires of the inductive apparatus became unequally heated,—the negative wire became red-hot and fused, whilst the other remained cold. He then tried the decomposition of some gases placed in a tube furnished with two wires, the extremities of which were very close together.

Sulphuretted hydrogen gave sulphur and hydrogen; the negative wire was incandescent and furnished the sulphur, the positive wire remained dull. Sulphurous acid underwent no decomposition. The negative wire fused, whilst the positive did not even redden.—Comptes Rendus, June 27, 1853, p. 113.

ON THE FORMATION OF CRYSTALLINE MINERALS.
BY A. DREVERMANN.

The author has instituted a series of experiments upon the artificial production of those crystalline minerals which are formed in the humid way, starting from the opinion that the crystals occurring in geodes could be produced neither by gradual evaporation nor by the cooling of saturated solutions. By the method employed, substances the most difficult of solution, as well as those which are readily soluble, may be crystallized in a simple and easy manner; it also at the same time admits of an infinite variety of modifications of the forces acting. The principle of the method consists in gradually changing the affinity of the solvent for the substance dissolved in such a manner that the latter may be gradually separated. This change is effected by the diffusion of one liquid into another, which liquids must be so chosen, that, when they mingle, a solid substance will separate.

The arrangement of the apparatus is exactly the same as in Graham’s experiments. Some powdered chromate of potash and nitrate of lead was introduced into two tolerably long gas cylinders, filled carefully with water, and then placed close together in a large beaker, which was also so far filled with water that the two cylinders were covered. In consequence of the upward diffusion, the nitrate of lead reached the large beaker in the course of a few months, when several very beautifully coloured amorphous compounds were formed on the margin of the cylinder filled with chromate of potash. Within the same cylinder beautiful orange-red brilliant acicular crystals of chromate of lead (PbO, CrO₃) were attached, together with small, dark-red, rhombic tables of melanochroit (3PbO, 2CrO₃): these constantly grow larger. The needles of chromate of lead formed in this manner attained a size of 3 or 4
millimetres, when they were detached by the constant shaking of the ground which always occurs in a large town, and falling on the bottom of the cylinder, got beyond the reach of the conditions necessary for their formation; were it not for this circumstance, they would certainly have attained a length of half an inch or more in the course of three or four months. In the same vessel crystals of carbonate of lead (PbO, CO₃) were also formed, evidently in consequence of the contamination of the chromate of potash by carbonate.

In a similar manner crystals of calc-spar, rhombic tables of 2CaO, HO, PO₄⁺₄HO, and other acicular crystals of a fatty lustre, which the author regards as 3CaO, PO₄, were obtained. As the principle of this method may be applied almost universally, and as amorphous carbonate of lead is soluble in 50,000 parts of water, and chromate of lead is still less soluble (sulphate of baryta is soluble in 43,000 parts of water), we may conclude that the fact of a compound being difficult of solution is no hindrance to its artificial preparation in a crystalline form. In the course of these experiments it was remarked, that the slowness with which these crystals were formed (they were generally only produced in several months) depended upon the fluids only coming together in that time; the experiment was therefore varied by introducing a glass vessel filled with a dry salt into another containing a saline solution, so that the former was only slightly covered. A considerable precipitate was produced, which was deposited upon the undissolved salt. Several days afterwards small crystals were observed upon the amorphous precipitate, which, as the conditions of their formation continued, must constantly increase in size. In this manner the author hopes to obtain large crystals of heavy spar, calc-spar, sulphate of lead (PbO, SO₄), pyromorphite [3(3PbO, PO₄⁺₁ Pb Cl)], apatite, &c. By the diffusion of solutions of silica and alumina in potash the author hopes to produce felspar.

The crystallization of soluble bodies is equally simple. Thus, if a solution of sulphate of iron be put into a beaker, carefully covered with a thin stratum of water, and the beaker filled up with alcohol, crystallization commences in the course of a few hours and proceeds rapidly. Crystals may probably be prepared in the same manner from acid and alkaline, alcoholic, etherial and other fluids; and the separation of two bodies by alteration of the solvent which is so often employed in organic chemistry, may thus be combined with a separation by crystallization.

The identification of the crystals obtained with the minerals does not repose upon chemical analysis. However, the number of compounds which might possibly be produced is very limited, and the crystals correspond most exactly with the minerals in their physical and qualitative chemical properties, as well as in their grouping and geometrical form.—Ann. der Chem. und Pharm. vol. lxxxvii. p. 120.

ON THE PASSIVE STATE OF NICKEL AND COBALT.

BY J. NICKLÉS.

