

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

Havod-y-wern Farm, Wrexham

#### 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

ago." That alteration was made by Sir Thomas Hanmer, and has been rejected by every subsequent editor, and rightly so. "Green" was a common epithet for the eyes, and examples occur in many of our early poets, from Chaucer to Milton. Dyce quotes from H. Weber (*à propos* of Cervantes), "Green eyes were considered as peculiarly beautiful." We have of Neptune, "Thy rare green eye," in "The Two Noble Kinsmen," v. 1, in a passage attributed by some to Shakespeare. That Shakespeare wrote *green* in "Romeo and Juliet" I think beyond reasonable doubt; and if he wrote *green* he certainly meant *green*, and not *blue*: for in "A Midsummer Night's Dream" green eyes are compared to leeks. In our day violet eyes have the precedence over green eyes, yet I think there is still a kind of fascination in the latter. I leave the eagles to the naturalists. *Ne sutor, &c.*

C. M. INGLEY

Valentines, Ilford

## OUR ASTRONOMICAL COLUMN

A VARIABLE STAR OBSERVED BY SCHEINER IN 1612.—In the last number of the "Vierteljahrsschrift der astronomischen Gesellschaft," Prof. Winnecke examines an observation made by Scheiner, of *Rosa Ursina* notoriety, which appears to involve for its explanation the variability of a star at a past time which of late years has exhibited no fluctuation in brightness. In Scheiner's second work, "De Maculis Solaribus," published at Augsburg in 1612, are several letters addressed to his patron, Welser, one of which, dated April 14, 1612, contains observations of Jupiter and his satellites from March 29 to April 8. (It will be remembered that Scheiner regarded the solar spots as in reality solar satellites, which explains the introduction of notices of the satellites of Jupiter in a work professedly relating to sun-spots.) On March 30 he remarked, in addition to the four known satellites of the planet, a fifth star in the same field of view, not observed on the preceding night. This star diminished to invisibility on April 9. Suspecting a slight proper motion, it was regarded by Scheiner as a *fifth satellite* of Jupiter. From figures showing the position of the star with respect to the planet on March 30 and April 7, it may be inferred that they were in conjunction in longitude on the latter day, with a difference of latitude of  $10'$ , the star to the south. Some years since Prof. Winnecke had calculated the place of Jupiter from Bouvard's table for the date of observation, with the view to identify the star which so soon disappeared, but Leverrier's tables for this planet being now available, he engaged Herr Küstner, one of the students at Strasburg, to compute the position of Jupiter for April 7, 1612, at Paris midnight: the geocentric longitude was found to be  $136^{\circ} 13' 4''\cdot3$ , and the latitude  $+1^{\circ} 6' 52''\cdot7$  (differing about  $6'$  from Bouvard's place); hence the position of Scheiner's star, referred to the epoch of the "Durchmusterung"—1855·0, will be in R.A. 9h. 29m. 21·2s., Decl.  $+15^{\circ} 52' 1''$ , thus identifying the object with a star of 8·5m., which the "Durchmusterung" places in R.A. 9h. 29m. 21·4s., Decl.  $+15^{\circ} 53' 5''$ . There are several observations of this star; it occurs in Lalande's zone, 1796, April 4 (No. 18886 of the reduced catalogue), as 8m.; Bessel observed it twice in 1825, estimating it, on February 24, 8m., and on March 12, 7·8m., and Struve using it as a reference-star for Biela's comet on October 26 in the following year, also rated it 7·8m. Again, it was observed by Preuss with the Dorpat meridian circle, in March, 1833, and noted of the same magnitude, so that during this period its brightness appears to have been constant, and Prof. Winnecke adds that repeated comparisons made by himself during the last seventeen years have not indicated any variation. The close agreement of place identifies the star satisfactorily, and he infers that we have here an instance of a star which, though apparently constant during the present century, was variable in Scheiner's

time. Prof. Winnecke remarks upon the interest that would attach to a spectroscopic examination of this object by the possessors of powerful telescopes. Its position for 1880·0 is in R.A. 9h. 30m. 44s., N.P.D.  $74^{\circ} 12' 7''$ . He considers that, notwithstanding Scheiner's inexpressible prolixity, the author of the *Rosa Ursina* does not deserve the severe reproach which he has received at the hands of the astronomical historian, but that he was thoroughly candid in communicating what he had seen, and much acquaintance with his writings has strengthened this opinion.

The unusual phenomenon to which we have adverted appears to have made a strong impression upon Scheiner, who transmitted his observation on the instant to Welser,

THE ZODIACAL LIGHT.—We have already alluded in this column to the very questionable accuracy of a statement so often made in popular astronomical works, that the evening zodiacal light is best seen in these latitudes in March, near the vernal equinox, the inclination of its axis to the horizon being then greater than earlier in the year. Notwithstanding this circumstance, it appears certain that of late years the finest views, or we would say the most conspicuous exhibitions of the zodiacal light have occurred between the middle of January and the middle of February. Many instances of bright displays of the phenomenon during this interval might be mentioned. Thus, on February 6, 1856, Secchi records that the light at Rome was brighter than he ever remembered to have seen it, and of great extent; it was yellowish towards the axis, and while the more conspicuous part of the Via Lactea, in Cygnus, was invisible in a hazy sky at a low altitude, the light was traceable to the horizon; it was slightly curved towards the north, and is described as presenting on the whole "un grande spettacolo;" on this evening, it is added, the rest of the sky was illuminated in an unusual manner. Again, it was in the middle of February, 1866, that Mr. Lassell, during his last residence at Malta, witnessed a remarkable display. He says as he went up to the Observatory the striking brightness of the zodiacal light riveted his attention as never before. It was at least twice as bright as the brightest part of the Milky Way, and fully twice as bright as he ever saw it before, and Mr. Lassell upon this occasion also remarked that its character was quite different to that of the Milky Way, a difference more easily recognised than described; generally it is of a much redder hue. In 1874, in the neighbourhood of London, the most conspicuous displays took place on the evenings of January 14 and 17, and February 18, and in 1875, on January 24, 25, and 30, on the first of these evenings the zodiacal light was surprisingly conspicuous, decidedly reddish, and much excelling any part of the Milky Way. Observations on the position of the apex during these favourable views of late years fully support the conclusion of Prof. Julius Schmidt in his treatise on the phenomenon, published in 1856, that the maximum eastern elongation of the apex falls about the middle of January. Towards the end of March, on the contrary, there is a minimum, according to the Athens astronomer, as regards elongation, breadth, and the inclination of the axis of the light on the north side of the ecliptic.

## BIOLOGICAL NOTES

NEW ASIATIC FISHES.—In the *Annals of Natural History* for 1873<sup>1</sup> was given a translation of Prof. Kessler's description of the new sturgeon, *Scaphirhynchus feditschenkoi*, recently discovered in the Syr Daria or Jaxartes, and a note by Dr. Günther, pointing out the interest attaching to the existence in Northern Asia of a second species of this curious form, hitherto only known from the single species, *S. cataphractus*, of the Mississippi. Recently, however, a second Asiatic species of *Scaphirhynchus* has been discovered.

<sup>1</sup> "On a Remarkable Fish of the Family of Sturgeons," &c. (*Ann. Nat. Hist.*, ser. 4, vol. x.i. p. 26).