tensity of the light is much increased and the impression upon the eye becomes continuous, but in other respects the phenomenon is the same as if there were but one spark.

In order to obtain a measure of the double refraction, which is rapidly variable in time, somewhat special arrangements are necessary. At the receiving end the light, after emergence from the trough containing the bisulphide of carbon, falls first upon a double image prism, of somewhat feeble separating power, so held that one of the images is extinguished when the leyden is out of action. The other image would be of full brightness, but this, in its turn, is quenched by an analysing nicol. When there is double refraction to be observed, the nicol is slightly rotated until the two images are of equal brightness. This equality occurs in two positions, and the angle between them may be taken as a measure of the effect. A full discussion is given in the paper referred to.

The finiteness of the angle, which in my experiments amounted to 12°, is a proof that the light on arrival at the CS_2 still finds it in some degree doubly refracting. To obtain the greatest effect the leads from the leyden to the deflagrator should be as short as the case admits, and the course of the light from the sparks to the CS_2 should not be unnecessarily prolonged. The measure of the double refraction, and in an even greater degree the brightness of the light as received, are favoured by connecting a very small leyden directly with the spark terminals, but the advantage is hardly sufficient to justify the complication.

The observations of Abraham and Lemoine bring out the striking fact that if the course of the light be prolonged with the aid of reflectors so as to delay by an infinitesimal time the arrival at the CS_2, the opportunity to pass afforded by the double refraction is in great degree lost, and the angular measure of the effect is largely reduced. There is here no change in the electrical conditions under which the spark occurs, but merely a delay in the arrival of the light.

The optical arrangements which I found most convenient in repeating the above experiment differ somewhat from those of the original authors. The sparks are taken at a short distance from the polarising nicol and somewhat on one side, and in both cases they are focused upon the analysing nicol. When the course is to be a minimum, the light is reflected obliquely by a narrow strip of mirror situated in the axial line, and focused by a lens of short focus placed near the first nicol. This lens and mirror are so mounted on stands that they can be quickly withdrawn, and by means of suitable guidance and stops as quickly restored to their positions. In this case the distance travelled by the light from its origin to the middle of the length of CS_2 is about 30 cm.

The arrangements for a more prolonged course are similar, and they remain undisturbed during one set of comparisons. The mirror is larger, and reflects nearly perpendicularly; it is placed upon the axial line at a sufficient distance behind the sparks. The light is rendered nearly parallel by a photographic portrait lens of about 18 cm. focus, the aperture of which suffices to fill up the field of view unless the distance is very long. In all cases the eye of the observer is focused upon the double image of the interval between the plates of the CS_2 leyden.

The earlier experiments were made at home somewhat under difficulties. For the blast nothing better was available than a glass-blowing foot bellows; but nevertheless the results were fairly satisfactory. Afterwards at the Royal Institution the use of a larger coil in connection with the public supply of electricity, and of an automatic blowing machine, gave steadier sparks and facilitated the readings. An increase of about one metre in the total distance travelled by the light reduced the measured angle from 12° to 6°, so that the time occupied by light in traversing one metre was very conspicuous.

It is principally with the view of directing attention to the remarkable results of Abraham and Lemoine that I describe the above repetition of their experiment, but I have made one variation upon it which is not without interest. In this case the spark is placed directly in the axial line and at some distance behind, which involves the use of longer leads, and therefore probably of a lower degree of instantaneity. The additional retardation is now obtained by the insertion of a 60 cm. long tube containing CS_2 between the sparks and the first nicol, and the comparison relates to the readings obtained with and without this column, all else remaining untouched. The difference is very distinct, and it represents the time taken in traversing the CS_2 over and above that taken in traversing the same length of air. It should be remarked that what we are here concerned with is not the wave-velocity in the CS_2, but the *group*-velocity, which differs from the former on account of the dispersion.

