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## MACQUORN RANKINE'S SCIENTIFIC PAPERS

*Miscellaneous Scientific Papers by W. J. Macquorn Rankine, C.E., LL.D., F.R.S., late Regius Professor of Civil Engineering and Mechanics in the University of Glasgow.* From the Transactions and Proceedings of the Royal and other Scientific and Philosophical Societies and the Scientific Journals. With a Memoir of the Author by P. G. Tait, M.A., Professor of Natural Philosophy in the University of Edinburgh. Edited by W. J. Millar, C.E., Secretary to the Institute of Shipbuilders in Scotland. With Portrait, Plates, and Diagrams. (London: Charles Griffin and Co., 1881.)

THE volume before us contains thirty-seven papers of rare scientific interest written by the late Prof. Rankine, who died now eight years ago. As to the cause of this long interval the Editor gives us no hint, nor is there anything in the volume to explain it. All the papers are reprints, without note or comment, except such as is contained in the concise but extremely graceful Memoir. These papers are not by any means all Rankine's original works. They are principally those relating to Thermodynamics and Hydrodynamics. There are however two important papers on the latter subject which are not contained in the volume ("On Stream Lines," *Philosophical Magazine*, 1865; "On the Mathetical Theory of Stream Lines," *Phil. Trans. Royal Society*, 1871). These can hardly have been omitted by design, as in the very last paper contained in the volume the author resumes the subject, directing attention to his paper of 1865, while the paper of 1871 is the most general and important paper Rankine wrote on this subject, besides being his last work.

The first twenty-seven papers contain the development by Rankine of that most modern of mathematical sciences, Thermodynamics, from its foundation-stone to the complete edifice as it exists at the present day. This by no means constitutes Rankine's entire work, nor do we think it his most useful work. But it is the largest gem in the casket, and should he be forgotten in all the rest this alone will secure for him a foremost place amongst those who have left their mark on philosophy.

The rapidity of the development of this branch of science is unrivalled. As profound as anything ever brought to light by the power of reason, it only occupied Rankine four years from the publication of his first paper until the theory was completed and applied to all cases. That the burning of coal was necessary to the production of steam, which was necessary for the working of an engine, and that the proportion of coal burnt bore some relation to the work done, were facts which for 200 years had been forcing themselves into notice, and gradually there had come to be an idea that in some way heat was the same thing as other forms of mechanical energy. But this was all, till, in 1843, Dr. Joule published his first experimental determination of the mechanical equivalent of heat. Published in an obscure way, it was some years before this novel but definite relation between heat and energy excited notice. The first published notice is by Thomson in 1849, although that Rankine had known it

for some time previously is shown by the first of these papers, published in July of that year. In December of the same year Rankine sent in the Papers III. and XIV.,<sup>1</sup> containing the elements and some applications of his theory, and in 1854 he had published the complete theory and its applications to various engines, making instant use of the splendid experimental results just then obtained by Regnault. From this time it has been as possible definitely to forecast the result to be expected from any kind of engine as from 1690 to predict the behaviour of the moon.

From a philosophical point of view, there was a keen race in discovery between Thomson, Rankine, and Clausius, a race in which Thomson had the start, but which was neck and neck between Rankine and Clausius. But from the practical point of view Rankine was alone. And in this respect these papers, as indeed all his others, have a value both intrinsic and as examples of method which even transcends their philosophical value.

It was Rankine's practical knowledge which gave him his great advantage, but he had in some respects an advantage in having based his theory by means of an hypothesis on the fundamental laws of motion. Rankine worked from an hypothesis of his own creation as to the molecular constitution of matter, which was perfectly definite and capable of including all the phenomena which he had to consider. The definiteness of his hypotheses gave that definite form to his formula which suggested many points otherwise overlooked.

But as often happens, the definiteness of his hypotheses was also his source of weakness; he assumed the atoms of matter to be masses of fluid subject to eddies and vibrations, but otherwise at rest. This suited the conditions of his problem, but it was only an hypothesis, and as it was definite, so any phenomenon with which it was incompatible sufficed to disprove the hypothesis and bring down the edifice raised upon it. And such phenomena, those of diffusion, existed; although they did not come within the range of his work.

Rankine was himself fully alive to his position, and having once obtained his ideas and framed his formulæ, took and acknowledged a hint from his contemporaries, Thomson and Clausius; and having shown that Carnot's theorem, which they had modified and made the basis of their reasoning, was a consequence of his molecular vortices, he adopted a general law as the base of his reasoning, and cut himself off from his hypotheses. This was easy for him to do, for, as may be seen in § 15a of Paper III., he had with no small care framed his hypotheses so as to fit the same law, though expressed in other words. This article is also interesting as showing the unlimited faith he must have reposed in the design and care of Providence. Not only does he conceive each atom of matter to possess a fluid atmosphere, in which exist a number of similar cyclones or eddies, symmetrically placed all over the atom, but he required that wherever two atoms touched there two eddies should face, and so exactly as to be coaxial. Many complicated properties were attributed by Newton and others to the corpuscles of light, but such a demand as is here implied on the attention of Providence has probably never been equalled

<sup>1</sup> Why this paper is placed so far out of its chronological order does not appear.

