THURSDAY, JANUARY 31, 1878

## TAIT'S "THERMODYNAMICS"

*Sketch of Thermodynamics.* By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. Second Edition, revised and extended. (Edinburgh: David Douglas, 1877.)

THIS book, as we are told in the preface, has grown out of two articles contributed in 1864 by Prof. Tait to the *North British Review.* This journal, about that time, inserted a good many articles in which scientific subjects were discussed in scientific language, and in which, instead of the usual attempts to conciliate the unscientific reader by a series of relapses into irrelevant and incoherent writing, his attention was maintained by awakening a genuine interest in the subject.

The attempt was so far successful that the publishers of the *Review* were urged by men of science, especially engineers, to reprint these essays of Prof. Tait, but the *Review* itself soon afterwards became extinct.

Prof. Tait added to the two essays a mathematical sketch of the fundamental principles of thermodynamics, and in this form the book was published in 1868. In the present edition, though there are many additions and improvements, the form of the book is essentially the same.

Whether on account of these external circumstances, or from internal causes, it is impossible to compare this book either with so-called popular treatises or with those of a more technical kind.

In the popular treatise, whatever shreds of the science are allowed to appear, are exhibited in an exceedingly diffuse and attenuated form, apparently with the hope that the mental faculties of the reader, though they would reject any stronger food, may insensibly become saturated with scientific phraseology, provided it is diluted with a sufficient quantity of more familiar language. In this way, by simple reading, the student may become possessed of the phrases of the science without having been put to the trouble of thinking a single thought about it. The loss implied in such an acquisition can be estimated only by those who have been compelled to unlearn a science that they might at length begin to learn it.

The technical treatises do less harm, for no one ever reads them except under compulsion. From the establishment of the general equations to the end of the book, every page is full of symbols with indices and suffixes, so that there is not a paragraph of plain English on which the eye may rest.

Prof. Tait has not adopted either of these methods. He serves up his strong meat for grown men at the beginning of the book, without thinking it necessary to employ the language either of the nursery or of the school; while for younger students he has carefully boiled down the mathematical elements into the most concentrated form, and has placed the result at the end as a *bonne bouche*, so that the beginner may take it in all at once, and ruminate upon it at his leisure.

A considerable part of the book is devoted to the history of thermodynamics, and here it is evident that with Prof. Tait the names of the founders of his science call up the ideas, not so much of the scientific documents they have left behind them in our libraries, as of the men themselves, whether he recommends them to our reverence as masters in science, or bids us beware of them as tainted with error. There is no need of a garnish of anecdotes to enliven the dryness of science, for science has enough to do to restrain the strong human nature of the author, who is at no pains to conceal his own idiosyncrasies, or to smooth down the obtrusive antinomies of a vigorous mind into the featureless consistency of a conventional philosopher.

Thus, in the very first page of the book, he denounces all metaphysical methods of constructing physical science, and especially any *à priori* decisions as to what may have been or ought to have been. In the second page he does not indeed give us Aristotle's ten categories, but he lays down four of his own:—matter, force, position, and motion, to one of which he tells us, "it is evident that every distinct physical conception must be referred," and then before we have finished the page we are assured that heat does not belong to any of these four categories, but to a fifth, called energy.

This sort of writing, however unlike what we might expect from the conventional man of science, is the very thing to rouse the placid reader, and startle his thinking powers into action.

Prof. Tait next handles the caloric theory, but instead of merely showing up its weak points and then dismissing it with contempt, he puts fresh life into it by giving (in the new edition) a characteristic extract from Dr. Black's lectures, and proceeds to help the calorists out of some of their difficulties, by generously making over to them some excellent hints of his own.

The history of thermodynamics has an especial interest as the development of a science, within a short time and by a small number of men, from the condition of a vague anticipation of nature to that of a science with secure foundations, clear definitions, and distinct boundaries.

The earlier part of the history has already provoked a sufficient amount of discussion. We shall therefore confine our remarks to the methods employed for the advancement of the science by the three men who brought the theory to maturity.

Of the three founders of theoretical thermodynamics, Rankine availed himself to the greatest extent of the scientific use of the imagination. His imagination, however, though amply luxuriant, was strictly scientific. Whatever he imagined about molecular vortices, with their nuclei and atmospheres, was so clearly imaged in his mind's eye, that he, as a practical engineer, could see how it would work.

However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts.

Certain phenomena were to be explained. Rankine set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particular arrangement of mechanism competent to do the work, and also in predicting other properties of the mechanism which were afterwards found to be consistent with observed facts.