In the above experiments the leyden, where the Kerr effect is produced, is charged comparatively slowly and only suddenly discharged. For some purposes the scope of the method would be extended if the whole duration of the double refraction were made comparable with the above time of discharge. This could be effected somewhat as in Lodge's experiments, where a spark, called the B-spark, occurs between the outer coatings of two jars at the same moment as the A-spark between their inner coatings. The outer coatings remain all the while connected by a feeble conductor, which does not prevent the formation of the B-spark under the violent conditions which attend the passage of the A-spark. The plates of the Kerr leyden would be connected with the outer coatings of the jars, or themselves constitute the "outer" plates of two leydens replacing the jars. RAYLEIGH.

ENTROPY.[1]

IN NATURE, April 30, 1903, there is an article entitled "Entropy," describing at some length the great practical use which the engineer now makes of the $t\phi$ diagram. Engineers very ignorant of mathematics are able with clearness and certainty to make quantitative computations such as used to task the powers of mathematicians. The problems so easily worked out are very numerous and of a useful, interesting character, and mistakes are not easily made. On the other side of this question, in a notice of Mr. Donkin's translation of Prof. Boulvin's "The Entropy Diagram and its Applications" (NATURE, May 4, 1899), it was pointed out that such books were doing much harm because they made an illegitimate use of the $t\phi$ diagram. Thus I say:—"Of course we may, if we please, say that when steam is released to the condenser, we may imagine the whole change as occurring in the cylinder itself; only we ought to remember that we are substituting a very simple hypothetical process for a very complicated reality, which has almost nothing in common with it. We ought to remember that the very pretty, beautifully complete, cyclic $t\phi$ diagrams, which we obtain from childish assumptions, may get to be looked upon by students, and even by ourselves, as having a real meaning."

It is evident that this misuse of the $t\phi$ diagram is too prominent in Mr. Swinburne's mind, and that he fails to see the real usefulness of ϕ to engineers.

[1] "Entropy or Thermodynamics from an Engineer's Standpoint and the Reversibility of Thermodynamics." By James Swinburne. Pp. x+137. (Westminster: Archibald Constable and Co.). Price 4s. 6d. net.

He seems to think it easy to study some of those irreversible changes which even the greatest of mathematical physicists have been afraid of, and it is my ungrateful duty to say that he is so ill equipped for the study that he does not comprehend the elementary principles of thermodynamics. Even in the last page of this book he states that thermodynamics "is perhaps the most slippery branch of science there is." He does not seem to know that in the books condemned by him there is an exact study of some irreversible processes, such as the wiredrawing of steam, and that the $t\phi$ diagram lends itself to the study of another irreversible process, the efflux of steam from an orifice.

I take it that this mental phenomenon is not, after all, curious; it is often exhibited when men of great individuality refuse to take the usual point of view, refuse to use words in the exact sense in which other people use them, and create a scientific language of their own which prevents mutual understanding with other people. Mr. Swinburne shows that he has not been able to study the subject from the usual scientific point of view; he has a view of his own much like that of David Deans in religious matters. He says:—"So far as I am aware there is not any work on the steam- or gas-engine in this country that gives a correct definition of entropy." Throughout the book he is everywhere severe upon other writers. "Most treatises on physics, English and foreign, contain incorrect definitions of entropy." We wonder whether any English writer would be particularly pleased in being told that his treatise was held by Mr. Swinburne to be one of the exceptions to this sweeping indictment. But at p. 119 he goes further. "I know of no writer who has tried to give any sort of explanation of what is meant by entropy, except that it is the quantity factor of heat, which is obviously nonsense." "As a young man, I tried to read thermodynamics, but I always came up against entropy as a brick wall that stopped my further progress." Of course it was not the simple idea of entropy with which we try to make all students familiar which stopped his progress. It was Mr. Swinburne's own idea, and any persevering person who manages to get through this book will say that this idea of entropy (or these ideas, for there are many and inconsistent) has not only stopped Mr. Swinburne's progress, but may send any ordinary man into a lunatic asylum.

