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SIR WILLIAM THOMSON'S "MATHEMATICAL AND PHYSICAL PAPERS"

Mathematical and Physical Papers. By Sir William Thomson. Vols. I. and II. (Cambridge University Press. 1882, 1884.)

EVERY one interested in the study of physics of the more profound kind will welcome this collection of essays by the celebrated natural philosopher, so many of which, hitherto scattered throughout various periodicals, difficult to gather together, or even wholly inaccessible to readers out of the reach of large public libraries, are yet of decisive importance for those chapters of the science to which they refer. With the two volumes now before us, in conjunction with the late publication, "Reprint of Papers on Electrostatics and Magnetism," the collection is now completed down to the date of February, 1856. Vol. II. contains, besides, all that the author has written on the Transatlantic Telegraphs, which, according to the strict order of time, might have been looked for in later volumes. The first volume begins with a series of essays, for the most part of a mathematical nature, ranging from the year 1841 to 1850. So far as these essays relate to physical problems, their chief interest turns on the difficulties connected with the analytic method. These difficulties were, however, even at that early period, treated by the youthful author with great skill, and under comprehensive points of view. The problems are, in part, geometrical and mechanical, referring to lines of curvature, systems of orthogonal surfaces, principal axes of a rigid body, &c. Most of them, however, deal with the integration of the differential equations, on which is based the doctrine of thermal conductivity and potential functions. The latter, as is well known, form the mathematical foundation of a large number of chapters in physics—the doctrine of gravitation, of electrostatical distribution, of magnetic induction, of stationary currents of heat, of electricity and of ponderable fluids. By treating all these problems collaterally and rendering concretely in some what in others appears in the highest degree abstract, the author has succeeded in overcoming the greatest difficulties, and we can only recommend every student of mathematical physics to follow his example. A field particularly favourable for the exercise of his powers was opened up to Sir W. Thomson by the phenomena, newly discovered by Faraday, in diamagnetic and weakly magnetic bodies, crystalline as well as uncrystalline. These our author rapidly and easily succeeded in arranging under comprehensive points of view. One great merit in the scientific method of Sir William Thomson consists in the fact that, following the example set by Faraday, he avoids as far as possible hypotheses on unknown subjects, and by his mathematical treatment of problems endeavours to express the law simply of observable processes. By this circumscription of his field the analogy between the different processes of nature is brought out much more distinctly than would be the case were it complicated by widely-diverging ideas respecting the unknown interior mechanism of the phenomena.

From the year 1848 and onwards there follows a long series of important investigations into the fundamental problems of thermo-dynamics. These start first with Saadi Carnot's conclusions respecting the mechanical functions of heat arrived at before J. P. Joule had experimentally demonstrated the equivalence of heat and mechanical energy. At the time when Carnot published his investigations heat was, by the majority of physical scientists, deemed an imponderable substance capable of flowing from one body to another, of entering occasionally into a more intimate kind of union with ponderable matter, and becoming, so to say, chemically united with it, under changes in the state of aggregation and under chemical processes. According to this older view temperature signified as much as the pressure under which the imponderable fluid stood in the warm bodies. In the case of a great number of thermal processes heat, in point of fact, acts entirely like a substance, showing the constancy of quantity, which is the most characteristic criterion of substances. In this way large sections of the doctrine of heat, embracing great bodies of facts, could very well be treated under the substantial conception of this agent—such, for example, as the exchange of heat between different bodies, the confinement and liberation of latent heat, the chemical production of heat. All that was necessary to render the substantial conception of heat apparently satisfactory was but to leave out of account all cases in which other forms of work are produced by heat or in which heat is produced by such. Cases of this kind then known were indeed very few, whereas the sections of the doctrine of heat already referred to were exactly those which till towards the middle of this century engaged the attention of natural philosophers. Carnot's highly acute investigation was an attempt to bring the phenomena likewise of the performance of work by means of heat into harmony with the assumption of the substantial theory of heat. The result of this endeavour was remarkable enough. He showed, namely, that heat was capable of performing mechanical work only when a quantity of it passed from a body of higher temperature into another body of lower temperature. A complete analogy thus seemed to be established between heat and those gases which through their pressure are capable of performing work, expanding, as they do, and abating their pressure in a measure corresponding with their expansion. The heat of a warm body corresponds in a manner with a compressed gas; it diffuses itself in space, passing into neighbouring bodies, to the lowering of the temperature of the body in which it was originally compacted.

