Playaux near Vevey, and on the southern at Voirons, east of Geneva. These two mountains are both situated on the borders of the Alpine chain. In order properly to grasp the relation which subsists between the overthrow of the strata along this line and the great depths of the lake, it is necessary to mark on the map the soundings of the lake by De la Beche, the positions of the crests of the Voirons, the Allinges south of Thonon, and of Playaux near Vevey *.

The principal soundings of the lake placed opposite Meillerie and Evian may likewise (and perhaps it is the most easily effected) be marked on the geological map of Savoy. Then Voirons and Playaux should be joined by a line (but not by a straight line, because the chains on the borders of the lake are curved) drawn through Calvaire (Voirons), Allinges, the point where the Alpine Macigno (M) (Affleure) descends to a level with the bed of the Dranse, at a distance of four kilometres from Thonon and the city of Evian. Such a line as this would terminate towards Playaux, passing over the northern bank of the lake between Corsier and St. Saphorin.

This course shows pretty nearly the line of the reversal of the strata situated on the flanks of the Alps; and, presenting a certain parallelism to the denudations of the different rocks traced on my geological map of Savoy, it passes through the midst of the soundings which indicate the greatest depth of the lake. Consequently this depth bears a relation to the reversal of the beds. *It is in such fractures, I am confident, that the true cause of the origin of these lake-basins is to be found.*

From a summary of these facts it may be concluded—

1st. That the Lake of Geneva deviates much from the median (central) line of the great glacier or glaciers which extended from the Rhone to the Jura.

2ndly. That these ancient glaciers not having had the power to remove the older drift below Geneva, have not been able to produce in the lake-basins what is called their excavation (*l'affouillement*). If they could not scoop out these basins, still less have they excavated the adjacent valleys which terminate in them.

3rdly. The valleys and the basins of mountain-regions are related to the cause which has given to the mountains their orographical characters, and to the strata their greater or less inclination.

* This mountain, called Pleyaux or Playaux, is indicated on the Federal map by the name of Pléiades. To facilitate the indication of it on my geological map of Savoy, on which it is not marked, I should say that it lies 6 kilometres from the mouth of the stream which discharges itself into the lake between Vevey and Corsier, and $6\frac{1}{2}$ kilometres from the point of Montreux.

4thly. We have seen, in fact, that, so far as the Alpine lakes generally are concerned, and as regards that of Geneva in particular, their position has been determined along a line of overthrow, or reversal of the strata. We have seen that the form of the Lake of Geneva was caused, in the eastern part, by the curvature of the mountains on its southern banks, and in its western part by its parallelism with the great anticlinal axis which traverses Switzerland.

Finally, we have remarked that the greatest depth of the Lake of Geneva lies along the line of reversed strata which occurs at the junction of the Alps with the plain. Consequently the sort of basin to which this lake belongs is not the result of a cause acting on the surface of the globe, but is what may be termed a volcanic effect (that word being used in the sense assigned to it by Humboldt), viz. the influence exerted by the interior forces of a planet on its external crust in the different stages of its cooling. Accept, &c. &c.,

ALPHONSE FAVRE.

P.S.—Since the dispatch of my previous letter, I have read with extreme interest your Address to the Geographical Society of London, of the 23rd May, 1864, with which you have been so good as to favour me. I find in that address a clear and precise summary of the state of the question, and valuable evidences derived from many parts of the world. I perceive in it, again, with pleasure that we are of the same opinion respecting the excavation of lakes and the erosion of valleys by glaciers. You make use of several highly important arguments against that view of the question, and you have already developed the idea on which I have dwelt—viz., that the form of the Lake of Geneva is divergent from the direction of the most powerful or central portion of the glacier of the Rhone, which advanced from the Valais in the direction of Yverdun, following what I have termed the median line.

Pray, Sir, oblige me by inserting this remark at the end of my letter of the 12th of January.—A. F.

Geneva, January 22, 1865.

XXXI. *On the History of Conservation of Energy, and of its application to Physics.* By Professor BOHN*.

IT is an old experience, that great and fruitful ideas make their entrance into the world neither suddenly nor in a state of complete perfection; they generally require a certain time of de-

* Communicated by the Author.

velopment and growth, during which they may be said to belong to different persons. He who first expresses an idea with perfect clearness and exactness is commonly regarded as the discoverer, although in fact there be more than one entitled to this name. Now this rule is also applicable to the question concerning the author of the idea of transmutation of work of one kind into work of another kind. This question, however, is complicated by a circumstance which creates a difficulty in all researches on the history of force, *vis viva,* of conservation of energy, and others: these terms, as well as those of "work," "momentum of force," "momentum of activity," "dynamical effect," "mechanical power," "quantity of action," "quantity of movement," and such like, are used by different authors, frequently even by one and the same writer, in a different sense, and in so undefined a manner that a sort of translation into the more precise scientific language of the present day is required in order to clearly show the meaning of the authors.