He has not only a view of his own about thermodynamics, but a painful examination shows that he has several points of view of his own. When he occupies one of these his statements sound quite orthodox, but presently the reader finds that he has completely changed his point of view, and it is exceedingly difficult for even a painstaking reviewer to find out what particular kind of mistake he is making. He is dealing with a mathematical subject, and yet he will not keep to one definition of any of the quantities he is dealing with. Because of certain old terms such as "latent heat" being in use, he seems to think that in thermodynamics we do not use the word *heat* in a definite sense, and from all that the ordinary writers of treatises say he is not sure that to them external work is not heat or chemical energy or electron-flights or the energy of pedesis (pp. 116–117). He himself takes great liberties with the word, and it is quite evident that he believes heat to be something not yet defined and not yet measurable. He sometimes uses the word correctly as meaning heat received by the working substance; but mostly he thinks of heat as something *in* the working substance, and in the majority of such cases what he calls *heat* is what we should call "intrinsic or internal energy" (see pp. 15, 16 and 32, where he uses "heat" and "internal energy" indifferently).

Thus, at p. 124, after some vague phrases which he seems to regard as a definition, he says, "this definition of heat includes the heat that makes things hot, and locomotive heat in general, and it also includes 'latent heat' at constant volume, but only part of any misnamed 'latent heat' that includes any form of external work. It includes latent heat of fusion, of vaporisation apart from external work, and of allotropic modification. What is most heterodox is that it includes chemical energy." It is hardly believable that in a dynamical illustration (p. 108) he should imagine the momentum of a system of two colliding bodies to be increased by the collision, in opposition to the most fundamental, most elementary principle of mechanics. Possibly, as in the case of entropy, he attaches a novel meaning to such a term as momentum. Men who use the *poundal* will be interested in a statement on p. 57:—"But as we have the foot-pound and I think, the poundal, as units of energy. . ." I mention only a few of these curious things without comment, because any adequate comment would almost seem to be a personal insult.

He possesses the power of persuasively stating or implying as a major premiss some general notion of his own and then drawing the conclusions which he wants to draw. For example (p. 136), "The fact that certain units in thermodynamics have no names goes to show that the science is not fully developed. Measurement is an essential in science." In the first part of this he implies the great major:—a science is not fully developed (as no science is fully developed, he means "is badly developed") unless the units of the quantities dealt with have names. Is dynamics badly developed? And is there a name for the fundamental unit of all, the unit of momentum? In the second part he implies that there is no measurement if there are no names for the units. Is there no measurement? Is there not the most accurate measurement of momentum? Is mathematics, is Euclidean geometry a science? What are Euclid's names for the units of length or area or volume? Is astronomy a science? What is the name for the units of force or momentum used by Newton? He immediately proceeds to give as an example that there is no name for differences of temperature according to the absolute Kelvin scale. I think that he does not mean the absolute scale of 1848, because that scale is only of historical importance; he probably means the perfect gas scale invented by Clausius in 1850, which Kelvin showed in 1854 to be independent of the nature of the working substance —well, why can he not be satisfied with the name "degree"? Surely he might have tried to suggest a better name.

The name *Rank* is used by many English speaking people for the British unit of entropy, and it even appears sometimes in examination papers; it is most appropriate. But of course, it would be out of the question to expect Mr. Swinburne to use an existing name, so he wishes to have the word *Claus* used for the British unit of entropy. Rankine used this unit always; it is impossible to imagine that Clausius ever did, or that any person not an Anglo-Saxon ever will. This may merely indicate love for the foreigner. Rankine, Cotterill, Ewing and others have given great pains to perfecting tables of the properties of steam. I know that my students and I spent some months on tables that I myself have published. But the only tables of which Mr. Swinburne makes mention are certain American tables which are obviously incorrect in very important particulars. Reeve's tables are certainly elaborate enough, but every one of the 789 values of the volume of a pound of steam happens to be wrong.

