versing with me at the Royal Institution. I broke the cover in his presence, and, finding that it contained a duplicate proof, handed one directly to him; for I wished him to see what I had there said regarding himself. He read that proof before I did; and though this occurred ten or twelve days prior to the publication of the paper, or about the 18th of October, the first murmur of his dissatisfaction comes, at once to the public and to me, in the December Number of the Philosophical Magazine. It is not, I believe, the rule of courtesy in this country to publish private correspondence without some mutual understanding, much less to garble it. But I trust I do not offend against this rule by stating that twenty-four hours before Dr. Akin's article "On Ray-transmutation" met the public eye, I received a friendly note from that gentleman, acknowledging some trifling civilities which it had been in my power to show him, but not containing the slightest intimation of his attack. During the last days of October, and the early part of November, there had passed between Dr. Akin and myself a somewhat voluminous correspondence, which, when it ceased to be useful, I was obliged to end, with an understanding, however, that it should be renewed as soon as his feelings had calmed down. I rejoiced to think that the friendly communication above referred to was an evidence that the period of calmness had arrived, and I resolved, if such were the case, to give him an opportunity of associating his name with the experiments I had been making on the invisible radiation of the electric light. The vanity of this resolve is now demonstrated. The words "will now realize and 'publish' a discovery," used in the last page of Dr. Akin's article, are also quite characteristic. No one could infer from these words that I had actually, out of consideration for him, waived all right of making my researches known until the 3rd of November 1865, for the express purpose of giving him the chance of prior publication. I may add that when I entered into this voluntary engagement, which, by his own deliberate act, he has now dissolved, I had no notion that Dr. Akin had any doubt of his ability to give his attention to scientific researches.

The following brief summary may, perhaps, spare him the time and trouble of further criticism regarding the "inconclusiveness" of my experiments.

1. By sending the beam from the electric lamp through a sufficiently thick layer of iodine dissolved in bisulphide of carbon, the luminous portion of the radiation may be entirely intercepted, and the non-luminous almost entirely transmitted.

2. The invisible rays, suitably converged, form, at their place of convergence, a clearly defined, but perfectly invisible image of the coal-points whence the rays emanate.

3. A piece of zinc-foil placed at the focus of invisible rays, burns with its characteristic purple flame. Chemists know that there is some difficulty in causing this substance to blaze, even in a flame of high temperature.

4. Placing a thin plate of a refractory metal at this focus, a space of this metal, corresponding to the invisible image, is raised to brilliant incandescence.

5. When, instead of a metal, a sheet of carbon, placed *in vacuo*, is brought into the focus of invisible rays, the incandescent thermograph of the coal-points is also vividly formed. Cutting the sheet of carbon along the boundaries of the thermograph, we obtain a pair of incandescent coal-points, larger and less intensely illuminated than the original ones, but of the same shape. Thus, by means of the invisible rays of one pair of coal-points, we may render a second pair luminous.

6. By a suitable arrangement of the carbon terminals a metal on which their image falls may be raised to *a white heat*.

7. The light of a metal thus rendered white-hot yields, on prismatic analysis, a brilliant spectrum, which is derived wholly from the invisible rays lying beyond the extreme red of the source.

8. When the electric light is looked at, directly, through the solution employed in these experiments, nothing is seen.

9. When, in a dark room, a suitable screen is placed in the focus of invisible rays, nothing is seen.

10. When a solution of sulphate of quinine, or a piece of uranium-glass, is placed in the focus, nothing is seen.

11. When the retina of the human eye is placed at the focus, in which metal plates are raised to incandescence, nothing is seen.

The injury to my eyes, resulting from this experiment, was, I believe, less than that produced by the night-labour which the writing of this article has imposed upon me.

Royal Institution, December 1864.

