

EDWARD JOHN ROUTH,* 1831—1907.

By the death of Dr. Routh on June 7, after a period of gradually failing health, a commanding figure in the recent history of English mathematics has been removed. Born at Quebec in 1831, the son of a distinguished British officer, he was educated in London at University College School, and subsequently studied mathematics under de Morgan at University College. He matriculated at Peterhouse in 1850, but did not drop his London connection, obtaining the gold medal in mathematics with the degree of Master of Arts in 1853, then a somewhat rare distinction. At Peterhouse he had Clerk Maxwell, who soon after migrated to Trinity, as his rival in the same year; while Tait and Steele were undergraduates of the College, and Lord Kelvin (already Prof. W. Thomson, of Glasgow) was a junior Fellow.

Not long after taking his degree—in January, 1854, being Senior Wrangler, and bracketed with Clerk Maxwell for the Smith's prizes—he began the career of tuition of advanced honour men in mathematics, which was soon to lead to a unique reputation as a successful teacher. From 1858 to 1888 he had, in all, between 600 and 650 pupils, of whom the great majority graduated as Wranglers, twenty-seven being Seniors, while forty-one were Smith's prizemen; between 1861 and 1885, when he retired from this strenuous work at the age of 54, he had all the Senior Wranglers as pupils, with but one exception near the end of the time.† The number of his pupils, which was for many years about 100, was not at all unprecedented; what was unique was the fact that for all this time he directed, almost without challenge, most of the intellectual activity of the *élite* of the undergraduate mathematical side of the University. This herculean task naturally demanded methodical arrangements, and the husbanding of his resources to the utmost. What he aimed at was to impart thorough mastery of the main principles of ascertained knowledge over the field of mathematics then cultivated at Cambridge; it was clearly out of the question to stray very far into the regions of nascent science, in which ordered theory gradually evolves itself in response to concentrated and specialised effort. He was in the habit of claiming that this would follow spontaneously in the case of the mathematician born, once he had learnt mastery of the resources of the science, while even when it did not follow, the record in the legal and other professions of persons who had done well in youth in mathematical studies proved their supreme value as a deductive mental discipline.

His plan was to take small classes, each of about ten men selected to run together, and to maintain an average by catechetical methods. Those

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† These and other facts have been taken from a valuable notice in the 'Cambridge Review,' signed W. W. R. B.

who could go faster than the average had extra material provided in the form of manuscript digests for study, and especially in the institution of a weekly paper of about a dozen problems, selected from recent examination papers, or abstracted from memoirs in the home and foreign mathematical journals. An element of competition formed a stimulus in answering these papers, while written solutions were afterwards at hand for study in cases of failure to unravel them. Looking back on those times, it might be thought that there was too much problem and too little sustained theory; but no one ever accused the standard of the problems selected of being lower than it ought to be, while, on the other hand, absence of some such rigid procedure would have rendered quite impossible that focussing of undergraduate mathematical activity and ambition in one place, which was a main feature of the system. Men with further ambitions would struggle with Thomson and Tait's "Natural Philosophy" or with Maxwell's "Electricity," or with brilliant and stimulating courses of lectures given on growing special subjects by the more eminent mathematical physicists, and thus learn that though in youth mastery may be rapid, yet at all times invention must be slow. It was, moreover, thus possible for the abler men to have time to spare, to expand their outlook by taking up some other branch of knowledge as a relaxation from mathematics, or for joining in other activities of the University. Nowadays the field covered by the mathematical instruction offered at Cambridge is vastly wider than would have been conceived as practicable twenty years ago; but the question is still unsettled how far it is expedient to extend the preliminary undergraduate course into complex special theories.