He says (p. 68), "The whole nomenclature of thermodynamics demands re-modelling." Of course we all know that there is much in scientific nomenclature which we should like to re-arrange, but his sweeping denunciations are mostly applied to things that are quite correct. For example, "To measure the heat received at constant pressure or temperature by a 'specific heat at constant pressure' or 'a specific heat at constant temperature' is absurd." The book is full of this sort of statement, delivered with the air of Cato the Censor, accompanied by very clever un-Cato-like gibing such as might be expected in a cheap monthly magazine when the writers are poking fun at scientific persons.

It is often quite impossible to find out the author's line of thought. For example, on p. 50, where he says, "$d\phi$ on the other hand is a complete differential in terms of the ordinates of the state diagram in which $pv = R\theta$, but it is not a complete differential with reference to the external work or piston co-ordinates of the Watt diagram." No reader of this book can fail to notice that Mr. Swinburne has some novel idea as to the meaning of "a complete differential," and I have given much thought to the above cryptic statement hoping that it would throw light upon this interesting matter, but, alas! it still rests in the deepest kind of obscurity. Want of clearness does seem, somehow, to be inherent in his study of this "slippery" subject, for in a footnote (p. 35) he states that "Rankine is not clear about his 'thermodynamic function'" (now called entropy by orthodox persons). "He certainly did not develop the idea of entropy and its relation to waste which forms the basis of this book. No doubt a man of his ability, if he had written on steam engines somewhat later" (Rankine's book on steam engines, published in 1859, is not altogether unknown), "would have been not only perfectly correct, but also clear and unambiguous in his statements and definitions." It is evident that Rankine and Bahram, the great hunter of Omar Khayyam, have something in common, and that in this note Mr. Swinburne departs more than usual from the attitude promised by him on p. 4, that he was not writing "in any spirit of superiority." One is inclined to use the language of Tennyson addressing Bulwer Lytton, "What, you a Timon, . . . !" but it is better not to quote the words; they are omitted from the later editions of Tennyson.

Probably the obscurity is deepest in connection with the meaning of a pv diagram. He says (p. 49), "There is considerable confusion as to the meaning of a pv diagram; that is to say, as to what p means in an irreversible change. As a pv or Watt diagram is . . ." I beg to tell Mr. Swinburne that a Watt diagram is not what anybody means (unless when speaking casually and hurriedly) by a pv diagram; that in thermodynamics we are dealing with a quantity of stuff the v, p and t of which are supposed to be known at each instant, and that if we are not so dealing, if we have irreversible changes, to speak of the pressure of the stuff is to talk nonsense; to speak of a pv diagram is to talk nonsense. He says (p. 71), "If the common statement that the area of a $\theta\phi$ is the same as or proportional to that of the pv diagram were correct" (it certainly is correct) "there would be . . ., and all steam and gas engines would have an efficiency of $(\theta_1 - \theta_2)/\theta_1$." I can explain the meaning of this very incorrect statement only on the assumption that Mr. Swinburne does not know the cycle of a steam or gas engine. The context shows that he means by θ_1 and θ_2 (at all events in the case of a steam engine) the highest and lowest temperatures. Now even on the Rankine cycle of the perfect steam engine the above efficiency is not reached, and any other steam engine cycle, even if reversible, known to us, has a smaller efficiency that the Rankine cycle.

I think that most of Mr. Swinburne's mistakes arise somehow from a belief that it is easy, or ought to be easy, to explain exactly what occurs in irreversible processes, and if without attacking other people he set himself to such a study, even so ill equipped as he seems to be for the task, he would have the sympathy of all students of thermodynamics. Most certainly it would be dangerous for me to criticise him, for I myself have given hostages to fortune in that some six years ago I published an attempt to study what occurs when steam is released from a cylinder, and the other irreversible operations in a steam cylinder. The late Prof. Fitzgerald commended my attempt, but I must confess that although I gave much thought to the matter I published it with some expressions of dissatisfaction. I must, however, say something about Mr. Swinburne's discovery, which resembles the famous pill to prevent earthquakes, namely, his $\theta\chi$ diagram. If θ is absolute temperature, $\theta . d\chi$ is the increase of energy "in the form of heat in the body itself." Close study shows that he here means the heat energy received by the body during a small change minus the work done in the body's expansion. Well, this is what we orthodox people call intrinsic energy dE. We may put it, then, in this way: if dH is the heat received by a body the p and θ of which are the same throughout,

