And the more complicated the system be, the larger the number of possible combinations of three bodies within it, the greater is the number of experiments or observations we can make to prove that the conservation of momentum is a general physical fact. The larger the number of such observations becomes, the further removed is the doctrine of the conservation of momentum from the character of a logical deduction from definitions.

Still, of course, the doctrine has only to do with relative velocities and relative accelerations of velocities. It loses, however, none of its reality and truthfulness on account of this. Why should not relations be capable of being real, even if not permanent? We are indeed incapable of conceiving anything as real which does not owe its reality in our conception simply to its relations to other things. If objective reality is in any way the opposite of relativity, then, certainly, so far as our knowledge goes, there is no such thing as objective reality. Our notions of momentum and of force, then, are relative to three bodies, and not to two bodies, and this seems to me to be an important point. The ELEMENTARY notion of momentum derived from DEFINITION is relative to TWO bodies only; but the PRACTICAL notion derived from EXPERIENCE is relative to three bodies at least, or to a complicated system of bodies. It should not be forgotten that the physical realities among which we live owe their existence to the complexity of nature. Throughout the complexity there are certain simple invariable relations, and these are the physical laws of nature. The law of conservation of momentum is this: the momentum of one system relative to another system remains unchanged by exchanges of momentum between the parts of the former system. Otherwise stated it is: exchanges of momentum may and do take place between the parts of a system without these exchanges being necessarily accompanied by an exchange of momentum between this system and any other system.

Energy is, of course, a quantity of as relative a character as momentum, although its relativity is not of just the same kind. Energy in general is usually defined as the power of doing work. Curiously enough this definition is frequently followed closely by the statement that a system may possess a very large amount of energy, and yet if there are no differences of potential within it no work can be done by it. The correct statement of what is meant by this last has often been given, viz., that in this case no work can be done by one part of the system upon another part of the same system. But still more often is the inaccuracy indulged in of saying that energy of one kind or another may be transformed into work. Now work is not energy and has no kind of similarity to energy, and therefore energy can never be converted into work. When energy is transferred from one body to another the first does work upon the second, the amount of work done being measured by the amount of energy transferred. The rate at which energy is transferred is the rate of doing work, or the horse-power. The doing of work or more shortly WORK, is the transference of energy from one body to another, but is not the energy itself. The confusion has never entered into the practical use of the word "work," which has always really been applied in the sense here explained, although very probably a good deal of confusion of ideas among both practical and theoretical men, may have been caused by the above noted incorrect statement that energy and work are convertible. The confusion is of the same sort as if we were to use the word force in the sense I have advocated and confuse it with acceleration of momentum. During some transferences of energy there is an invariable transformation of energy. If during the transference, the whole of the energy transferred is also simultaneously transformed, then the rate of doing work is also equal to the rate of transformation, and the amount of work done is numerically equal to the amount of energy transformed. But the phrase "work done" is only used when transference takes place. When a portion of one kind of energy in a body is converted into energy of another kind without any energy leaving the body, it is not the custom to say that work has been done. Work is only done by one body upon another, so that work is the TRANSFERENCE, not the TRANSFORMATION of energy. To say that so much energy has been spent in doing an equivalent amount of work is a convenient and quite allowable mode of saying that this amount of energy has been transferred from the working body without specifying what has become of the energy; that is, without specifying into what other body the energy has been transferred, and without specifying in what form the energy has appeared in the other body. But to say that the energy is converted into work is quite a different thing, and altogether wrong.

When a body possesses in two parts of it two quantities of heat at two different temperatures, the amount of work which the one part has the power of doing on the other in consequence of this difference of temperature is not nearly equal to the whole amount of heat energy in the two parts. Thus the energy in a body is not the power measured quantitatively, possessed by its parts of doing work on each other.

If in a collection of bodies there be a certain one body with a certain amount of kinetic energy, calculated from its velocity, relative to the centre of inertia of the group, that one body might deliver up the whole of this kinetic energy by direct impact upon another body which had zero velocity relative to that centre of inertia, provided these two bodies were exactly alike in certain particulars as to mass and shape. But if there did not exist in the group any body which had this particular relation of shape and velocity to the first, then this first could not possibly deliver up all its kinetic energy, so as to get its velocity relative to the centre of inertia of the whole group reduced to zero. It is thus clear that the internal kinetic energy of a collection of masses is not measured by the amount of kinetic energy calculated from the velocities relative to the centre of inertia of the collection that can be transferred from one part to another.