In the Philosophical Magazine, S. 4. vol. xxviii. pp. 473, 474, Dr. C. K. Akin quotes a few sentences from Placidus Heinrich and Dr. Mohr, which he considers as the earliest statements of the "allotropy of force." It appears to me, however, that it is essentially left to the individual judgment of the reader whether he will or will not find in those sentences a certain proof of the author's firm conviction of possible transmutation of common mechanical work into heat or electricity, and *vice versâ.*

In my opinion the following remark of L. N. M. Carnot is more striking and less ambiguous than the above-mentioned quotations of Dr. Akin.

"*Vis viva* can figure either as the product of a mass and the square of its velocity, or as the product of a moving power and a length or a height. In the first case it is a *vis viva* properly called, in the second it is a latent *vis viva.*"

This seems to show that, according to Carnot's meaning, the *vis viva* consumed in raising a weight or performing other work is latent or stored up, and may be again employed in reproduction of a motion, or in the performance of another work at the cost of the first done work. This appears more evidently from another passage from Carnot:—

"All effects of propelling powers (*forces mouvantes*) may be compared to the raising of a weight to a certain height, and consequently to a *vis viva,* be it a real or a latent one."

For these statements I refer to Carnot's *Principes de l'équilibre et du mouvement;* they are surely to be found in the edition of 1803, perhaps already in the first (of 1783), whereon, not having the books at hand, I am not able at present to decide. For the same reason I was unable to quote literally; but I trust

that I have succeeded in giving an exact statement of Carnot's meaning. It must be left to the reader to decide on the importance of these sentences of Carnot.

As to the statement of Professor Faraday quoted by Dr. Akin, I can only declare my perfect concurrence with the latter.

In researches on the priority of physical truths, the preference is most justly allotted to those publications which for the first time treat a question by measure and numbers. The first quantitative study of the reciprocal transmutation of different kinds of work originated, as far as I know, from Baron Liebig. In the fourth* of his "Chemical Letters" (*Beilage zur Allgemeine Zeitung* vom 30 September 1841) he surveys the expectations which were at the time in question set on electromagnetism as a mechanical power for propelling ships, &c. In order to show the amount of clearness then pervading the ideas of the celebrated chemist on the subject in question, it would be necessary to transcribe some pages of the letter quoted. I content myself, however, with reproducing a few words:—

"Wärme, Electricität und Magnetismus sind in einer ähnlichen Beziehung einander aequivalent wie Kohle, Zink, und Sauerstoff. Durch ein gewisses Maass von Electricität bringen wir ein entsprechendes Verhältniss von Wärme oder von magnetischer Kraft hervor, die sich gegenseitig aequivalent sind. Diese Electricität kaufe ich mit chemischer Affinität, die, in der einen Form verbraucht Wärme, in der andern Electricität oder Magnetismus zum Vorschein bringt. Mit einer gewissen Summe von Affinität bringen wir ein Aequivalent Electricität hervor, gerade so, wie wir umgekehrt durch ein gewisses Maass von Electricität Aequivalente von chemischen Verbindungen zur Zerlegung bringen. Die Ausgabe für die magnetische Kraft ist also hier die Ausgabe für die chemische Affinität," &c.

Another passage is the following:—

"Aus nichts kann keine Kraft entstehen; in dem berührten Fall wissen wir, dass sie durch Auflösung (durch Oxydation) des Zinks hervorgerufen wird; allein abstrahiren wir von dem Namen, den diese Kraft hier trägt, so wissen wir, dass ihre Wirkung in einer andern Weise hervorgebracht werden kann," &c.

It is worth while to compare the first words of the latter quotation with Dr. Mayer's sentence, "ex nihilo nil fit—nil fit ad nihilum," such being the basis of his speculations in 1845.

With regard to Dr. Akin's remarks upon Huyghens (at p. 472 of No. 191 of the Philosophical Magazine), I finally beg leave to

* The twelfth in the edition of 1851 of the *Chemische Briefe,* p. 202 *et seq.*

refer to Lagrange, *Mécanique Analytique*, 3e édit. par M. Bertrand, vol. i. pp. 215, 217, or to Montucla, *Histoire des Mathématiques*, vol. iii. p. 618, and more particularly vol. iii. p. 622.

