# MATHEMATICAL RESEARCH

(*Presidential Address.*)

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MR. PRESIDENT, LADIES AND GENTLEMEN :—

We are met together at a serious time. We are all so much moved by the terrible suffering and misery which the War causes, so distressed by the woeful destruction of ancient cities and historic monuments by which it has been marked, so shocked by the failure of civilised opinion to restrain the predatory ambitions of the leaders of a nation supposed to be friendly, that we find it difficult to concentrate our attention on ordinary affairs, and may even have entertained doubts as to the appropriateness of holding our Annual General Meeting. But I think that we may be reassured on this point. While, at the present crisis, it is the simple duty of every man of suitable age and sound physique to place his services at the disposal of the military authorities, it is equally the duty of other citizens to meet whatever happens with courage and calmness; and the best means to this end is to maintain the normal course of civil life with as little disturbance as possible. For this reason scientific societies, as distinguished from their younger and more robust members, should, as far as may be, act in the same way as in time of peace. Among scientific societies, mathematical societies occupy a special position owing to the nature of the trust confided to them. Such a society is a trustee for a treasure which has accumulated through the ages, a treasure which war cannot destroy and even barbarism can but temporarily dim, a treasure which has been described by my predecessor in the Chair as "the main heritage of man, his little beacon of light amidst the solitudes and the darknesses of infinite space." That heritage is mathematics. The process by which it has been won is research. To maintain and improve it is the purpose for which our Society exists. I would therefore crave your indulgence while I set before you some considerations bearing upon various aspects of mathematical research.

The chief matter which I propose to discuss is a question which must be especially interesting to the Council of the Society. It is this: What is it that constitutes value or importance in mathematical research? To put it another way: Wherein lies the difference between valuable research and laborious trifling? Or, again, it may be asked: How is valuable research to be distinguished from the construction of examination questions or from mathematical recreations? The distinction is recognized to exist. It is recognized that some kinds of work are better worth doing than others. We are told, for example, concerning Ritz that he had a very true sense of the relative importance of problems. But I do not remember to have come across any general discussion of the question. It is easy, however, to note certain qualities by which valuable research work is characterized.

One of these qualities is novelty. Prof. Baker, in his Address to Section A of the British Association at Birmingham, has emphasized the aspect of mathematics as a creative art. In any valuable research this element of creation or novelty cannot be absent. The new thing, or the created thing, may be a new idea, or a new method, or a new result, or a new proof of a known result. Mathematics is primarily an affair of ideas much more than of formulæ or calculations or technique. But it seems to be necessarily true that a new idea, to gain acceptance, must be developed into a method and fruitful of results, so that a difficulty may arise in judging of the value of a piece of work which purports to contain a new idea. As examples of new ideas I would mention the idea of incommensurables, which is said to have arisen from a contemplation of the solution of the problem of dividing a segment in extreme and mean ratio; the idea of the comparison of evanescent increments, virtually present in Napier's invention of logarithms, and subsequently developed into the Differential Calculus; the idea of a group, first noted by Ruffini in connexion wlth the substitutions which leave a rational function unchanged, and afterwards so much developed, especially by Galois in algebra and Lie in geometry. These examples suggest that new ideas in mathematics grow from small beginnings, and that a moment comes when some man of genius seizes their essential import and makes them part of our intellectual heritage. Many examples might be given of valuable researches which have been regarded by their authors as introducing or establishing new methods. It may suffice to recall the fact that Leibniz' invention of the Differential Calculus was published in a short paper of less than seven pages as a method for finding maxima and minima and the tangents of curves. It is unnecessary to give instances of writings which contain new results or new proofs of known results, though it may be permitted to refer to a curious example of a famous theorem for which a new proof had to be found because the original proof had disappeared: I mean Pascal's theorem of the inscribed hexagon. Pascal's investigations, which were never completely published, seem to have been founded upon the neglected work of Desargues on perspective, but the theorem is not contained in that work. Our proofs are due to much later writers, such as Brianchon, who was the first to develop systematically the theory of anharmonic ratio, although the fundamental result that such ratios are unaltered by projection was known to Desargues. In this fragment of history we have the emergence of a new mathematical idea—anharmonic ratio, the development of a new geometrical method founded on this idea, a new and striking result, a new and elegant proof. It seems that the progress of mathematics needs many kinds of work. A worker who introduces a new idea may be compared with the exploring prospector who discovers that there is gold in a country; one who invents a new method, with the mechanician who devises the processes and perfects the tools by which the gold can be extracted; one who obtain new results, with the miner who extracts the gold; one who obtains new proofs of known results, with the metallurgist who refines the gold and uses it for making beautiful objects. Few of us can hope to play the part of the prospector or the mechanician, but their efforts would be fruitless without the work of the miner and the metallurgist.