Carnot's deductions, although based essentially on the erroneous assumption that the quantity of heat was constant like that of a substance, proved in reality correct so far as they respected transitions of heat within very narrow limits of temperature. They cease, however, to be strictly accurate when they are extended to wider intervals of temperature, for in that case finite parts of the transferred heat become transformed into work and no longer continue as heat. We now know through the experiments of Joule that heat does not possess the absolute constancy of a substance, but only the relative constancy of an equivalent of work which, to be sure, can neither be produced from nothing nor come to nothing but is yet capable of being transferred into other forms of equivalents of work which may be presented in a very diverse and hardly recognisable manner.

In his first Essays, Art. XXXIX., "On an Absolute Thermometric Scale," and Art. XLI., "An Account of Carnot's Theory of the Motive Power of Heat," dating from the years 1848 and 1849, our author still occupies essentially Carnot's standpoint, but he nevertheless calls attention to the fact that the argument adduced by Carnot in support of his theorem, apparently valid though it was in all points, was yet defective if the experiments by Joule, which were just then made known, should be confirmed, according to which heat might be generated anew by work (vol. i. p. 116). That which more immediately directed Sir William Thomson's studies to this subject was the possibility of attaining, in accordance with Carnot's theorem, to an absolute scale of temperature, and he endeavoured to utilise the observations which Regnault had shortly before carried out with special care in reference to the pressure and latent heat of steam for he purpose of calculating such a scale. But in doing so, he was obliged to apply the hypothesis, not perfectly exact in this case, that the density of steam was to be calculated from pressure and temperature according to the laws of gases.

The theory of Carnot next obtained highly surprising confirmation from the theoretical deductions drawn by Prof. James Thomson, the elder brother of Sir William, touching the alteration of the freezing-point of water in consequence of differences of pressure. The accuracy in point of fact of this deduction was experimentally demonstrated by Sir W. Thomson. This was a discovery which perhaps more than any other served to draw the attention of physical scientists to the accuracy and the importance of Carnot's theorem.

Meanwhile our author, no longer able to doubt the correctness of Robert Mayer and Joule's thesis respecting the equivalence of heat and work, devoted himself to the problem of how Joule's and Carnot's laws might be combined. This question he answered in his treatise of March, 1851, "On the Dynamical Theory of Heat," Art. XLVIII. Prof. Clausius, in Germany, had, however, been busied with the same problem, and had published the results at which he arrived before Sir W. Thomson, in May, 1850. The essential results of the two investigations coincided exactly; only in their numerical values for the absolute scale of temperature, the two authors had started with two different hypotheses, and had therefore reached different conclusions. Sir William Thomson had, as above mentioned, calculated the density of steam from pressure and temperature, as if for complete gases, whereas Prof. Clausius had accepted the hypothesis set up by Robert Mayer, according to which the work of a gas expanding itself was exactly equivalent to its loss of heat. Later on, when his opponents set forth the unsatisfactory basis of this hypothesis, Robert Mayer pointed to an old and very little-known experiment of Gay-Lussac, according to which a gas diffusing itself in empty space without encountering any resistance suffered no diminution of heat. The same experiment was afterwards carried out by Joule without his having any knowledge of the earlier observation of a similar nature. This form of the experiment was, however, as a whole, not fitted to yield very precise results, seeing that the mass of air available for it, whose consumption of heat was to be measured, was necessarily very small in comparison with the mass of water of the calorimeter. It was not till the investigations into the changes of temperature undergone by a mass of gas made to pass through a very dense porous substance—an investigation carried out in common by J. P. Joule and Sir W. Thomson, in 1852, and described in Art. XLIX., "On the Thermal Effects of Fluids in Motion"—that it was demonstrated how, in point of fact, R. Mayer's hypothesis was accurate to within a very close degree of approximation, although not with absolute precision, in respect of hydrogen and atmospheric air, whereas carbonic acid showed greater deviations.

