only can testing become a real benefit to the administration."

Again he says, "We know quantitatively the electrical state of the lines at all hours of the day, and seasons of the year; we are able to localise faults of all kinds very accurately and repair them with despatch; we test all our telegraphic material, and by it have greatly improved its essential qualities; we are not groping in the dark any more—we *measure and know*."

It never must be forgotten that testing is in reality a physical experiment, and these physical experiments are being conducted every day throughout the whole of our English telegraph system. Our cable electricians under the guidance of Sir William Thomson have carried this system of physical experiment to a high standard of perfection, and our Indian friends would do well to profit by their teaching.

Mr. Schwendler's explanation of the theory of the bridge is not clear, nor does his use of Kirchhoff's corollaries to Ohm's law much help the student. Indeed it is very doubtful whether his proof that the sensibility of the bridge method is greatest when the branch and the resistance are equal is true. At any rate in our practice we find that the more delicate the galvanometer of the bridge the more sensitive and the more accurate is our test.

The most valuable portion of Mr. Schwendler's book is his abstract of Ohm's classical paper, a translation of which is to be found in Taylor's "Scientific Memoirs," and also in his account of Kirchhoff's corollaries to this law.

The practice generally of line testing and testing for faults contains nothing new, but his chapter on natural currents, showing the effect of polarisation of earth plates and the presence of earth currents, is interesting.

He says, also "*Defective insulation at a few points in a line is a fruitful source of currents. At all such points polarisation is produced by the working currents, in a manner precisely similar to that of the earth plates, by the same cause already alluded to, and to a degree dependent on the resistance and the position of the faults. These currents will be strongest in rainy weather, when the line is in contact with trees, when the insulators are covered with dew—in fine, under those circumstances which diminish the resistance of faults and promote electrolytic action.*"

"The stronger the working currents used, and the fewer the defective points, the stronger will be the polarisation currents."

"If these currents become very strong their direction may be reversed by sending for a short time a strong current with zinc to line; and, in such a case, this invariably indicates a single fault in the line or cable." This is a defect which we do not experience in England.

We find that (p. 66) "on all the lines in India positive signalling currents (copper to line) are used in order to have the greatest possible insulation of each line under all circumstances. Now, when measuring the insulation of a line with a positive test current, it is evident that the value obtained must give the insulation much too high, *i.e.*, higher than the line actually has when signals pass through it; because the signalling currents can only have a comparatively small oxidising effect on the line, since only a very small part can escape to earth in the different points of the

line, while a positive testing current, the further end of the line being insulated, must all escape to earth at the defective points of the line. Again, when measuring the insulation of a line with a negative testing current, we get a value which gives the insulation of the line much too low, because negative signalling currents are never used. In the absence of any known law, which would give us how much too high the insulation of the line is obtained with a positive testing current, and how much too low with a negative testing current, we can do nothing better than to take the arithmetic mean of the measured values as representing the insulation the line probably has when signals are passing through it. Of this mean it may, however, be said that it must be always somewhat too low, for the very reason that negative signalling currents are never used, and therefore the arithmetic mean again of the *first mean* and the *positive measured value* would represent a value most probably approximating to the one which the line actually has when signals pass, and which alone is of practical interest and consequence to be known."

The latter part of the book is devoted to fault testing, *i.e.*, to the localisation of the positions of faults.

The book itself is a very valuable addition to the literature of the subject, but we doubt whether it will be of any practical use to our English electricians.

OUR BOOK SHELF

Sketches of Wild Sport and Natural History of the Highlands. By Charles St. John. Illustrated Edition. (London: Murray, 1878.)