After this all-important character of novelty, or artistic creation, we may note other qualities which valuable research must possess. One of these, which is not very easy to define, I propose to call "relevancy." A piece of work, to be valuable, must be a branch of the tree of knowledge; it must stand in a proper relation to the state of mathematical knowledge existing at the time when it is produced. If it is isolated or has no such relation, it is irrelevant. A proposition may be new and true and difficult to prove and yet it may be irrelevant. Let me give an example. Prof. Hobson, in his little book on the *Squaring of the Circle*, has given us a series of most interesting surveys of the state of knowledge in regard to this problem existing at various periods. Anyone who should now spend time on developing new series for calculating approximate values for $\pi$, after the fashion of Gregory's series for the inverse tangent, or Newton's series for the inverse sine, would be doing work that might have been valuable in the seventeenth century but would be irrelevant now. This example is rather extreme, but the quality of relevancy or irrelevancy attaches in greater or less degree to all original work in mathematics.

Another quality which characterizes valuable original research may be named "definiteness." A piece of research work should aim at giving a definite answer to a definite question. For example, the most famous work of Galois aimed at answering the question: What algebraic equations can be solved by means of radicals? We observe in regard to this question that when asked it was supremely relevant. In the first half of the sixteenth century more than one Italian mathematician, Cardan being the best remembered, had found how to reduce the solution of the cubic equation to that of a quadratic equation, and Ferrari had found how to reduce the solution of a biquadratic equation to that of a cubic equation. At a later time, in the early part of the eighteenth century, Euler had found a different way of reducing the problem of solving the biquadratic equation to that of solving a cubic equation, and had entertained the idea that a similar reduction must be possible for an equation of any degree to one of the next lower degree. The development of this idea had been undertaken by Lagrange, with the result that the suggested process had been found to fail definitely for the quintic; and Abel had finally proved that the general quintic cannot be solved by means of radicals. The precision which had been given to the question by the work of Abel would have been unattainable in the time of Cardan, and thus Galois was able to propose a perfectly definite question. He also found a perfectly definite answer. In regard to this quality of definiteness it is of interest to bear in mind the influence that has been exerted upon the progress of mathematics by problems. We shall find that many valuable researches have arisen in the effort to obtain the solution of some definite problem. As an example, I would cite the Problem of Three Bodies, especially as illustrating the way in which a problem becomes transformed. In its first form it is the theory of the motion of the moon around the earth, as disturbed by the attraction of the sun, a problem which can be solved, with sufficient approximation for practical purposes, by various methods. In the more general form, in which it is the problem of $n$ bodies, it includes the problem of planetary theory, also solvable for practical purposes by various methods of successive approximation. But the theoretical interest of the mathematical problem remains undiminished after the practically useful approximations are completed; and it has given rise to numerous interesting questions in regard to the general differential equations of analytical dynamics and the theory of differential equations. We know that it has now been proved that the equations of the problem do not admit any other integral of the same type as the known integrals, which express the constancy of the energy, the linear momentum, and the moment of momentum; but the question of completing the integration remains unsolved. This question has perhaps not yet been asked in the definite form that it must receive before any advance can be made. It is sometimes true that a question properly asked is half answered.