As long as the training of the naturalist enables him to trace the action only of particular material systems without giving him the power of dealing with the general properties of all such systems, he must proceed by the method so often described in histories of science—he must imagine model after model of hypothetical apparatus till he finds one which will do the required work. If this apparatus should afterwards be found capable of accounting for many of the known phenomena, and not demonstrably inconsistent with any of them, he is strongly tempted to conclude that his hypothesis is a fact, at least until an equally good rival hypothesis has been invented. Thus Rankine,[1] long after an explanation of the properties of gases had been founded on the theory of the collisions of molecules, published what he supposed to be a proof that the phenomena of heat were invariably due to steady closed streams of continuous fluid matter.

The scientific career of Rankine was marked by the gradual development of a singular power of bringing the most difficult investigations within the range of elementary methods. In his earlier papers, indeed, he appears as if battling with chaos, as he swims, or sinks, or wades, or creeps, or flies,

> "And through the palpable obscure finds out
> His uncouth way;"

but he soon begins to pave a broad and beaten way over the dark abyss, and his latest writings show such a power of bridging over the difficulties of science, that his premature death must have been almost as great a loss to the diffusion of science as it was to its advancement.

The chapter on thermodynamics in his book on the steam-engine was the first published treatise on the subject, and is the only expression of his views addressed directly to students.

In this book he has disencumbered himself to a great extent of the hypothesis of molecular vortices, and builds principally on observed facts, though he, in common with Clausius, makes several assumptions, some expressed as axioms, others implied in definitions, which seem to us anything but self-evident. As an example of Rankine's best style we may take the following definition :—

"A PERFECT GAS is a substance in such a condition that the total pressure exerted by any number of portions of it, at a given temperature, against the sides of a vessel in which they are inclosed, is the sum of the pressures which each portion would exert if inclosed in the vessel separately at the same temperature."

Here we can form a distinct conception of every clause of the definition, but when we come to Rankine's Second Law of Thermodynamics we find that though, as to literary form, it seems cast in the same mould, its actual meaning is inscrutable.

"*The Second Law of Thermodynamics.*—If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

We find it difficult enough, even in 1878, to attach any distinct meaning to the total actual heat of a body, and still more to conceive this heat divided into equal parts, and to study the action of each of these parts, but as if our powers of deglutition were not yet sufficiently strained, Rankine follows this up with another statement of the same law, in which we have to assert our intuitive belief that—

"If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

The student who thinks that he can form any idea of the meaning of this sentence is quite capable of explaining on thermodynamical principles what Mr. Tennyson says of the great Duke :—

> "Whose eighty winters freeze with one rebuke
> All great self-seekers trampling on the right."

Prof. Clausius does not ask us to believe quite so much about the heat in hot bodies. In his first memoir, indeed, he boldly dismisses one supposed variety of heat from the science. Latent heat, he tells us, "is not only, as its name imports, hidden from our perceptions, but has actually no existence;" "it has been converted into work."

But though Clausius thus gets rid of all the heat which, after entering a body, is expended in doing work, either exterior or interior, he allows a certain quantity to remain in the body as heat, and this remnant of what should have been utterly destroyed lives on in a sort of smouldering existence, breaking out now and then with just enough vigour to mar the scientific coherence of what might have been a well compacted system of thermodynamics.

Prof. Tait tells us :—

"The source of all this sort of speculation, which is as old as the time of Crawford and Irvine—and which was countenanced to a certain extent even by Rankine—is the assumption that bodies must contain a certain quantity of actual, or thermometric, heat. We are quite ignorant of the condition of energy in bodies generally. We know how much goes in, and how much comes out, and we know whether at entrance or exit it is in the form of heat or of work. But that is all."

If we define thermodynamics, as I think we may now do, as the investigation of the dynamical and thermal properties of bodies, deduced entirely from what are called the First and Second laws of Thermodynamics, without any hypotheses as to the molecular constitution of bodies, all speculations as to how much of the energy in a body is in the form of heat are quite out of place.

Prof. Tait, however, does not seem to have noticed that Prof. Clausius, in a footnote to his sixth memoir,[1] tells us what he means by the heat in a body. In the middle of a sentence we read :—

". . . . the heat actually present in a unit weight of the substance in question—in other words, the *vis viva* of its molecular motions" . . . .

Thus the doctrine that heat consists of the *vis viva* o

[1] On the Second Law of Thermodynamics. *Phil. Mag.* Oct. 1865, § 12, p. 244; but in his paper on the Thermal Energy of Molecular Vortices, *Trans.* R S. Edin., xxv. p. 557 [1869] he admits that the explanation of gaseous pressure by the impacts of molecules has been proved to be possible.

[1] Hirst's translation, p. 230, German edition, 1864, p. 258, "wirklich vorhandene Wärme, d.h. die lebendige Kraft seiner Molecularbewegungen."

molecular motions, and that it does not include the potential energy of molecular configuration—the most important doctrine, if true, in molecular science—is introduced in a footnote under cover of the unpretending German abbreviation "d.h."

J. CLERK MAXWELL

(*To be continued.*)

## WOLF'S HISTORY OF ASTRONOMY

*Geschichte der Astronomie.* Von Rudolf Wolf. (München: R. Oldenbourg, 1877.)