The curious property possessed by iron of becoming less oxidizable after being placed in contact with fuming nitric acid, has exercised the sagacity of many chemists and physicists. This passive state is
induced, not only by the contact of fuming nitric acid, but also when the iron is blued by the spirit-lamp, or when it is touched with a piece of platinum whilst immersed in non-fuming nitric acid. The same effect is obtained when the iron is attached to the positive pole of a voltaic battery.

In consequence of the modifications which it undergoes under these circumstances, iron does not precipitate sulphate of copper when it is employed as the anode of a voltaic element; oxygen is evolved around it without attacking it; it bears contact with aqueous nitric acid without alteration, but becomes active again when it is plunged into water after being taken out of the acid.

The same thing takes place more or less with nickel and cobalt when drawn into wire. In contact with fuming nitric acid these two metals only acquired a passive state of very short duration, but their passivity becomes permanent, when, after bluing over the flame of a spirit-lamp or a charcoal fire, they are plunged whilst hot into this acid; they then behave exactly like passive iron, except that they are less negative than that metal in nitric acid. They nevertheless communicate their passive state to active iron immersed in non-fuming nitric acid, and arrest the violent attack of the acid on the metal.

Platinum is always negative in regard to these three metals when rendered passive, and each of the latter is also negative in regard to the three active metals. The neutrality of passive iron in sulphate of copper, pointed out by Schönbein, could not be reproduced out of the circuit; in all the experiments the passive iron was soon surrounded with metallic copper.

The examination of the electro-chemical relations of iron, nickel and cobalt, in their active and passive states in contact with different acids and with a watery solution of potash, gave the following results. The negative character of passive iron is really distinct only with nitric acid; in the other liquids employed in these experiments, the positive electricity is transferred to the iron instead of emanating from it. In solution of potash the relations were the same with the active as with the passive metals, which would seem to indicate that the passivity of these metals is destroyed by this alkali. This, however, is not the case; for if after immersion in potash the metals are again brought into contact with nitric acid, of specific gravity 1.34, they again acquire their passive state, and are not attacked by the acid.

The following table gives the relations of iron, nickel, and cobalt in both states, commencing with the positive and concluding with the negative metal, the experiments being made with the liquids indicated:

<table>
<thead>
<tr>
<th>Liquid employed</th>
<th>Active metals</th>
<th>Passive metals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fuming nitric acid</td>
<td>+ ..........................</td>
<td>+Co, Ni, Fe</td>
</tr>
<tr>
<td>Nitric acid of specific gravity 1.34</td>
<td>Fe, Co, Ni</td>
<td>Co, Ni, Fe</td>
</tr>
<tr>
<td>SO₄⁺HO</td>
<td>Co, Fe, Ni</td>
<td>Ni, Co</td>
</tr>
<tr>
<td>SO₄⁺HO, diluted with nine parts water</td>
<td>Fe, Ni, Co</td>
<td>Fe, Co, Ni</td>
</tr>
<tr>
<td>Solution of potash</td>
<td>Fe, Ni, Co</td>
<td>Fe, Ni, Co</td>
</tr>
</tbody>
</table>

—Comptes Rendus, August 16, 1853, p. 284.
RESEARCHES ON THE ELECTRICAL CONDUCTIBILITY OF GASES AT HIGH TEMPERATURES. BY M. EDOMOND BECQUEREL.

The apparatus employed in these researches consists of a long platinum tube without a joint, surrounded by an earthen tube which traverses a furnace horizontally. This tube, which is to receive the gases submitted to experiment, is of sufficient thickness to allow of a vacuum being produced in it without disfigurement, even when a portion of it is at a red heat.

Two platinum wires, completely insulated, are stretched lengthwise through the tube parallel to one another, in such a manner that if each of them communicates with one pole of the battery, the electrical current can only circulate when the gaseous medium separating the wires becomes a conductor. These two wires are sometimes replaced by a rod of platinum or iron, also insulated and parallel to the tube, when the rod and the tube are employed as electrodes instead of the two platinum wires.

In the course of the circuit are interposed a galvanometer of extreme sensibility, and a rheostat of peculiar construction. This rheostat consists of a column of fluid of very small diameter contained in a divided capillary tube like that of a thermometer, and so arranged that its length can be varied at pleasure whilst kept at the same temperature. The circuit now containing the battery, the gas which has acquired conductive power, the rheostat and the galvanometer, it is easy, by always bringing the deviation of the needle to the same degree, to compare the conductive power of the gas with that of the fluid in the rheostat.

By these means of investigation the author has measured exactly the resistance of the gases in different conditions, as that of solids and liquids has been measured. The gas being contained in a platinum tube, he was able to change its elastic force at pleasure at the same time that he raised its temperature. He has arrived at the following results:

Gases do not become conductors of electricity to an appreciable extent until towards a red heat; from this point they conduct better in proportion to the elevation of their temperature. They then afford a passage for the weakest currents of electricity that can be produced by means of a small pair of plates.