We have noted three qualities as characteristic of valuable research in mathematics: novelty, relevancy, definiteness. I would add to this catalogue a fourth: generality. The best work is never parochial, it is never restricted to a narrow outlook. The quality of generality may seem to be opposed to the quality of definiteness, but generality must not be confused with vagueness. As an example of a piece of work which shows conspicuously the mark of generality, I would cite Gauss' famous memoir on the hypergeometric series. At the time when this was published it could be said of it that it included the theory of almost all the functions which up to that time had been investigated by analysts. But there is nothing indefinite or vague about Gauss' work. As another example of generality, I would cite the shifting of the theory of elliptic functions from the relatively narrow basis of the Jacobian theory, with its modulus, its three related functions, and its appalling array of quasi-trigonometrical relations, to the comparatively simple but much more comprehensive foundation afforded by the theory of the doubly periodic functions. These examples lead us to think that generality is the mark of good theories, while definiteness attaches rather to problems, but it must not be forgotten that theories have their roots in problems and bear their fruits in the solution of problems. Is there then such a thing as excessive generality? A story is told concerning a certain variety of roses which were in great demand among the makers of bouquets, not only on account of their beauty, but especially because the stalks were very long and stiff. The growers took steps to increase the length of the stalk, and were very successful, producing blooms with stalks as much as seven feet long. But unfortunately as the stalk lengthened the bloom dwindled, indeed most of the very long stalks bore no flowers at all. It may be treading on dangerous ground to suggest that there is such a thing as excessive generality, though even so convinced an analyst as Picard is not without misgivings on the subject. Yet it can sometimes be wished that writers who develop general theories at great length would pause to enquire how far they are available for the solution of special problems. Let me give an example. The theory of ordinary linear differential equations is very highly developed, but applications to particular equations are beset by a difficulty which the theory leaves untouched. I do not undervalue the interest or importance of the existing theory; everyone who has studied it must have been impressed by it. To proceed to the difficulty, let us suppose, for example, that we have before us a linear differential equation of the second order with rational algebraic coefficients and known singular points. We can form, and solve, the indicial equations relating to the finite singularities, and obtain sequence equations to determine the coefficients of the series which represent regular integrals in the corresponding neighbourhoods; but we cannot usually write down the coefficient of the $n$-th term of one of our series, unless it should happen that the sequence equation is a linear difference equation of the first order, or one of the second order of the type that can be solved by means of relations between hypergeometric series. When the sequence equation has not one of these characters, if it is an equation of the second order, an equation of three terms, we may obtain a practically sufficient solution by the use of continued fractions, as Laplace did in the theory of the tides; but if it is of an order higher than the second, say an equation of four terms, even this road is blocked. Now it seems to me that much, if not all, of the theory of linear differential equations is developed on the understanding that the coefficients of the series which represent the integrals in the neighbourhoods of the singularities have been effectively obtained, whereas it is only the sequence equations that are really obtained. This is not the occasion to do more than indicate the existence of this gap in the theory, but the example suggests that the value of a piece of research work is increased if the quality of definiteness is combined with the quality of generality, just as the quality of relevancy should be coordinated with that of novelty.

Let us turn for a few moments from this general discussion of the qualities of valuable research in mathematics to consider the distinctive features of research in mathematical physics, including, of course, such subjects as mechanics and hydrodynamics, as well as such subjects as electricity and thermodynamics. It would be taking too narrow a view to contend that mathematical physics and pure mathematics are identical, although there is a constant tendency for mathematical physics to be absorbed in pure mathematics. Thus Mr. Bertrand Russell claims dynamics as a branch of pure mathematics, and the German Encyclopædia of mathematics classifies the theory of potential under differential equations. On the other hand, it is known that geometry had originally an empirical foundation, and it may be suspected that the same is true even of arithmetic. We can trace the passage of geometry from the experimental to the abstract stage; the beginnings of arithmetic are hidden in the times before the dawn of history. We might then enter a counterclaim to the effect that all mathematics is mathematical physics. But it is well understood that this view also would be too narrow. Mathematics is not concerned with the process of passing from empirical data to an abstract theory. This is the primary concern of mathematical physics. Mathematics is not much occupied with the numerical tabulation of the results that can be obtained, not at all with the comparison of such tabulated results with the results of experimental measurements; but a piece of valuable research work in mathematical physics may be composed largely of such matter. Mathematics is not so much occupied with apparently isolated special problems as mathematical physics necessarily is. I say "apparently" isolated, because in valuable research work, however isolated a problem may appear, its solution is desired for the sake of the light which it is expected to throw upon some question of greater generality. In the attempt to solve such problems there arises in mathematical physics a kind of research work which would be trivial in pure mathematics—the kind of work in which we take some piece of pure mathematical theory and use it to obtain the solutions of the special physical problems which it is competent to solve. This kind of work has been humorously described in the phrase "Given the solution it is required to find the problem." Kirchhoff's solutions of problems in discontinuous fluid motion illustrate the process perfectly. Such work can be of great value, as indeed Kirchhoff's was, when it really helps us to understand some natural phenomena; it is ridiculous to do it for its own sake. I would say then that, in addition to the qualities of novelty, relevancy, definiteness, and generality, there is necessary to valuable research work in mathematical physics another quality which may be described as "realism" or adherence to fact. There is work to be done in showing how new observations fall under existing theories, how existing theories must be modified in order that new observations may fall under them, how previously existing, or newly proposed, theories may be tested by deducing analytically the results that follow from them, and comparing these results with laboratory experiments or general experience. Facts make their appearance at the beginning and also at the end. Even the argument by which a question is solved may have a physical interpretation, as happens, for example, when the notion of an "image" is introduced; and this is the ideal of mathematical physics—to conduct the analysis in such terms and by such processes that the argument may be couched in physical language with the minimum use of uninterpreted auxiliary quantities or relations devoid of physical significance. Often it happens, however, that a new result, obtained at first by a more abstract method, is afterwards proved by methods approaching more closely to this ideal. For example, Poisson solved the problem of induction, for a conducting sphere under the influence of a point charge, by means of harmonic analysis, years before electric images were thought of.