Also, if another body, or another group of bodies, existed apart from this first group, and possessed a velocity of centre of inertia either zero, or of any other value, relative to the centre of inertia of the first group, the kinetic energy of this first group, measured either relatively to its own centre of inertia, or to that of the other group, or to the centre of inertia of the two combined, could only be wholly transferred to this second group, provided that this second group had very special and very ingeniously contrived relations with regard to mass and configuration to the first group. Thus the kinetic energy of any collection is not measured by the power it may possibly have of doing work upon bodies outside the collection. And quite evidently the same may be said of any other kind of energy possessed by the body.

For each kind of energy we have more or less accurate means of comparing quantitatively different amounts of that kind of energy, and thus of measuring the amount of that kind of energy possessed by a body in terms of the quantity which is adopted as unit of that kind of energy. We have also means of converting different amounts of any one kind into most other kinds of energy; and since in several carefully-made experiments upon the conversion of different kinds of energy there has on the whole been a very fair agreement in the ratios furnished by these experiments between the adopted units of the different kinds, we have come to believe in the truth of the law of conservation of energy—the more especially since this belief is supported by theoretical reasoning based on the hypothesis of the truth of the conservation of momentum. This latter theoretical reasoning, however, we have, hitherto, at any rate succeeded in applying only to transferences of kinetic energy of visible motion, and to the thermodynamics of perfect gases.

But taking this principle of conservation of energy for granted as true, we have the means of measuring the amount of energy of any kind possessed by a body in terms of the adopted unit for kinetic energy of visible motion.

ROBERT H. SMITH

(*To be continued.*)

The Unseen Universe—Paradoxical Philosophy

WILL you permit me to ask through your columns how the idea of the authors—that the present universe is developed out of our unseen universe, which unseen universe is itself developed out of another, and so on in an endless vista up to the unconditioned—works when applied to the present universe as itself developing a lower universe?

The present universe must be a conditioning as well as a conditioned universe, or there would be a breach of the principle of continuity, and there must, on the same principle, be an endless vista of such lower universes.

Have we any hint of any lower universe? Ought we not to have more than a hint? Ought we not to be fully conscious that our own universe is developing and sustaining such a lower universe, to the living intelligent beings in which we are, in fact, supernatural agents, as the angels in the universe above us are to ourselves?

I think that the authors have expanded their idea in one direction only, and I have not seen any reviews of their books applying this idea in the other direction. If, however, this application has been made, I shall be glad to be referred to the passages containing it. W. A. T. HALLOWES

New University Club, St. James's Street, S.W. January 4

Atmospheric Electricity

THE traces afforded by the self-registering electrometer at this observatory show that the conditions of the atmospheric electricity at Kew were very similar during the recent frosts to those observed at Montsouris by M. Descroix. We have, however, in the automatic instrument the great advantage of continuous registration, and therefore our information is not limited to the results afforded by seven observations daily.

The whole period of the frost was characterised by extremely high tension which with us averaged and frequently exceeded the amount which sufficed to derange the French instrument.

The absolute maximum tension recorded equalled 600 volts, and occurred about 4 P.M. on December 16.

The most noticeable feature in the curves of electrical disturbance during the period is that of the daily range of the instrument having attained a maximum usually between 8 to 10 P.M., the tension reaching over 400 volts at the time on the 17th, 18th, and 21st, and over 500 on the 22nd ult.

The fall in tension on the 25th was irregular and the value became almost zero at 6 A.M. on the 26th, for the whole of which day it continued low. Negative electricity was recorded for the first time from 1 to 3 A.M. on the 29th.

Undoubtedly the value of the tension of the atmospheric electricity, as measured by the Thomson electrometer is, as M. Descroix states, only a relative one. We have determined experimentally that with the same instrument the indicated tension is largely influenced by the distance of the nozzle of the water-dropping collector from the wall of the building in which the instrument is placed, and in accordance with a suggestion of Sir W. Thomson, we replace during the passage of thunderstorms our ordinary discharge-tube by a very short one, so as to get the scale of tensions within the range of the electrometer.

Kew Observatory, January 6 G. M. WHIPPLE

Electrical Phenomenon

I HAVE just read in NATURE (vol. xix. p. 182) an account of a strange electrical phenomenon observed at Teignmouth. In connection with it the following incident may be of some interest:—When in Switzerland, not long since, I made with some friends the ascent of Monte Rosa. The weather was unsettled, and on gaining the summit we saw a thunderstorm advancing in our direction from the Italian valleys, and not wishing to turn ourselves into lightning-conductors we deemed it wise to retire from the summit. We had retreated a very short distance along the *arête* when the storm-clouds swept up upon us; the fine snow fell so thick that we could hardly see one another, and we were all suddenly attracted by a peculiar ticking or fizzing from our hair; when I held up my axe the ticking was most distinctly heard from the top of it. The thunder ceased, and we felt that we were acting as points, through which the ground electricity was flowing off into the cloud; if it had been dark, the bluish light observed at Teignmouth might have been visible.

