

simply one of various possible algebraic function; that is, of which energy is one of various possible quantitative measures, and of which momentum is another such measure. But although the reality of force implies the reality of energy and of momentum, the absolute quantitative definiteness of force does not imply any corresponding quantitative definiteness of energy or of momentum. Now physics is distinguished from metaphysics by being essentially quantitative. It appears, then, that force is a physical reality independent of relation to axes of reference, and that energy and momentum become physical realities only when they are referred to such axes, because when not so referred, they have no quantitative definiteness. They remain, however, when not referred to axes, what may be called non-quantitative realities, and probably many people would choose to call them on that account metaphysical realities.

In conclusion I may offer one remark not strictly bearing upon the subject of this letter, which is the proper PHYSICAL use of the words force and energy, but which was suggested during an explanation of the above definition of force to a friend. There are some minds so constituted that they cannot get on at all without continually referring to metaphysical ideas. This fact should make those whose minds are not so constituted unwilling to believe, as they are very apt to do, that metaphysics is only an unreal, improper, and injurious phantasy or disease of the brain. If there are two such real sciences as metaphysics and physics, in the first place it is clearly advantageous to avoid confusion of the two as far as possible, and we may hope to be able keep them separate from the top down to the base where they rest together, or one upon the other. If there are certain words which it is very convenient to use in both these sciences and with accuracy, it is clear that they must have different definitions, *i.e.*, different meanings in the two. But it would be unfortunate if there were no correspondence between the two meanings. If the two sciences are realities they must consist in two different methods of assimilating as part of our knowledge the same facts; and the statements of the one science ought to be capable of definite translation into the language of the other. And this ought to be held in view in arranging the nomenclature of the two. Now I think that the strictly physical definition of force I have given, *viz.*, the time-rate of transference of momentum, has a true correspondence with the ordinarily accepted metaphysical idea of force as "the cause of the change of velocity in masses." Metaphysically the cause of the acceleration of momentum of the one body is the transference of momentum from the other body, and this transference is also the cause of the retardation of momentum of the other. In the physical definition quantitative accuracy is obtained by introducing the idea of the "time-rate." In a metaphysical definition quantitative accuracy is neither possible nor is it desired, the inherent difference between metaphysics and physics being that the latter is quantitative while the former is not so. The friend to whom I threw out this hint objected that I was here only going one step further back, and that the question became "what was the cause of the transference of momentum?" It was evidently he who had made the step backwards, and of course it was a metaphysical step, not objectionable in itself, but having no bearing on the matter in hand. The above question is no objection to the metaphysical statement or definition, that the cause of the acceleration of momentum is the transference of momentum. If metaphysics is fit to do anything at all it ought to be able to investigate the cause of a cause; but even if it were not able to follow the chain of causes beyond any certain point, that would not constitute any objection to the statements of causative sequence made in following along the chain to the possible limit. The metaphysical answer to the question, "What is the cause of transference of momentum?" would probably be different according to the circumstances of the transference, whether it were by impact or by gravitation, or otherwise. To show, however, that my physical definition of force has a true correspondence to the metaphysical idea, it is quite unnecessary to answer this question, it is unnecessary to go beyond the cause which is called "force" in metaphysics.

ROBERT H. SMITH

Absorption of Water by the Leaves of Plants

I FEEL sure that many of your practical readers will be pleased with the article in NATURE, vol. xix. p. 183, on the "Absorption of Water by the Leaves of Plants," as a correction of a

fallacy long held by many physiological botanists in antagonism to the experience of plain observers of nature.

In reference to the concluding remark on the statements of Prof. Calderon, the following may perhaps be interesting.

Every botanist who visits my Sewage Farm is struck with the luxuriance not only of the cultivated crops, but with that of weeds found growing, out of reach of the hoe, on hedge-banks and places whence it is impossible for their roots to reach the fertilising stream, which readily accounts for the growth of the crops.

It seems clear, therefore, that plants can absorb nitrogenous organic matter which may be wafted over their leaves by winds from a sewage-irrigated field, and I welcomed Mr. Darwin's account of insectivorous plants as a confirmation of my theory; but, after all, no one has ever doubted the power of absorbing carbon through leaves since van Helmont's celebrated experiment with the willow, and it can hardly be unnatural to credit plant-life with the power of obtaining another element of nutrition by the same channel.

ALFRED S. JONES

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The Formation of Mountains

I HAVE deferred replying to Mr. Fisher's letter (NATURE, vol. xix. p. 172) till I had an opportunity of looking at Maxwell's "Theory of Heat;" but, having done so, I am no wiser, for I do not find the point in dispute anywhere referred to. In the "English Cyclopædia," art. "Heat," I find, however, the following statement: "If we suppose the mass of the earth to have been at any remote period at a very high temperature, the effect of the radiation of its heat through the colder surrounding space would be, to cool first the superficial strata, and successively, *though in a less degree*, the internal strata." This slower cooling of the internal parts of a heated mass seems a necessary result of the "law of exchanges," to which the supposed "more rapid cooling of the interior of the globe than the crust" seems as decidedly opposed.

Mr. Fisher's illustration certainly shows how the centre *might* cool more rapidly than the outside, if heat were not subject to laws, and could set the law of exchanges at defiance. He says: "As the people disperse they move off the more quickly the further they get from the dense mass." This would be true for heat, and exactly corresponds to the quotation given above from the "English Cyclopædia;" but it is inconsistent with Mr. Fisher's statement a little further on, that the numbers in an outer belt "may continue about the same, while those in the central crowd become fewer and fewer." The two things are contradictory; and I still fail to see how the "more rapid cooling of the interior of the earth," limited as it must be to that superficial layer within which the effects of solar heat are confined, can be held to furnish a *vera causa* for the compression and contortion of deeply seated rocks and their upheaval into mountain chains.

ALFRED R. WALLACE

Musical Notes from Outflow of Water

EVERY one is familiar with the sounds produced by water running out through a pipe from the bottom of a vessel, when the water-level has got low. The other evening I witnessed a phenomenon of this order, which has, I think, certain interesting features. Desiring to empty my cistern, and the pipes being frozen, I rigged up a gutta-percha tube siphonwise, and brought the water through it. When the orifice of the tube in the cistern got partially uncovered by the descending water-level, a series of rhythmical vibrations was generated, giving a musical note. The plane of the orifice was about vertical; but notes may be had when it is at any inclination with the horizontal water-surface. The intensity of the notes depends, I believe, partly on the difference of level of the vessels; but I cannot furnish exact data as to this, or the way the pitch is affected by various influences (width of pipe, &c.). Would some one proffer an explanation of the "mechanism" or essential character of the phenomenon?

M.

Shakespeare's Colour-Names

MR. BREWIN'S assertion that Shakespeare's "word was doubtless *keen*" (not *green*) in the passage ("so green, so quick, so fair an eye") in "Romeo and Juliet," iii. 5, may be put on a par with his "wonder that the correction was not made long