$$\theta d\chi = dE = dH - p.dv,$$

or

$$d\chi = dE/\theta = dH/\theta - p.dv/\theta.$$

Now Mr. Swinburne uses a $\theta\chi$ diagram to show the changing state of the water-steam stuff, and so means what we mean when we say that $d\chi$ is a complete differential. As, to Mr. Swinburne, the subject is, as he himself says, "slippery," I would ask him to take no difficult case, no irreversible case, but to take any pv diagram of any steam engine, and he will find that he cannot close his cycle in a $\theta\chi$ diagram. In fact, when p and v and θ and E and ϕ all return to their old values at the end of a cycle χ does not do so. This happens to be a matter of mathematical proof, for if $dH = k.d\theta + l.dv$, Mr. Swinburne's $d\chi$ cannot be a complete differential unless l is equal to p. That is, the substance must be one the intrinsic energy of which is a function of its temperature only. A perfect gas has this property. Changing water-steam certainly does not possess it. If his discovery is found to be worthless in all cases where we have a pv diagram where we can test its value, why should we think it of worth for irreversible cases of which we know so little?

Probably the most curious of his conflicting notions about entropy is what he develops in chapter iv. When heat is being conducted along a bar or through a plate from furnace to water, he speaks of the great growth of entropy. It is useless to point out to him the importance of keeping to one definition. But surely even he must see that there is something quite inconsistent in two of his ideas. First, that if the state of a quantity of stuff is known, its entropy is known. This is, of course, a mere statement of the second law of thermodynamics, and he occasionally admits its truth. Second, a thin slice of bar which is conducting heat keeps in the same state all the time, and yet it is losing entropy continually, that is, it is giving out more entropy than it receives. He introduces a new idea quite inconsistent with his other ideas, that entropy is something which may travel from one body to another. He grudgingly allows us to talk of heat being transferred, or any kind of energy being transferred, but cheerfully introduces this new idea of a peripatetic entropy.

The fact is, so soon as a man departs from the mathematical definition of a quantity like entropy, he is in danger of all sorts of inconsistency. Conduction of heat implies that temperature is *not* constant in the thinnest slice of a bar or portion of fluid, and we have no right to speak of the entropy of a portion of stuff or of its pressure or of its temperature unless it is in the same state throughout. It is obvious that underlying Mr. Swinburne's statements throughout this book it is not always the entropy of a quantity of stuff that he thinks of; it is often the entropy of a quantity of heat, just as if we said:—Heat H in the furnace at a high temperature θ_1 has entropy H/θ_1; in the water of the boiler θ_2 is the much lower temperature, and the entropy H/θ_2 is much greater than in the furnace, and so on. Wherever there is conduction or any kind of irreversible operation there is a growth of entropy. This sort of representation is familiar to all users of the $\theta\phi$ diagram, but they know how to put the matter quite clearly (see NATURE, April 30, 1903) without using terms in a wrong sense, without confusion of ideas, without condemning wholesale what other men have written, without contradicting the fundamental laws of thermodynamics.

This notice may seem to be unduly long; I may seem to waste valuable space in NATURE and give undue importance to an unscientific book. But unhappily it is necessary. Mr. Swinburne's vague denunciations of writers on thermodynamics in letters and articles to the engineering papers have done a great deal of harm to young engineers, and I am peculiarly bound to the very ungrateful task of pointing out his mistakes. A writer who proves that the earth is flat deserves no notice, for he can do no harm, but although Mr. Swinburne's heresies are just as unscientific, just as absurd, they must be noticed and condemned. He uses a jargon which sounds quite scientific to a young engineer; he involves a reader in his mistakes so persuasively that if this reader is an earnest young engineer I feel sure that he must get utterly discouraged with the idea that the study of thermodynamics can be of any use to him. Probably the best of antidotes to this poison are the two articles in NATURE referred to at the beginning of this notice.

