the whole of the outside surface, top, bottom, and sides, which is completely engaged by the negative on the two interior surfaces. No free charge is required to hold the engaged lines together; the condition is similar to the concentric spheres of 52.

(41, 42.) The appearance of positive on β depends on a partial discharge having spontaneously taken place during the elapsed time, as in 35.

(43.) The lower cake positive by induction; the upper by partial discharge of a few of the negative lines that pass upwards through the three cakes. It is remarkable that the inductive power is limited in its action to the surface in contact with the surface excited.

(44.) In this, as in 36., the surface in contact with the excited surface appears to share, to a small extent, its excitement, as if conduction had actually followed very slowly behind induction.

(45.) The permanent effect on the cylinder was similar to the temporary effect on a conducting cylinder of the same size, the charged B. D. being supposed close to but not touching it.

(47.) This is a very instructive experiment; there was no actual loss of charge, only an apparent loss, so long as the cake was on the electroscope and the contact surface inductively excited.

(53.) The large radius of the hoop appendage that characterizes Winter's electric machine gives slow divergence to the electric lines that issue from it. See 53. This gives them, as part of the system of electrical lines that includes the lines between the spark balls, great power of lateral compression upon them previous to the spark.

The thunder-cloud as a charged surface is an extreme example of the spark-producing power of slowly converging electric lines.

(58 *a.* and 58 *c.*) The distribution computed from 58 *a.* subjected to the equation in 58 *c.*, ought to stand the test. In an ellipsoid or spheroid it might be practicable to execute the calculation, and thus obtain further confirmation of the law of mutual dependence of the lateral repulsive and root-pulling or contractile force.

In conclusion I may mention that the theory of electric lines here given was deduced from Harris's experiments about twelve years ago; since which time I have been in the habit of applying it to the published results of experimentalists, and thus continually testing it. It is very suggestive of new experiments. Some of the simpler sort of these I have been able to make, but there are others, chiefly with respect to the production of light and mechanical effect, that require greater means and appliances, not to mention aptitude; for to suggest and to execute are specialities that do not always go together. The main end and purpose of these would be to obtain some idea as to the working arrangements between the æther—that higher potential form of matter in which the might of the Infinite resides—and ordinary molecules, the agents of its development. If we confine our attention to the planetary movements, nothing seems clearer than that its density must be inappreciable. On the other hand, were we to make legitimate inferences from the most obvious phenomena of radiant heat, there is evidence that its density may not differ much from that of water, and at least that it is quite impossible that its non-resistance to the celestial motions can be owing to its extreme rarity.

Edinburgh, December 4, 1864.

XXIX. *On the Conservation of Force.* *By* Dr. C. K. Akin*.

Absence, and another controversy of a very different nature, prevented my noticing hitherto Professor Tait's answer to my remarks published in the last December Number of this Magazine. Professor Tait begins by calling attention to the fact that, although omitting the words "in omni instrumentorum usu" from the passage which he quoted from Newton's *scholium*, he indicated the omission by dots. The readers of this Magazine will have seen that in reproducing from Professor Tait's paper the paragraph in question, I took care to cause the dots also to be inserted, to which I made special reference in my remarks. On this point, therefore, there can be no misunderstanding. But when Professor Tait says that "in ordinary mechanics" is the "perfectly complete" free rendering of the above Latin words, I can only partially agree with him. No doubt the rendering is free, "not literal," and in some instances it might also be correct; but I contend that in the present case it is not properly admissible. In the sentences preceding the one cited both by Professor Tait and myself, Newton instances expressly the cases of the "balance," "pulley," "clocks," "screw," and "wedge"; and in my opinion, therefore, the free English translation of "in omni instrumentorum usu," as applicable to the case in hand, is not "in ordinary mechanics," but as given by Motte, "in the use of all sorts of machines," or something like it.

Professor Tait allows that "in Newton's time, and long afterwards, it was supposed that work was *absolutely lost* by friction"—in other words, that Newton himself supposed it to be so; but, considering that it was known that friction excites heat, as well

* Communicated by the Author.

as the other facts I have mentioned, I cannot agree with Professor Tait "that, so far as experimental facts were known in Newton's time, he had the Conservation of Energy complete." In the case of any other man it might appear ungenerous to look too closely into claims to a scientific discovery put forward on his behalf by well-meaning advocates, especially when there is not any better-entitled competitor in the field; but in the case of Newton, whose head is already so thickly covered with laurels, this remark could not apply. I cannot help thinking that the principle of the Conservation of Force, in its widest sense, was discovered by no single person, but was only gradually evolved and developed; and I am mistaken if we are already in full possession of its meaning.