The quality of realism may attach to valuable research work in mathematical physics in very various degrees. A theory, or the solution of a problem, may conduce directly to the increase of material well-being, as Maxwell's theory of electrodynamics contained the germ of wireless telegraphy, or it may satisfy intellectual curiosity, as Darwin's theory of tidal friction throws light upon the past history of the earth and moon. There is a certain mental satisfaction in knowing that "Sodium is in the sun," although sodium in the earth can alone be used in chemical industries. The thirst for knowledge is not confined to the immediately practical. Such matters as the origin and past history of the solar system are legitimate objects of curiosity even if an advance of knowledge concerning them is not likely to lead to improvements in the art of navigation. An investigation may be of value even though it may have but a remote bearing upon the rational scheme under which a wide range of facts are, as Prof. Pearson would say, "resumed." Reference has already been made to the Problem of Three Bodies. Apart from approximate solutions, such as constitute the lunar theory, its bearing upon theoretical astronomy may be conceded to be remote. In regard to this problem we may observe that one of the numerous cosmogonies, which have been put forward without proof, makes a great point of the capture theory of comets. The sun by itself cannot capture a comet. Can the solar system capture a comet? Could even a simpler system, consisting of a central sun attended by a single planet, do it? The answer to this question would be found in a particular solution of the Problem of Three Bodies.

A rather difficult question sometimes arises as to the validity of approximations made by workers in mathematical physics. For example, the whole theory of elasticity is founded upon the assumption that quantities of an order higher than the first in the components of strain may be disregarded. The effect of such assumptions underlying a theory is to make the general equations by which the theory is expressed more tractable. Even when the approximation, which is involved in such a reduction of the general equations as is here referred to, has been made, the solution of the equations appropriate to some special problem may be impossible of attainment with existing analytical resources. This happens, for example, in the problem of calculating the distribution of stress in a masonry dam. In such cases it is necessary to simplify the problem by disregarding circumstances which may be assumed to be of little consequence. For instance, in the problem of the dam, the material is regarded as homogeneous, and the system is replaced by a two-dimensional one, while for the actual boundary there is substituted a more regular geometrical shape. Even when a problem has been simplified to the utmost, in such ways as this, it may remain defiant; and it may be necessary to have recourse to some numerical method of approximate solution. Much of the work of the mathematical physicist may take the form of devising, and applying, suitable methods of approximation. A trained physical instinct is the only sure guide to the selection of the circumstances to be disregarded. One of the most remarkable examples of the success of approximate methods specially devised with a view to a particular class of questions is to be found in Fresnel's theory of diffraction. Fresnel did not know the differential equations which govern the propagation of light *in vacuo;* his geometrical notions of the process of wave-transmission took their place. He did not know, nor do we as precisely as we could wish, how the presence of matter in its course would affect the passage of a wave; but he made an assumption which has been found by experience to be approximately true, the assumption that the light that comes to an aperture in obstructing matter is alone concerned in the construction of the effects observable beyond the aperture. He could not even sum exactly the series constructed by the aid of this assumption, but he divined the principle by which the important terms can be distinguished from the comparatively unimportant residuum. Similar problems arise in the theory of sound. Here the analytical conditions are more precisely known, while the physical interest is much smaller. Yet the problems remain, for the most part, unsolved. Lord Rayleigh remarks: "Although the general character of the phenomena is well understood, and therefore no very startling discoveries are to be expected, the exact theoretical solution of a few of the simpler problems, which the subject presents, would be interesting." The only exact theoretical solution we have is that, first obtained by Sommerfeld, for the diffraction of plane waves at a straight edge. There seems to have been a certain element of good fortune in the discovery of this solution, as efforts to extend the method to other problems have so far proved fruitless. The theory of diffraction is not the only department in which the exact theoretical solution of a few simple problems would be welcome, even though the general character of the phenomena is well understood; and we may note here that it may not be necessary to aim at any startling discoveries in physics in order to do valuable research work in mathematical physics.