THE "History of Astronomy," by Prof. Rudolf Wolf, of Zurich, a volume of 800 pages issued at a very moderate figure, is a contribution to the literature of the science of no ordinary value to the student. The production of such a work, involving an outline of the progress of astronomy from the earliest times to the present period, must have been a labour of great extent, requiring much research, notwithstanding the assistance that might be afforded by historical treatises previously in the hands of astronomers, and it is only due to Prof. Wolf to acknowledge the very able and complete manner in which he has accomplished the heavy task he had imposed upon himself some years since.

Those of our readers who may have been desirous of acquainting themselves with the general history of practical astronomy, and of familiarising themselves with the names and the nature of the services of the principal workers who have successively contributed to advance our knowledge of the science, more especially during the last three centuries, will, we think, have experienced difficulties which the volume before us is well calculated to obviate. The English reader has, it is true, Prof. Grant's classical work, the "History of Physical Astronomy," but there is much to be found in this volume, which it was hardly within the scope of Prof. Grant's work to incorporate. The writer of these lines very well remembers the fragmentary manner in which, some thirty-five years since, an English student of practical astronomy was under the necessity of obtaining information, more especially in private reading; and it is one of the most happy circumstances for the astronomical student of the present day that this want of suitable guides has been to a great extent removed, and his time therefore need not be wasted in a search for knowledge in second-rate or doubtful authorities, mistakes which he would be not infrequently led into thereby, being corrected only after vexatious delay and trouble.

Prof. Wolf divides his work into three books. The first deals with ancient astronomy and progress down to the fifteenth century, including theories, instruments, and writings. The second commences with "the reformation of astronomy" consequent on the publication of the great work of Copernicus, "De Revolutionibus Orbium Cœlestium," and treats of the advances made to the time of Newton; we find therefore in this division a summary of the labours of Galileo, Apian, Tycho Brahe, Kepler, Fabricius, Harriot, Hevelius, Huyghens, Gascoigne, and many others, including notices of the more important publications of the period, which are of interest and value. The third book treats of "the new astronomy," commencing with the discovery of universal gravitation and brings down the history of astronomical research and discovery to the present epoch. A very great amount of information is compressed into this last section of the work, and it is here that the care and research of the author are more particularly evidenced. There is much to be found in it, for which we should look in vain in a collective and compendious form elsewhere. It is well and accurately put together, the few errors we have remarked being of comparatively trifling nature; thus the Saturnian satellite *Tethys* appears as *Thetis*. The biographical notes, which are extended to contemporary astronomers, will be a welcome feature to many readers.

Students and others interested in the history of the most ancient of the sciences, who can command a sufficient knowledge of the German language, will find their advantage in the possession of Prof. Wolf's elaborate work, and we must not omit to say that that great desideratum in all works of the kind—a very sufficient index, at least as regards names mentioned in the history, will render it of easy reference.

J. R. HIND

(*To be continued.*)

## OUR BOOK SHELF

*Photographic Spectra.* 136 *Photographs of Metallic, Gaseous, and other Spectra printed by the Permanent Autotype Process.* With Introduction, Description, &c., by J. R. Capron, F.R.A.S. (E. and F. N. Spon.)

WE gather from the author's introduction that he has chiefly aimed "to popularise a subject hitherto somewhat of a sealed book, confined to the laboratories of workers in special research." In this he should certainly succeed, though we think that his readers would not have been driven away if they had found a little more reference to the explanations of the various phenomena and the conclusions which have been drawn from them. As it is, the book is a good companion to Lecoq de Boisbaudran's "Spectres Lumineux." The spectra are sharp and clear, and the autotype process has lent itself well to this reproduction. The results are all the more commendable because Mr. Capron has not had the advantages of considerable dispersion.

The account of the method employed is full and clear, and will make the book a very useful one to beginners in spectrography.

## LETTERS TO THE EDITOR

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*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.*]

### Sun-spots and Terrestrial Magnetism

PROF. PIAZZI SMYTH will no doubt welcome from any quarter a satisfactory answer to his question about the discrepancy between Dr. Wolf's sun-spot period, 11·1 years, and the supposed 10·5 years' period for the magnetic needle. If Mr. Smyth will refer to Prof. Loomis's chart of magnetic oscillations given in Prof. Balfour Stewart's paper on the subject in NATURE (vol. xvi. p. 10), he will see that there are exactly seven minimum-periods from 1787 to 1871, the mean of which is twelve years; the mean of the seven corresponding maximum-periods is 11·8 years. The true magnetic declination-period is then the mean of these, viz., 11·9 years. In exactly the same manner I have found that the mean period of sun-spots is 11·9 years. The auroral displays also have the same period.

But what is this period of 11·9 years? It is Jupiter's anomal-