Professor Tait protests against the allowing of any weight to the opinion of John Bernoulli "on a question of this nature," because he "seriously demonstrated the possibility of a perpetual motion." I am not aware that, in point of theory, "perpetual motion" is impossible; although, no doubt, "perpetual work" is. But waiving altogether this point, would it not be possible also on such a principle to impugn the value of any opinion of Newton—for instance, on the subject of light, on the plea that his fundamental notion of the nature of light was wrong?

Like many others, I am anxious for the appearance of Professors Tait and Thomson's long-promised treatise; and in the meanwhile the "Sketch of Elementary Dynamics," published for the use of the students of Glasgow and Edinburgh, might perhaps with advantage be made more accessible to students in general than I have understood it to be.

London, February 1865.

XXX. *On the Origin of the Alpine Lakes and Valleys. A Letter addressed to* Sir Roderick I. Murchison, *K.C.B., by* M. Alphonse Favre, *Professor of Geology in the Academy of Geneva, and Author of the Geological Map of Savoy**.

Sir, Geneva, 12th January, 1865.

I AM glad that you have asked my opinion of the new theory, according to which the Alpine lakes have been excavated or scooped out by glaciers; and of that which also explains the origin of the Alpine valleys by means of the erosion produced by glacial action†.

* Communicated by Sir Roderick I. Murchison, K.C.B., D.C.L., F.R.S., &c.

† A great many arguments against these theories have been advanced in various memoirs, as in those of Mr. Ball (Phil. Mag. 1863, vol. xxv. p. 81), Desor (*Revue Suisse*, 1860), Studer (*Archives des Sc. Phys. et Natur.* 1863, vol. xix.), &c. However unwilling I may be to reproduce the arguments which they have already employed, it is almost impossible not to revert to them occasionally.

I am a strong partisan of the notion of the transport of erratic blocks by ice, at the period of the great extension of the glaciers, and as a Swiss I am attached to this theory, which is worthy of the term national. But, at the same time that I acknowledge it to be accompanied by certain difficulties, I cannot comprehend the two other theories, although they have the advantage of being advocated by able men of science. Amongst these is to be counted Professor Ramsay, a highly distinguished geologist, to whom long practice on the Geological Survey of England has given great powers of observation and a sure eye (*coup d'œil*), Mons. de Mortillet, who is well acquainted with the Alps, and Professor Tyndall, whose works on physics hold the first place. Not that I do not sincerely respect the opinions of the learned authors who have developed these views, and who have done so, I acknowledge, with considerable ability.

It is evident, indeed, that existing glaciers abrade the rocks on which they move, inasmuch as they polish them. But this action is so feeble, that I cannot see how it has been inferred therefrom that it has been able to scoop out deep lake-basins many hundreds of feet below the mean level of the valleys, even on the supposition that it has been exerted during very long periods. I understand still less how this same action could have excavated valleys many thousands of feet deep in a great rock-mass like that of the Alps.

A limit must be set to certain effects. This limit exists in all geological questions, and it is indispensable to establish it.

On seeing a dune on the sea-shore, twenty or thirty metres high, formed by means of grains of sand driven by the wind, shall I be right in concluding that in some hundreds of thousands of years this same dune could attain the height of the Alps or that of the Himalaya?

I have no wish to maintain that the glaciers have not exerted any influence on the forms of lakes and valleys. It seems to me to be impossible that masses so considerable as those which moved in the valleys during the glacial epoch, should not have fashioned, more or less, the *borders* of these depressions. But I cannot become an advocate of the belief that glaciers are the original cause of the formation of lake-basins and valleys. I believe both to be a direct consequence of the formation of mountains, and that they both owe their origin to movements of the earth's crust.

Let us now leave these general arguments, and arrive at more precise facts relative to the origin of the Lake of Geneva. According to all glacial theories, the union of all the glaciers of the Valais at Martigny, to a portion of those of the main body (*massif*) of Mont Blanc, formed one enormous glacier, to which