To this have to be added extended investigations into thermo-electric currents, and the equivalent of their operations (Appendix to Art. XLVIII. and Art. LI. "Experimental Researches in Thermo-electricity," Vol. I., Art. XCI. Bakerian lecture, pp. i., ii., and iii., Vol. II.). In a thermo-electric chain which, from its conducting wire, sets magnets in motion, or generates heat in them, the heat conducted to the soldering seams is manifestly the source of the operations. We know that in such a case, according to the important observations of Peltier, heat disappears from the warmer soldering seam, and becomes developed in the colder. That is, in fact, the condition, according to Carnot's law, under which heat becomes transferable into other forms of work. This particular process was, however, of special interest for the universal validity of the theory, seeing that the work of heat is here produced under conditions altogether different from those of the steam-engine and hot-air engine. Our author was by this investigation led to the conclusion that, contrary to the opinion hitherto entertained, it was not in the soldering-seams of the metals, at all events not in those alone, but in the whole length of the wires, by a process which he calls "electric convection of heat," that the essential cause of the thermo-electric force was to be sought; and, in point of fact, he succeeded by a series of very laborious and subtle experiments in demonstrating that the conduction of heat in iron proceeded more rapidly in the direction of the current of negative electricity, and in copper in the direction of the positive current.

In the first volume of the book which is the subject of notice, the consecutive stages may thus be followed in the development of one of the most remarkable chapters in the history of discoveries, a chapter specially remarkable also as an example of how discoveries are arrived at in a manner not always rational. The course of this discovery reminds one in some measure of the invention of achromatic telescopes. Starting with the erroneous supposition that the eye of man was achromatic, Euler inferred that Newton's assumption of the proportionality between refraction and dispersion of light was false, and that his conclusion as to the impossibility of achromatic telescopes was without foundation. Thereupon Euler gave the receipt for the making of achromatic telescopes—a correct conclusion from a false premiss; similar to the case of Carnot with the doctrine of heat. After all the confirmations which have been obtained in the different branches of physics for the validity of the deductions of the corrected Carnot law there can hardly longer remain any doubt that we have here found one of the most comprehensive and important laws of nature of unlimited applicability. Down to the present moment we are, however, not yet in a position to derive a complete argument for its truth from the general principles of kinetics. Our analytic methods are inadequate even to the problem of completely determining the movement of three bodies reciprocally attracting one another. In the case, however, of motion which we perceive as heat, there are myriads of atoms engaged, all in the most irregular movement, and influenced by forces the nature of which is still almost wholly unknown to us. It is highly probable that the peculiar difficulty of reducing thermal motion into other forms of mechanical energy, which is expressed in Carnot's thesis, is due to the circumstance that thermal motion is a completely "unregulated" movement, that is, that there is no kind of similarity between the movements of atoms immediately neighbouring one another. Even in the case of the most rapid vibrations of light and sound, on the other hand, the movements and conditions of neighbouring atoms are so much the more similar to one another the nearer they are to one another. These, therefore, I am in the habit of calling "regulated" in antithesis to thermal motion. Sir W. Thomson has introduced for this conception the name of "dissipation of energy." Prof. Clausius denotes the quantitatively determined measure of the same magnitude by a more abstract name, "entropie." The dissipation of energy is capable, according to Carnot's law, by every known process of nature in the inorganic world, only of constant increase, never of decrease, and this leads to the much-talked-of conclusion that the universe is tending towards a final state of absolute unchangeableness with stable equipoise of all its forces under the establishment of complete equipoise of temperature, as our author expressed it in the year 1852 (Art. LIX., "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy").

On the other hand the ascertained laws of dynamics yield the deduction that if we were able suddenly to reverse the total movements of the total atoms of an isolated mechanical system the whole system would of necessity retraverse all the states which up to that point of time it had passed through. Therewith also would all the heat generated by friction, collision, conduction of electrical currents, &c., return into other forms of energy, and the energy which had been dissipated, would be all recovered. Such a reversion, however, is a postulate beyond the power of human means to fulfil. We have no agency at our disposal by which to regulate the movement of atoms. Whether, however, in the extraordinarily fine structure of organic tissues a mechanism capable of doing it exists or not is a question not yet to be answered, and I deem it very wise on the part of Sir W. Thomson that he has limited all his theses respecting the necessity of increasing dissipation by restricting their validity to "inanimate matter."