IX. *Note on the History of Energy.* *By* P. G. Tait, *M.A.**

In the December Number of the Philosophical Magazine, Dr. Akin has called in question the statement that Newton, in a Scholium to his Third Law of Motion, "completely enunciated the Conservation of Energy in ordinary mechanics." He calls attention to the circumstance that the words "*in omni instrumen-*

* Communicated by the Author.

torum usu" which, for brevity, I omitted in the quotation from the *Principia*, appear to him to alter the meaning and application of the passage. Now I consider them to involve precisely that restriction ["in ordinary mechanics"] under which I made the assertion about Newton. In fact the three English words form a perfectly complete, though not literal, translation of the four Latin ones. Any rigid body, subject to such forces as pressures, gravitation, &c., is really a machine—whether it be employed for mechanical purposes or not. I took care to indicate the omission of this qualifying clause, though it had, in fact, been supplied in my general statement.

I regret that the Treatise on Natural Philosophy, on which Prof. W. Thomson and I have been for a long time engaged, is not yet published. The portion bearing on my present subject was printed off considerably more than a year ago. I shall not, however, quote from it, but from a 'Sketch of Elementary Dynamics'* published in October 1863 for the use of students in Glasgow and Edinburgh. In that pamphlet—after quoting Newton's memorable words—we proceed (p. 30),

"In a previous discussion Newton has shown what is to be understood by the velocity of a force or resistance; *i. e.*, that it is the velocity of the point of application of the force *resolved in the direction of the force*, in fact proportional to the virtual velocity. Bearing this in mind, we may read the above statement as follows:—

"*If the action of an agent be measured by its amount and its velocity conjointly; and if, similarly, the Reaction of the resistance be measured by the velocities of its several parts and their several amounts conjointly, whether these arise from friction, cohesion, weight, or acceleration;—Action and Reaction, in all combinations of machines, will be equal and opposite.*"

We then show, in passing, that D'Alembert's principle is distinctly pointed out, and proceed thus (p. 31):

"The foundation of the abstract theory of energy is laid by Newton in an admirably distinct and compact manner in the sentence of his scholium already quoted, in which he points out its application to mechanics. The *actio agentis*, as he defines it, which is evidently equivalent to the product of the effective component of the force, into the velocity of the point on which it acts, is simply, in modern English phraseology, the rate at which the agent works. The subject for measurement here is precisely the same as that for which Watt, a hundred years later, introduced the practical unit of a '*Horse-power*,' or the rate at which an agent works when overcoming 33,000 times the weight of a pound through the space of a foot in a minute; that is, producing 550 foot-pounds of work per second. The unit, however, which is most generally convenient is that which Newton's definition implies, namely, the rate of doing work in which the unit of energy is produced in the unit of time.

* Edinburgh: Maclachlan and Stewart. Pp. 44.

"Looking at Newton's words in this light, we see that they may be logically converted into the following form:—

"'Work done on any system of bodies (in Newton's statement, the parts of any machine) has its equivalent in work done against friction, molecular forces, or gravity, if there be no acceleration; but if there be acceleration, part of the work is expended in overcoming the resistance to acceleration, and the additional kinetic energy developed is equivalent to the work so spent.'

"When part of the work is done against molecular forces, as in bending a spring; or against gravity, as in raising a weight; the recoil of the spring, and the fall of the weight, are capable, at any future time, of reproducing the work originally expended. But in Newton's day, and long afterwards, it was supposed that work was *absolutely lost* by friction."

This shows that, so far as experimental facts were known in Newton's time, he had the Conservation of Energy complete; the cases of apparent loss by impact, friction, &c. were not then understood.

The opinion of James Bernoulli on a question of this nature would undoubtedly be valuable, but he seems not to have noticed Newton's remark. But I must protest against the allowing any weight to that of John Bernoulli, who, while inferior to his brother as a mathematician, was so utterly ignorant of the principle in question as seriously to demonstrate the possibility of a perpetual motion, founded on the alternate mixing of two liquids and their separation by means of a filter.

I take this opportunity of mentioning, with reference to Mr. Monro's paper in the December Number of the Philosophical Magazine, that in the very paper by Professor Thomson in which the word "naturalist" is used (after Johnson) for Natural Philosopher; Dynamics is divided, as Mr. Monro suggests it should be, into Statics and Kinetics. The same division is employed in the pamphlet above quoted from.

6 Greenhill Gardens, Edinburgh,
December 13, 1864.