The quantity of electricity which passes is augmented as the size of the negative electrode is increased. The same effect is observed in the transmission of electricity through liquids.

The resistance of a gas varies according to the number of pairs of which the battery consists, and the intensity of the electric current traversing it.

Below red heat there is no appreciable difference between the conductibility of a rarefied gas and one at the ordinary pressure; they neither of them conduct electricity. Above this limit, differences show themselves, and the dilated gas always proves the best conductor.

When the elasticity of a gas brought to a red heat is diminished, its conductibility increases; when brought to the highest degree of rarefaction possible with pneumatic apparatus, the gaseous
medium attains its maximum conductive power, although it still offers a readily appreciable resistance.

The diminution of the elasticity of the different gases gradually diminishes the difference between their conductive powers until, when we arrive at a pressure of only 3 or 4 millimetres of mercury, all the gases form equally good conductors.

It is remarkable to see heat acting in the same manner as diminution of pressure in rendering the differences between the resistances of the gases gradually less. But if heat diminishes the quantity of matter lying in the way of the electric current in a given space, so as to facilitate the passage of the electricity, it has also a very distinct action of its own; since at ordinary temperatures even the most perfect vacuum that can be produced by air-pumps does not possess conductive power; whilst at a red heat the electricity can pass through, even a condensed gas, where the mass submitted to experiment is greater.

It is easy to understand how complicated the comparison of the resistances of gases must be, since their conductive power varies with their temperature, their pressure, and the intensity of the current. The results are also influenced by the nature of the metals employed in passing the electricity into the gas; and it is necessary to ascertain the resistance offered to its passage from the electrodes into the gas in order to arrive at the actual resistance presented by the latter.

The author gives the following table of the conductive powers of the gases employed in his experiments, at a red heat and at the pressure of the atmosphere, with platinum electrodes. The resistance of the air is regarded as unity, and the gases are arranged in the order of their conducting power, commencing with the best conductors.

Hydrogen (resistance from 0.3 to 0.4 of that of the air, according to the conditions of the experiments).
Light carburetted hydrogen.
Oxygen (resistance between 0.4 and 0.7).
Chlorine (resistance not more than 0.92).
Air.
Protoxide of nitrogen \{ differ but little from air.
Nitrogen
Carbonic acid (resistance from 1.2 to 2.0).

Further details are given by the author in his memoir.—*Comptes Rendus*, July 4, 1853, p. 20.

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**ON THE CALCULUS OF OPERATIONS.**

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I beg leave to correct the two following errors which occur in my recent paper on the Calculus of Operations.

In page 352, the expansion of \((\nabla + c)^{-1} \phi D\) should be

\[ \text{Phil. Mag. S. 4, Vol. 6, No. 41, Dec, 1853.} \]
Meteorological Observations.

\[ \frac{1}{c} \phi D - \frac{1}{c^2} \phi' D + \frac{1}{c^3} \phi'' D - \frac{1}{c^4} \phi''' D + \ldots \]

in lieu of

\[ \frac{1}{c} \phi D - \frac{1}{c^2} \phi' D + \frac{1}{c^3} \phi'' D - \frac{1}{c^4} \phi''' D + \ldots \]

which involves a similar correction for the formula at the bottom of page 355.

I observe, also, that the mode of obtaining the coefficients of

\[(a + bx + cx^2 + \ldots)^{-n}\]

given at the beginning of page 362 is erroneous, and in lieu of it the usual well-known methods should be applied.

This does not require any change in the results of the paper.

I am, Gentlemen,

Your obedient Servant,

C. J. Hargreave.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1853.


Mean temperature of the month ........................................ 49° 99
Mean temperature of October 1852 ...................................... 46° 22
Mean temperature of Oct. for the last twenty-seven years ............ 50° 06
Average amount of rain in Oct. ....................................... 2'64 inches.


Mean temperature of Oct. for twenty-six previous years .............. 47° 64
Mean temperature of this month ........................................ 48° 66
Mean temperature of Oct. 1852 ......................................... 46° 88
Average quantity of rain in Oct. for thirteen previous years ...... 4'99 inches.
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| Mean.        | 29'711   | 29'596         | 29'29           | 29'548   | 29.566         |                 | 58'77          | 41'22  | 50'5      | 49'29    | 48'04 |                 |

|             | 3'78     | 2'70           | 5'94           |                 |                 |                 |                 |        |            |           |       |                 |
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END OF THE SIXTH VOLUME.
In Fig. 1 and 2, the Inferior Tourmaline & Selenite Stage are employed. The arrows show the plane of the Tourmaline.