As at Teignmouth, so on Monte Rosa; it was freezing hard when the phenomenon was observed. W. S. GREEN

Alta Terrace, Monkstown, Cork

Time and Longitude

As the questions I propounded under this head in NATURE, vol. xviii. p. 40, have been again alluded to by Mr. E. L. Layard, I may remark that they receive a complete answer in the "Geographical Reader," by C. B. Clarke, M.A. (Macmillan and Co., 1876). At p. 19 he says: "At the town of Sitka, in Alaska, half the population are Russians who have arrived from Russia across Asia; half the population are Americans who have arrived *viâ* the United States. Hence, when it is Sunday with the Russians it is Saturday with the Americans; the Russians are busy on Monday while the Americans are in church on Sunday to the great interruption of business."

It is evident, then, that our new year first commenced in Alaska at 9 A.M. Greenwich time on December 31. Each of our days commences at the same hour and lasts forty-eight hours; the year exists for 366 days. LATIMER CLARK

January 4

Magnetic Storm of May 14, 15

THE magnetic storm of May 14, 15, which was observed simultaneously in England, China, and Australia, and which made itself felt in the telegraph wires of Persia and India, was also perfectly observed in America. Mr. G. F. Kingston, director of the government observatory at Toronto, Canada, has kindly forwarded to me a tracing of his magnetograms, and I find that all the principal inflexions of the declination, as well as of the components of the intensity, bear a striking resemblance to those recorded at the Stonyhurst observatory. The correspondence between the two vertical force curves on May 14 is very remarkable for such distant stations. Comparing the times of the principal minimas in the V.F. trace, and of the chief maximum of the declination, we have the following results in Toronto mean time:—

	Principal V.F. min. P.M.	Secondary V.F. min. P.M.	Decl. Max. P.M.
Toronto Observatory ...	6 17 ...	4 0 ...	6 39
Stonyhurst Observatory	6 42 ...	4 20 ...	6 54
	0 25 ...	0 20 ...	0 15

The disturbing force would thus appear to have been felt somewhat earlier in Canada than in Europe.

The extent of the extreme oscillation of the V.F. magnets cannot be compared, as that at Stonyhurst was too sensitive, and was consequently thrown off its balance; but the rapid movement of the declination needle immediately preceding the maximum was almost identical in England and in Canada, the Stonyhurst curves showing a rise of 28′ 39″ in less than twenty minutes, and that of Toronto an increase of 26′ 53″ in the same time.

It is important to note that I have used the terms maximum and minimum in reference to increase and decrease of ordinate, but it so happens that an increase of ordinate signifies a decrease of H.F. and V.F., and also of W. declination in the Toronto curves, whilst it shows an increase of all these elements in the magnetograms of Stonyhurst. S. J. PERRY

Stonyhurst Observatory, December 28, 1878

Blowpipe Experiment

I BEG to inform you of the following curious results which may be considered of sufficient interest to lead to further investigation of the subject.

Having received a quantity of blowpipe charcoal from Freiberg, about two months ago, I placed two sticks in a "stoneware" jar full of pure water in order to saturate them therewith, so that small squares cut with a saw and placed on aluminium plate as a support, might stand the blowpipe heat longer. I also found that thus treated there is little or no black sawdust, which dirties the hands, &c., more than anything else in blowpipe operations.

Having also placed in the same jar of water two "aluminium spoons" (thick rods about five inches long), I was surprised to find that after the charcoal had sunk to the bottom on saturation, the aluminium rods were covered with semi-opaque roundish *crystals* (part being perfectly transparent) near the surface of the water, and also at the very bottom where the spoons rested on the jar.

Thinking the crystals might be due (although I could not tell how with such a deliquescent substance) to some phosphoric acid I had previously fused upon the aluminium spoons, I cleaned them thoroughly and placed them in fresh pure water with the charcoal about a fortnight ago, and they are again covered with the same kind of crystals. I now carefully scraped the crystals off the aluminium rods with a penknife and placed them on an agate slab, where, when dry, they had a perfectly white, sugary appearance, with some minute transparent fragments. Taking up some of these opaque white fragments upon a hot bead of boric acid, I submitted them to the action of the blowpipe, and found—

(*a*) That they at first emitted a slight yellow pyrochrome, so that they could not be due to *potash*.