MANY of our readers must be familiar with the inimitable "Sketches" of St. John, which has long ago achieved the position of a classic for both the sportsman and the naturalist. We do not know of any descriptions of sport to equal those that abound in these pages, in truthfulness, vigour, and genial humour. To the naturalist who loves to know the habits of an animal in its native haunts, the book must be a treasure; and now that Harrison Weir, Whymper, Corbould, Collins, and Elwes have adorned it with their art, the book should become a greater favourite than ever. No artist equals Whymper in his faithfulness to life in drawing animals. Every picture in the book—and there are about eighty of them—is a masterpiece in its way, and an impressive lesson in natural history. We need only say that the engraver is Mr. J. W. Whymper to convince our readers that the artists' charming work has been faithfully and skilfully rendered. No one can read a chapter of the book without being both refreshed and instructed.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Paradoxical Philosophy

It is strange to see a writer on philosophy like Mr. S. H. Hodgson, as well as physicists so exceptionally able as Prof. Clifford, and now Prof. Clerk-Maxwell, falling into the same errors of observation as more ordinary mortals. Neither the authors of the "Unseen Universe," nor any of the members of the Paradoxical Society, have, so far as I am aware, expressed the

notion that the invisible order of things which continuity requires as antecedent to the visible order, is in any sense *material*. They only assume that it must be conditioned. Indeed, the authors of the "Unseen Universe" have expressed this conviction in the preface to the second edition of their work, in italics, and in language that is not only exceedingly clear, but also extremely strong.

But it seems to be taken for granted on all sides that a man of science can only imagine a mechanical unseen. This is really very hard.

The analogy (however inadequate) furnished by Thomson's vortex atoms, and the invisible fluid which they postulate, is too good an illustration of a novel and difficult conception to be disregarded; but it will have to be laid aside at once, if it can be shown to be necessarily productive of such extraordinary misconceptions even in intellects of the highest order.

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, December 25, 1878

Force and Energy

SINCE a year or two back, when Herbert Spencer started, in the columns of NATURE, a discussion as to the real meaning of the word "force," most careful-thinking students of mechanics have probably come to the conclusion that either the use of the word "force" must be discontinued as a physical scientific term, or that it must be defined in a different manner from that adopted almost universally by those "doctors" whose writings seemed to weigh so heavily on the brain of "poor Publius." They all agree in saying that in its physical application the word "force" means that which produces *i.e.*, the CAUSE of change of momentum. It is needless to give quotations. They are all, except one, curiously explicit. Germans, French, and English agree. "So sehen wir diese Aenderung als Wirkung irgend einer in demselben thätigen Ursache an; diese URSACHE nennen wir Kraft." (Ritter's "Mechanik," p. 36). "On donne, en general, le nom de FORCE à la CAUSE quelconque qui met un corps en mouvement, ou seulement qui tend à le mouvoir." (Poisson: "Traité de Mécanique," Introduction, p. 2.) Although in Tait's "Recent Advances" we find on p. 11 "that we have not yet quite cast off that tendency to so-called metaphysics which has so often blasted," &c., &c.; yet on p. 16 of the same book there is reproduced the fine old crusty Newtonian maxim to which Thomson and Tait and Tait and Steele cling with such fond reverence: "force is any CAUSE which," &c. Clerk Maxwell gives no formal definition of force in his "Electricity and Magnetism." On p. 5 he simply gives its dimensions. On p. 83 of his invaluable "Theory of Heat" he defines, "force is WHATEVER changes or tends to change," &c. This is a very ingenious mode of escaping the difficulty by simply giving no definition at all. We are told what the result of force is, but not what force itself is. We are told that force is "whatever," which is not very clear. Jeames would hardly think that justice was done him if we asserted that the complete definition of him was "whatever opens a door," and made no mention of the fact of his humanity or of his grand plush breeches. It is, in fact, a confusion between a statement of the mode of measuring quantitatively the force, and the definition of the force itself. A physical definition should certainly show clearly what the proper way of measuring the quantity is; but this latter is not the definition itself. Moreover, there may be different almost equally good modes of measurement, all leading to the same numerical result. Clerk Maxwell's definition is clear as to a mode of measuring force, but furnishes absolutely no information as to the nature of the thing intended to be defined. It, therefore, differs from the others in that they are real metaphysical definitions, presumably comprehensible to those who understand metaphysics, while his is no definition at all. Prof. John Perry, in his book on "Steam," adopts the same device as Prof. Clerk Maxwell, substituting the word "anything" for "whatever." Rankine forms a remarkable exception. He says that "force is an action between two bodies either causing or tending to cause change in their relative rest or motion." Here the word "cause" is used in such a sound, practical, common-sense way that no one could take exception to such use of it, even in a physical definition, and probably "action," as here used, might be explained clearly enough for all useful purposes as "a changing relation" or "a change of relation." Rankine, however, does not take the trouble to do this last.