—a whole crew of Maxwell's demons on each atom would be required to warp and moor for every movement that might occur. But so true was Rankine's knowledge of mechanics that all this elaborate refinement did not prevent his hypothesis leading him to correct results.

This refined organisation, however, which renders his hypothesis in the highest degree improbable, suggests a most important consideration. For the almost infinite complexity of his particular arrangement indicates almost to the extent of a proof that the results he obtained must depend upon circumstances so general as to be independent of any particular hypothesis, so long as it is in conformity with the laws of motion, and hence the trail of these general circumstances is crossed.

In Rankine's hypothesis the temperature comes out as a direct measure in any particular substance of the kinetic or actual energy of the molecular motion. This conclusion, to which he adhered in the final foundation of his theory, is general, but it does not appear to be the most general conclusion of which our present experiments admit. It led Rankine to give a definite form as well as name to his thermodynamic function, which forms the fundamental equation of all the mathematical work. But it was subsequently shown that the differential equation to the same lines could be obtained without the assumption with regard to temperature, and then it did not appear that there was sufficient experimental data for the complete determination of the constants which enter into the integral. This is owing to the hitherto impossibility of determining the exact form of the adiabatic curve for solids and liquids. With gases it is different, and with these Rankine's law is found to fit, but so might a law framed on the supposition that in other cases the kinetic energy was some other function of the temperature. What is proved therefore is not that the temperature is a direct measure of the kinetic energy, but that this is some function of the temperature. This is apparently all that has yet been accomplished, so that Rankine's definite conclusion must be looked upon as suggested rather than proved by experiment. There can be no doubt however that this definiteness led to a vast development of the subject, and hence it was no mere fancy or partiality for his own view which led him to adhere to that form of second law which included his earlier view. Nor will the study of Rankine's earlier papers be time wasted on the part of those who seek to understand this extremely difficult subject. They will there find a model of the machinery by which the general result might be obtained, and if, as is the case with most new inventions, the machinery is unnecessarily complex, it is still the only machine which has accomplished the results.

They will also find, what must for ever add an interest to these papers, the first use of the terms thermodynamic function, adiabatic curve, potential energy, and others now in general use; for Rankine's nomenclature, to a great extent his notation, and entirely his graphic method, have been universally adopted.

Rankine's methods have been called "uncouth," "diffuse and obscure," and without doubt they must seem all this to those who come to the subject with all the latest inventions in the form of mathematical machine tools in perfect working order,—just as the axe or adze must seem barbarous when there is a planing machine at

hand to do the work, and the material has been prepared for it. But let the shape required be of a novel kind, or let the material be in the rough, and then how does it fare with the planing machine?

Like that of Green, the whole career of Rankine is one rebuke to those who would exhaust the finest material on this earth—the best brain of our youth—converting it into elaborate mechanism only adapted to reduce, in however elegant a manner, already prepared billets to elegant and improved copies of masterpieces which, having once been shaped, although roughly with primitive tools, can never have to be shaped again. The material at last existed for a great mathematical edifice, of which the want had long been felt, and our great mathematical workshop was crowded with the most refined mechanism rusting for want of material to work upon. But this material was in the rough, and while waiting for some one to strip off the bark the chance was lost, for the obscure, self-taught mechanic who set to work with axe and adze did not stop at the bark, but with rapid and well-directed strokes brought out the form divine. However uncouth Rankine's methods may be, they have the great merit that they require nothing but a bold front—the result being obtained without adventitious aid. They are inscrutable to those who, having learnt the relations between quantities as expressed by symbols, have forgotten if they ever knew the purpose of their formulæ. But to the reader who thinks Rankine's methods are a statement of his thoughts, and though often a rough task, any one who succeeds in understanding Rankine finds to his satisfaction that he has done more than this, that he understands what Rankine understood.

Nor is this true only of his great work. What seems to us his most useful work is that of showing how the elementary mathematical methods were sufficiently adaptable to be applied to almost all cases of practical mechanics. The results are only approximate; but where neither the data nor the desired result can be exactly measured, this is all that could be obtained, were the methods never so exact. One might as well set bricks by Sir Joseph Whitworth's millionth-of-an-inch machine as use the exact equation of thermodynamics to determine the probable work to be obtained from a steam-engine.

The graphic method was Rankine's great weapon. This, which is probably as old as any mathematical method, had been long neglected, except that it was sometimes used for engineering purposes. Rankine early perceived its applicability to the subjects he had to teach, and in his treatises on Applied Mechanics, Shipbuilding, and the Steam-Engine there are many instances of its novel and useful application which have been copied far and wide, while his graphic treatment of the subject of thermodynamics has been universally adopted. But the height of his achievement in the application and development of the graphic method is only reached in his papers on the motion of fluids.

These papers, with the omission already noticed, are collected at the end of the volume, and they constitute by no means its least valuable part. They are comparatively his later work. The first, "On the Exact Form of Waves at the Surface of Deep Water," was published in 1862, after his thermodynamical work was essentially complete.