Whatever may be thought as regards Dr. Routh's views on postponing special research in favour of thorough preparation, it could not be urged that he did not himself, notwithstanding his other absorbing work, set an example of what research might be. Many of his earlier papers, mainly in the 'Quarterly Journal of Mathematics,' related to the dynamics of rigid solids, spinning tops, rolling globes, precession and nutation, and such like; they were distinguished by the development of methods relating to moving systems of co-ordinate axes, and to the differentiation of vectors such as velocity and momentum with regard to them. In another connection he applied the kinematics of special systems of co-ordinate axes, moving along a curve, to problems of curvature and torsion. The advantages of these methods in differential geometry have come again into recognition, as may be seen in such works as Darboux's "Théorie des Surfaces." Afterwards, arising out of his researches on dynamical stability, which will be referred to presently in more detail, there came a series of papers in the 'Proceedings of the London Mathematical Society,' on the propagation of waves, and the analysis of complex vibrations in networks of interlacing threads, and in other such laminar systems, leading up to a mechanical treatment or illustration of the broad general theory of harmonic analysis, principal periods, and related topics.

In the early 'seventies, the question of the possible explanation of steady, including apparently statical, relations of material systems by the existence of latent steady motions, such as the rotations of concealed fly-wheels or gyrostats attached to the system, was much to the fore. The fundamental problem as regards such representations is their degree of permanence; for a state of motion which falls away, however slowly, cannot be appealed to in elucidation of secular steadiness of relations. At a later stage the ideas of the subject were crystallised by Lord Kelvin in his British Association address, Montreal, 1884, entitled "Steps towards a Kinetic Theory of Matter," and in later addresses on cognate topics, mainly reprinted in vol. i. (Constitution of Matter) of his "Popular Lectures and Addresses," culminating in a way in 1897 in his gyrostatic model of a rotationally elastic optical aether.

It is thus not surprising that the Adams prize subject at Cambridge for the period 1875-7, announced over the signatures of Challis, Clerk Maxwell, and Stokes, should have been the search for "The Criterion of Dynamical Stability." This subject suited Routh's predilections exactly; and his classical essay, "A Treatise on the Stability of a Given State of Motion, particularly Steady Motion," composed, as he states in the preface, almost entirely during the year 1876, was the result. The greater part of the work in the essay is analytical, and is concerned with the discussion of the nature of the roots of the algebraic equation determining the free periods of slight vibration of the dynamical system; but where it enters upon the discussion of dynamical principles, such as the criteria connected with the Energy and the Action, the essay moves in a high plane. In particular, the burning question of how adequately to represent latent, and therefore unknown, steady motions, such as those of concealed fly-wheels or gyrostats attached to the system, is solved at a stroke by the famous theorem of the "modified Lagrangian function." It was established, in fact, that the presence of concealed steady motions does not fundamentally alter the standard mode of analytical specification of dynamical interaction developed originally by Lagrange, except in the one respect that the effective Lagrangian function now involves terms linear in the velocity-components as well as quadratic terms. The procedure of Lagrange, evolved originally from the side of the Principle of Action, constituted the science of general dynamics by eliminating from the problem all variables the values of which are prescribed in terms of the remaining ones by relations of permanent constraint, thus reducing the dynamical analysis to the discussion of just as many quantities as are required to specify the state of the system. It gives cause for some surprise that nearly a century elapsed before the correlative step was taken, namely, the elimination, from the analytical specification of the system, of permanently steady or cyclic motions, as well as the permanent geometrical constraints above mentioned. In the hands of the analysts who treated the subject meanwhile, the requirements of the actual planetary and lunar theories were perhaps the main aim; it is only recently, and largely

in the hands of the English school, notably Lord Kelvin and Clerk Maxwell, in later conjunction with Helmholtz, and building largely on the earlier work of W. Rowan Hamilton, that the subject of general dynamics has been welded into an instrument for the inductive, and in many cases speculative, exploration of physical processes in general. Anyhow, it will be evident how fundamental an advance in the principles of the dynamical interpretation of nature was involved in Routh's formulation of what he called the "modified Lagrangian function."