The recognition of this scientific law of so universal applicability and so rich in consequences is, be it repeated, due in the first place, through Carnot, to an erroneous assumption regarding the nature of heat. The universal demonstration given by him of the principle, a demonstration which in his day appeared completely satisfactory, is based purely on this assumption. And, what is still more noteworthy, it is hardly to be supposed that the principle in question could have been deduced from the more correct view—namely, that heat is motion, seeing that we are not yet in a position to establish that view on a completely scientific basis. The two natural philosophers, moreover, who brought Carnot's and Joule's principles into harmony with each other, and whom we have to thank for our present knowledge on this subject, are able to refer their conclusions only to an axiom generalising the experience that heat tends ever to expand, never to concentrate. Sir W. Thomson expresses this axiom in the following terms:—"It is impossible by means of inanimate material agency to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects."

The reviewer has, further, succeeded in demonstrating that the peculiar limitation affecting the transformation of heat into other forms of work likewise applies to other classes of motions revolving on themselves, so long as no external forces are brought into play directly opposing or accelerating the internal motion.[1]

When by J. P. Joule's experiment it was demonstrated that the basis of Carnot's proof was defective, it might have been apprehended that along with the element of error the element of truth in it would also be rejected. It must therefore be regarded as a special merit on the part of Prof. Clausius and Sir W. Thomson that, while removing the mistakes, they brought the truth into precise expression and into universal recognition, and that the recent theory of heat has become so fruitful in discoveries respecting the most secret connections between the different physical qualities of bodies in nature.

The second volume of these Reprints contains chiefly the researches having relation to the laying of the first submarine telegraph cable. The motion of electricity in these cables undergoes a peculiar retardation in consequence of the fact that the conducting wire separated from the sea-water, which is likewise a tolerably good conductor, only by a thin isolating layer of gutta-percha, forms an enormous Leyden jar, which must first be charged with the electricity entering it before the current will pass with full force along the whole length of the wire to the other end. The physical laws of the processes which here come into play were generally known, but a far-searching mathematical investigation was still needed to determine the whole procedure of these currents and to ascertain the amount of influence exercised on them by the dimensions and conductivity of the wire, by the neighbourhood of other wires, and by the particular quality of the gutta-percha, as also to arrive at a knowledge of the conditions under which the most rapid series of signals might be transmitted and received at the opposite end.

All these questions our author disposed of thoroughly and exhaustively, having also to contend with opposition to his views based on observations made under restricted conditions on other cables. He was then a comparatively little-known young man, and did not enjoy that recognition and authority now everywhere freely accorded him.

To this were joined mechanical problems connected with the sinking or eventual raising and repairing of

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[1] H. von Helmholtz, "Studien zur Statik monocyklischer Systeme." Sitzungsberichte der Berliner Akademie, 1884, März 6, 27, und Juli 10.

the cable; further, the construction of telegraphic signal apparatuses able to utilise the first weak beginnings of the current arriving at the other end of the cable. These ultimately led to the invention of the siphon-recorder—a writing apparatus in which the tube containing the ink does not come into immediate contact with the strips of paper on which it has to write, and is therefore not hindered by friction from moving even under the least electro-magnetic impulse. By electric charges it is brought about that the ink spurts over the paper in a series of fine points.

The conclusion of the second volume is formed by the Bakerian Lecture for 1856, which gathers up the results of the author's investigations into the qualities of metals as displayed under the conduction of electric currents, and under magnetisation, and the changes they undergo in consequence of mechanical, thermal, and magnetic influences.

Let us hope for an early continuation of this interesting collection. There are still nearly thirty years of scientific activity on the part of the author to be accounted for. When we think of that we cannot fail to be astonished at the fruitfulness and unweariedness of his intellect.

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OUR BOOK SHELF

Paradise Found. The Cradle of the Human Race at the North Pole. A Study of the Prehistoric World. By William F. Warren, S.T.D., LL.D., President of Boston University, &c. (London: Sampson Low and Co.)

IT has come to be an understood thing that when geologists or biologists propound theories as to past stages of life on the earth, and these theories attain to a certain popularity, some theologian shall twist the words of the Book of Genesis into a new interpretation, to show that this was what the inspired author meant all the time. A fresh musician has set Moses to dance to a new scientific tune. Since the publication of well-known modern views as to the diffusion of plants and animals from the Polar Region, it was to be expected that we should have a book proving that man was created in an Arctic Paradise with the Tree of Life at the North Pole; and here the book is. Other ancient cosmologies, such as the Greek and Indian, are made to bear their not always willing testimony. Those who take up the book should notice that the commendatory letters published from Professors Sayce, Tiele, and Whitney do not at all imply that these eminent scholars countenance the Polar Paradise doctrine. The President of Boston University seems to have sent them a paper some years ago on "Ancient Cosmology and Mythical Geography," their acknowledgments of which they are now perhaps hardly delighted to find figuring as certificates in a "Paradise Found."