Both the method and matter of this paper are unique. The results are obtained by a simple geometrical study of rolling circles. And there for the first time definite reasoning is adapted to the actual proportions of deep-sea waves, all previous work on the subject having been based on the assumption that the height of the wave is small compared with its length.

It is however in the next paper that he first shows what may be done by Maxwell's method of the graphic use of families of surfaces or curves. Here we have what is invisible in the fluid itself and had only been expressed by complex algebraical formulæ—the internal motion of the fluid—shown in such a way that not only the direction but the magnitude of the motion at every point may be taken in at a glance as well as definitely measured, and all deduced by simple but rigorous geometrical methods. The credit of this, which is certainly one of the highest achievements in the art of expression, must be divided. It was Faraday who first conceived the force of a magnet expressed by a family of lines; and it was Maxwell who discovered the rigorous method of drawing Faraday's lines; while Rankine realised in this the means of applying and expressing the principles of the steady flow of fluids propounded by Stokes now forty years ago.

In these papers on Hydrodynamics, as in all his other work, Rankine had a practical purpose in view. In this case it was the skin resistance and wave resistance of ships. And if, owing to the neglect of friction in the fundamental equations of motion, some of the results are still doubtful, yet in this respect the work is on a par with all the rest that has been done on this subject. And these papers, owing to the clear conception they convey of the internal motions of fluid and the direct purpose of the means adopted to elucidate these, afford by far the best chance for any one wishing to pursue the subject up to the highest position it has at present attained.

That Rankine himself owed much to having early directed his thoughts to fluid motion appears in all his work, as well as being shown by his theory of molecular vortices—a strictly hydrodynamical conception—amongst the intricacies of which nothing but his exact knowledge of the subject could have kept him straight.

It must be remembered however by those who would make a like use of such knowledge that Rankine did not begin his career by the study of mathematics; but that as an engineer from his birth, as we are told in the Memoir, he first became aware of the circumstances and problems of mechanics, and only evolved or acquired his mathematics as he found them necessary to his work. In this way his knowledge of mathematics must have included the knowledge of the necessity for each step. It was necessity first, and then method or invention; and not, as is too often the case with those who begin to learn mathematics before they are aware of what it is they are to do, all means and no ends.

In Rankine's text-books, as in his original papers, the ends are always kept in view. It is often impossible for others to follow him unless they begin by actually mastering the circumstances of the problem and trying to solve it for themselves, then if they honestly fail they will find that Rankine will help them; while if they succeed they will find that Rankine was before them. These books, both as regards originality of matter and the attention

paid to the circumstances of each problem, have more the character of original papers than orthodox text-books. From this as well as his other writings it is clear that he acquired his knowledge of mathematics from the original works of the master, and not from text-books.

His example should therefore be the best recommendation for all those who would really understand mechanics to read the works direct from the hand of this master—a task which, with the aid of this volume, they may now accomplish without that trouble of search which, small as it is, leaves many a masterpiece on the shelf in some dark corner, while a mutilated and garbled extract disgusts the reader and discredits the thinker.

OSBORNE REYNOLDS

#### THE FERNS OF NORTH AMERICA

*The Ferns of North America; Coloured Figures and Descriptions, with Synonymy and Geographical Distribution, of the Ferns of the United States of North America and British North American Possessions.* By D. C. Eaton, Professor of Botany in Yale College. The Drawings by J. H. Emerton and C. E. Faxon. 2 Vols. quarto, pp. 352 and 285; 81 Plates. (Boston: S. E. Cassino, 1880.)

THIS handsome work, which has been brought out in parts, issued about one every two months, beginning with 1878, is now completed. Although ferns have long been popular in the United States, both with collectors and cultivators, this is the first large illustrated monograph of the indigenous species which has been attempted. For our own country we have several, of which the best known are Hooker's "British Ferns," with coloured figures, in large octavo; Lindley and Moore's "Nature Printed Ferns," in more than one edition; and Newman's "British Ferns," in which the plates are uncoloured woodcuts; but of the American ferns there are but few figures, and those widely scattered in general works, and even leaving figures out of the question there has been no descriptive handbook specially devoted to them, so that those who wanted to work at the subject have been placed at a great disadvantage. Prof. Eaton, who is the grandson of a well-known botanical author, has been universally recognised for the last twenty years as the leading authority on the subject. He has a large library and general collection of his own, has visited Europe and studied the American ferns in the public herbaria of the Old World, has proved himself in other departments of botany to be a careful and judicious systematist, and he is a teacher of botany of many years' experience, and has been looked up to for a long time by all the collectors of ferns throughout the Union as their referee in cases of doubt and difficulty; so that he has had every advantage for dealing with his subject in a thorough and exhaustive manner, and as he has been ably seconded by his two artists, the result is a monograph which is thoroughly satisfactory in every way, and which will be universally accepted both at home and in Europe as a standard work.

The geographical area which it covers is the whole of the American continent, from the Pole to the southern boundary of the United States. The true ferns only are included, not the Lycopodiaceæ, Equisetaceæ, and Rhizocarps, which are monographed along with the ferns by