Giessen, January 28, 1865.

XXXII. *On the History of Calorescence.* *By* John Tyndall, *F.R.S., &c.**

On the 26th of May, 1859, I presented to the Royal Society a "Note on the Transmission of Radiant Heat through Gaseous Bodies." The question had occupied me some time; but as the experimental difficulties were very great, I published the Note referred to in order to enable myself to vanquish those difficulties at my leisure. All the time at my disposal in 1859 and 1860 was devoted to the subject, and towards the end of 1860 I was so far advanced as to be able to prepare a memoir, which was presented to the Royal Society on the 10th of January, and, being chosen as the Bakerian Lecture for that year, was read on the 7th of February, 1861.

In that memoir the comparative deportment of elementary and compound gases towards radiant heat is for the first time announced. I had attempted to make radiant heat an explorer of molecular condition, and had found that the simple gases possessed a power of transmission immensely greater than that of the compound ones. In the autumn of 1861 I pursued the subject, and by purifying more perfectly the elementary gases, rendered the differences between them and the compound ones still more vast. I then turned my attention to solids and liquids, and confirmed Melloni's experiments on the diathermancy of lampblack; I also tried to render the substance more transparent to invisible heat-rays by ridding it of the hydrocarbons which attach themselves to it during its formation. I next examined the element bromine and found it eminently diathermic; I tried sulphur dissolved in bisulphide of carbon and found it still more so. I finally operated on a solution of iodine in bisulphide of carbon, and found that a layer of it, sufficiently dense to intercept completely the light of the noonday sun, offered a scarcely sensible obstacle to the passage of the invisible calorific rays†

* Communicated by the Author.

† The same *à priori* considerations which led to the discovery of the iodine, point also to red glass coloured by the *element* gold, instead of that coloured by the suboxide of copper, as most suitable for experiments on ray-transmutation. The colouring matter of the former appears to be without sensible action upon the invisible heat-rays; and the rays transmitted through it are competent to raise platinized platinum to a white heat.

This fact is thus announced in a footnote at page 67 of the Philosophical Transactions for 1862:—"A layer of bromine, sufficiently opake to intercept the entire luminous rays of a gas-flame, is highly diathermanous to its obscure rays. An opake solution of iodine in bisulphide of carbon behaves similarly. The details of these experiments shall be published in due time: they were publicly shown in my lectures many months ago.—June 13, 1862."

Turning to my published lectures on Heat as a Mode of Motion," I find at page 357 one of these experiments described in the following words:—"I cannot use iodine in a solid state, but happily it dissolves in bisulphide of carbon. I have the densely coloured liquid in this glass cell. I throw the parallel electric beam upon the screen; this solution of iodine completely cuts the light off; but if I bring my pile into the path of the beam, the violence of the needle's motion shows how copious is the transmission of the obscure rays." Turning back to page 307 of the same work, I find experiments on smoked rock-salt and black glass described as follows:—"Here is a plate of rock-salt coated so thickly with soot that the light, not only of every gas-lamp in this room, but the electric light itself is cut off by it. I interpose the plate of smoked salt in the path of the beam; the light is intercepted, but this rod enables me to find with my pile the place where the focus fell. I place the pile at this focus; you see no beam falling on the pile, but the violent action of the needle instantly reveals, to the mind's eye, a focus of heat at the point from which the light has been withdrawn."

I would ask the reader to picture any experimenter standing by a focus of invisible rays, the exposure to which, for an instant, of the face of my thermo-electric pile caused the heavy needles of a coarse galvanometer to dash against their stops. Could he escape the temptation to put his hand there? I did so fifty times, trying moreover to concentrate the radiation by pushing out my lens. The camera of my electric lamp was furnished with a concave reflector, at the centre of which stood the carbon-points whence issued the electric light. The rays were converged by a lens in front; and when the points were at their proper elevation, the focus of the lens coincided with that of the mirror. Causing the two foci to coincide, and converging the rays as much as possible, I cut off the light by the solution of iodine, and brought in succession my hand, my cheek, pieces of brown paper and of lead-foil into the dark focus. It was simply a substitution of these bodies for the face of my thermo-electric pile. But while the action on the skin was almost intolerable, I obtained neither the charring of the paper nor the fusion of the foil. I knew that a large portion of the radiant heat