The problem thus solved by Routh with remarkable simplicity had already been some time in evidence. In the first edition of Thomson and Tait's "Natural Philosophy" in 1868, the equations of Lagrange had been applied in most effective manner to problems of motions of solids in fluid media, the energy function involved being determined in terms of the motions of the solids alone, and the fluid thus being *ignored* in the subsequent work. This procedure was soon challenged by Kirchhoff, as going beyond the existing conditions of validity of general dynamical theory; a special justification for the case of motion in fluids was given by him, on the basis of a Least Action analysis, and a brief statement of it was included by the author in the German translation of the treatise. Soon afterwards the same difficulty was pressed on Lord Kelvin independently by J. Purser, who also published a justification on more physical lines. This was, not unlikely, the origin of Lord Kelvin's general theory of "ignorance of co-ordinates," first published in 1879 in the second edition of Thomson and Tait's treatise, but which probably existed in manuscript anterior to Routh's essay. A report was once current that most of it was worked out in the harbour of Cherbourg, while his yacht was refitting, and the carpenters were all the time hammering overhead. This form of the theory, though more expressly suggested by the needs of physical dynamics, was less complete in one respect than Routh's, in that it did not bring the matter into direct relation with a single characteristic function (Lagrangian function of Routh, kinetic potential of Helmholtz), but simply obtained and illustrated the equations of motion that arose from the elimination of the cyclic co-ordinates that could be thus ignored.

Later still, Helmholtz, in his studies on monocyclic and polycyclic kinetic systems, which began in 1884, and culminated in the important memoir on the physical meaning of the Principle of Least Action in vol. c. (1886) of 'Crelle's Journal,' developed the same theory more in Routh's manner, and built round it an extensive discussion of physical phenomena, so that on the Continent the whole subject is usually coupled with his name. Shortly before, the work of Routh and Kelvin had already been co-ordinated with the Principle of Action by more than one writer in England.

The most elaborate published result of Dr. Routh's scientific activity was the "Treatise on the Dynamics of a System of Rigid Bodies," which began as a thorough, though rather difficult, handbook in one octavo volume, but expanded in successive editions in a manner of which other classical instances

readily occur to mind, until it became a sort of cyclopædia of the dynamical section of theoretical physics. In the course of an inquiry some ten years ago as to the reason why English mathematical physicists had so much practical command over the application of their knowledge, the mode of teaching in Cambridge came under review; and in particular this book was discovered by Prof. F. Klein, of Göttingen, who made arrangements for its introduction to the Continental public in a German translation, containing some brief valuable annotations such as the wide analytical outlook at Göttingen suggested. Especially was emphasis given to the great extension of the scope of abstract dynamics above described, with which Routh's name was associated, it is to be hoped permanently. Somehow the book does not seem to have attracted even yet much sustained attention in France.

Until lately, Dr. Routh's presence was a familiar and welcome one to residents in Cambridge. Though he never sought public positions, his services were in requisition in many ways, as Senator and Fellow of the University of London, as member of the University Council at Cambridge, member of Council of the Royal Society, and in other activities; while he declined more prominent offices more than once. In society he was bright and attractive, though somewhat retiring, simple, and entirely free from any suggestion of superiority. The respect and affection which he inspired in a long succession of distinguished pupils found expression on the occasion of his partial withdrawal from work in 1888, when at a remarkable gathering of judges, engineers, and men of science, his portrait by Herkomer was presented to Mrs. Routh, with many expressions of warm appreciation. His leisure he employed mainly in mathematical research, and in the preparation of a series of treatises on subjects of mathematical physics, of which the only criticism to be made is that his wealth of valuable material tended to convert them into cyclopædias rather than text-books. His last public action was to take the lead in opposition to the proposals for change in the system of the Mathematical Tripos at Cambridge. It is possible that he did not fully realise the altered circumstances of the time, and the insistent claims of other studies; anyhow, it will be matter for congratulation if the new arrangements work as well and as smoothly as did the older Mathematical Tripos during the long period when the practical direction was mainly in his hands.

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