Now clearly a cause is a metaphysical entity, if it is an entity

at all, and from the very nature of the difference between metaphysics and physics, a metaphysical entity cannot possibly be made of any use in physical investigations. If, then, the word force is to be usefully employed in physics, it must be defined as something else than a "cause." When we talk of forces, the physical facts the observation of which we think of, are accelerations of momentum; and in his Glasgow lecture Prof. Tait seems half inclined to use "force" and "acceleration of momentum" as synonymous terms. But an acceleration of momentum is a function of one body only; and every one knows that what is mentioned in Rankine's definition is true, namely, that force is a function of two bodies, and can have neither objective nor any other kind of existence except as a relation between two bodies. Seeing that it is so, I beg to lay before your readers for their favourable consideration the meaning of the word force which I have used for several years past. I wish force to be defined as "time rate of transference of momentum." A transference of anything can only take place between one body and another, and in the transference the amount transferred from the first body to the second is necessarily equal to the amount transferred to the second body from the first. This might seem to be such a truism as to be a mere repetition of words; but we must remember that it is the law of motion which the "transcendently lucid" Newton discovered from his extensive physical experience; and, in order to discountenance scepticism, we might add, by way of parenthesis, that during the transference no spilling takes place.

"Poor Publius" might thus get a hint that there is such a physical fact as conservation of momentum which is independent of all formal definitions. If momentum is conserved, *i.e.*, if it has an enduring existence so that at one time there is no more nor less of it than at another, then during a direct transference of some of it from one part of the system in which it is lodged to another part, the amount lost by the one part must evidently be the same as that gained by the other part. Thus an acceleration or time-rate of gain of momentum to one part necessarily implies a simultaneous equal time-rate of loss of momentum from another part, and also a simultaneous equal rate of transference of momentum from that other to the first part. All these three rates have directions inasmuch as they are time-rates of directed quantities. The first is a rate of gain of momentum, which momentum has a certain direction. If that direction be reckoned positive the gain is one of positive momentum, and the acceleration is naturally reckoned as positive. The second is a rate of loss of momentum of the same direction, *i.e.*, a loss of positive momentum which is equivalent to a gain of negative momentum, and therefore this time-rate is naturally reckoned negative. The meaning of this is simply that the proper physical sign to ascribe to acceleration of momentum is the directional sign of the momentum gained. The two opposite signs of the above two rates have given rise to the idea of two equal and opposite forces acting between the bodies. If the forces were located IN the bodies and not BETWEEN them, the phraseology would be consistent with Tait's definition of force as simply "acceleration of momentum." But I do not hesitate to say that this idea of force is quite unnecessarily out of accord with the commonly received notion of force as a mutual action or relation between two bodies, because in this view force would distinctly have reference to only one body. If, however, we use force to mean the transference of momentum, there is, of course, only one force between the two bodies. The question is what sign is to be given to this force, and it is not quite easy to answer. Force is in this view a flux, a rate of flow of momentum. This flow takes place in a certain direction, and it is the flow of a directed quantity. Are we to take the direction of the flow or the direction of the momentum that flows, to determine the proper sign of the force? These two directions need not be the same. Thus in a bar subjected to tension the flow of momentum is in the direction opposite to that of the momentum itself. In a bar in compression the flow of momentum takes place in the same direction as that of the momentum. In a mass subjected to shearing stress the direction of the flow is perpendicular to that of the momentum. In the case of the attraction of gravitation between two bodies the direction of the flow of momentum is always the exact opposite of that of the momentum that flows from one to the other in whatever way the two may be moving. In the case of impact if we take the direction of the flow of momentum as that of the perpendicular to the surfaces that touch during impact drawn from the body that loses momentum towards the body that gains momentum, then this direction of