JOHN PERRY.

AGRICULTURAL EDUCATION AND RESEARCH IN INDIA.

THE last mail brings an issue of the Allahabad *Pioneer*, containing the resolution of the Government of India regarding the establishment of an agricultural college and research station at Pusa, in Bengal. It will be remembered that Mr. Henry Phipps gave a sum of 20,000*l*. to be devoted to whatever object of public utility (if possible in the direction of scientific research) the Viceroy might prefer, and on the decision to create with this sum an imperial centre for agricultural investigation Mr. Phipps increased his donation by another 10,000*l*. It was at first proposed to make the existing laboratory at Dehra Dun the nucleus of the new work, but the superior advantages offered by the estate at Pusa have resulted in the decision "to make Pusa the headquarters of the Imperial Agricultural Department, and to establish there the laboratories required by the experts, combining with them farms which will offer every convenience for practical work, and an agricultural college." For this purpose the estate has been transferred from the Government of Bengal to the Government of India, and the existing staff at Dehra Dun will move to Pusa when the laboratories are ready, which is expected to be in September, 1905.

The agricultural college is intended to serve not only Bengal, but the whole of India, and to provide a supply of trained men, who "will be required to fill posts in the Department of Agriculture itself, such as those of assistant directors, research experts, superintendents of farms, professors, teachers, and managers of court of wards and encumbered estates."

At the research institute it appears that the staff is to consist of two chemists, one being specially concerned with bacteriology, two botanists, one cryptogamic, the other "biological," and an entomologist.

This scheme ought to grow into an institution of the utmost value to India, a country which is full of agricultural industries, involving great interests, yet proceeding wholly by rule of thumb tempered by occasional analyses performed in London. Systematic investigations of the conditions of the industry on the spot have been wanting except latterly among the tea-planters of Ceylon and Assam. Indigo growing affords a case in point; for years it was obvious that the natural product was going to meet with severe if not ruinous competition, yet nothing was done until the artificial indigo had reached the position of being able to undersell the Indian article, then at last a chemist and a bacteriologist were hurried out to try to save the failing industry. But how can the most eminent scientific man be expected to descend from Europe like the god from the car and revolutionise an old and complicated business at sight?

The new institute at Pusa will be well situated among some of the best agricultural developments in India, so that the scientific staff will have an opportunity of learning where their skill can be of service to the cultivator, and of trying to keep this or that industry in a healthy condition instead of being called upon to resuscitate it when *in extremis*. There may be even now a chance for the grower of indigo if only he is given some of the systematic scientific effort which has hitherto been the monopoly of his competitor.

NOTES.

PRESS messages from New York contain an account of the discovery, by Prof. Baskerville, of the University of North Carolina, of two new elements possessing somewhat remarkable properties. By distilling thorium oxide in a quartz tube with carbon and chlorine there are produced a greenish condensable vapour to which the name berzilium is given, and a crystalline, pinkish substance which adheres to the quartz tube and is named carolinium, whilst a certain quantity of thoria remains unchanged in the tube. Prof. Baskerville has at his disposal 5 grams of carolinium and 2·5 grams of berzilium, presumably in the form of volatile chlorides. In a lecture before the Chemists' Club Prof. Baskerville exhibited the two elements in a darkened room, and showed that each of them is capable of shedding an illumination through tubes of copper, brass, iron and glass, all covered with cloth. Further investigations are in progress, in which Prof. Zerban, of Berlin, will cooperate.

PROF. R. W. BOYCE, F.R.S., has been appointed a special advisory member of the committee of the African trade section of the Liverpool Chamber of Commerce on matters relating to health and sanitation.

REUTER'S Agency is informed that the British Antarctic vessel *Discovery*, with Captain Scott and his staff, is not