Epping Forest. By Edward North Buxton, Verderer. (London: Stanford, 1885.)

THE public generally, and especially the people of London, and those who take some interest in natural history, are to be congratulated on the acquisition of so charmingly complete a little itinerary of Epping Forest as that now issued in a cheaper form by one of the Committee of Conservators, who is a resident on the borders, and an enthusiast as to the attractions of the Forest. It is, as the author observes in his preface, "hardly a desirable state of things" that so small a percentage of the summer visitors to the Forest "ever venture far from the point at which they are set down by train or vehicle;" and, with the choice of a score of beautiful walks, described in Mr. Buxton's book, and the guidance of his six carefully prepared maps, five of which are on the scale of three inches to the mile, there is no longer any reason for their not venturing into those depths of the Forest in which its chief beauties are to be seen. The chapter on the history of the Forest which the author has wisely prefixed to the itinerary, that visitors may be reminded of the events which secured this magnificent playground for their enjoyment, is most complete, though it is to be regretted that the late City Solicitor, Sir Thomas Nelson, is not mentioned *by name* on p. 22. The practical character of the book may be gauged from the inclusion of railway time-tables, the fact that the distinctive letters of each route have been cut on trees at some points, and from such suggestions as that an east wind is, in Epping Forest, the best for views, because not smoke-laden. Personal experience has convinced the present writer of the skill with which the routes have been selected; the "objects of interest within and around the Forest," and their historical associations, are fully described and illustrated by some excellent drawings, the latter by Mr. Heywood Sumner; but what must render the work peculiarly gratifying to all lovers of nature, is the ample space—more than half the volume—devoted to the fauna and flora of the Forest. The mammals, reptiles, birds; the chief moths and butterflies; the trees, flowering plants, ferns, fungi, and mosses, are all enumerated, with general, *i.e.* not too specific, localities; and the notes on the mammals and birds will be of interest to naturalists in other districts. Such lists can, fortunately, never be complete; insects marked as "rare" are notoriously liable at any time to prove common: even since the publication of this work evidence has been produced suggesting the addition of *Sparganium neglectum* to the list of flowers, and each year's cryptogamic meeting of the Essex Field Club has as yet added several species to the catalogues of the lower plants. There may yet be room for a more pretentious monograph of Epping Forest, and, of course, from the naturalist's stand-point, so rich a collecting-ground affords material for a library of expository literature—the freshwater algæ, for example, call for recognition;—but, for its purpose, the present work could hardly have been executed in a manner more creditable both to author and publisher.

G. S. BOULGER

Traité de Minéralogie appliquée aux Arts, à l'Industrie, au Commerce et à l'Agriculture, &c. Par Raoul Jagnaux. Avec 468 figures dans le texte. (Paris: Octave Doin, Éditeur, 1885.)

THIS work of 883 pages, as is stated in a title-page of corresponding length, is intended for the use of French students in their preparation for a degree in the subjects of engineering, chemistry, metallurgy, &c. We do not think that in its purely scientific contents it is likely to be of advantage to English students. The first part, devoted to the subject of crystallography, is rather incomplete and unsatisfactory, even if regard be had to the main purpose of the work. As usual, in the figure of Wollaston's goniometer the crystal is represented as adjusted in a way that every practical student is immediately taught to avoid. Nor will the chemical formulæ meet with the favour of English students: though the atomic weights of oxygen and silicon are given as 16 and 28 respectively, silica appears throughout as SiO_3, water is still HO, while to nitre is assigned the formula $KO . AzO_5$. Further, the ordinary symbols for the atoms are occasionally, as in the forty-nine formulæ of pp. 423-5, used to signify equivalent proportions of the oxides; olivine, for instance, being given as $(Mg . fe)Si$. The classification is likewise ancient; in the description of the species alum stone immediately follows the oriental chrysolite, a precious stone, merely because both substances contain alumina. In its explanation of the uses which have been discovered