An examination of the solitary known theoretically exact solution of the problem of diffraction prompts the remark that a mathematical physicist cannot know too much mathematics. And this remark brings me to the equipment of an investigator. Some essential elements of this equipment seem to be skill, knowledge, curiosity. In order to produce valuable research work, either in pure mathematics or in mathematical physics, the worker must be an accomplished mathematician. It is not enough to have native ability. It is necessary to be ready with the various artifices by which mathematical work is effected. More than this, it is necessary to have wide and deep knowledge of the mathematics that has been invented, even of the most recent work that has not yet found a place in the current textbooks. It has been said that the first essential for research is knowledge, especially a minute knowledge of the work that has been done in the subject proposed for investigation. Evidently the first thing to be done by anyone who would advance knowledge is to determine the position of the front. But it is equally necessary not to be oppressed by the vastness or the intricacy of that which is known if progress is to be made into the unknown. The effort to obtain knowledge must not be overwhelming; the worker must retain his freshness. He must still be able to ask himself questions, and to wish to know the answers. The attitude of a mind which is always seeking to know the answers to questions which are relevant, definite, and general, is what I mean by "curiosity" as an element in the equipment of an investigator. It cannot be acquired, though it may be encouraged. Skill can be acquired by developing native ability through constant practice, but the native ability must be forthcoming. Knowledge is constantly being increased. Fortunately the means for acquiring it are also constantly being improved. Text-books and treatises include always later additions to knowledge. Encyclopædias, such as the *Encyclopädie der Mathematischen Wissenschaften*, enable the would-be investigator to get rapidly to the place where, at the time when one of its articles was written, new work in the subject could be attempted. Indexes, such as the *Jahrbuch über die Fortschritte der Mathematik*, the *Revue Semestrielle*, or the *International Catalogue of Scientific Literature*, put it in his power to ascertain what new work has been done since the date of the Encyclopædia article. Reprinted collections of the writings of great investigators enable him to become acquainted with the best of the older work at first hand; and all the indexes and encyclopædias that have ever been compiled cannot take the place of the great original memoirs. They are the corner-stones of the edifice.

And here I would plead for more attention to the history of mathematics. Towards elucidating this history Great Britain has not done very much; the study of it receives here but little encouragement. Yet it seems to me to be extremely desirable, if not actually indispensable, for entering into the heritage that has been bequeathed to us, and for seeking to enhance its value. It may be hoped that the celebration this year of the tercentenary of Napier's invention of logarithms, and the series of excellent monographs on the work of the ancient Greek mathematicians, which we owe to Sir Thomas Heath, may induce us, as a nation, to take more interest in the history of our science.

Finally, to revert to the aspect of mathematics as a creative art, I would urge that an essential element in the equipment of an investigator is a literary education, or, if you prefer it, a training in the means of expression. It is necessary to be articulate, but more than this is desirable. It is desirable to be mathematically articulate, to be able to express mathematical ideas in such a way that they can be comprehended easily by those who have the requisite training. Some great work is marred by obscurity. This charge has been brought against even so great an originator as Abel. Others, such as Laplace, are models of lucidity. There is such a thing as style in mathematics, and it is worth cultivating. The mathematician is an artist; and every artist, we have been told by Mr. Bernard Shaw, must grow his own style out of himself. But there are points of style to which it is desirable to attend, such as clearness, arrangement, rigour, avoidance of haste, conciseness, notation. It is desirable to say exactly what one means, neither less nor more. It is desirable to introduce new ideas, or new relations, one at a time, so that each one seems to arise naturally just at the place where it makes its appearance in a piece of written work. No trouble is too great to secure rigour, if it can be secured. We have all heard how Newton kept back the publication of the work, which was ultimately embodied in the *Principia,* until he had obtained a conclusive proof that spheres attract as if their masses were condensed at their centres. If absolute rigour has occasionally to be sacrificed, it should be made perfectly clear at what points it is absent. A memoir should not bear marks of hurry; the argument should be developed in a straightforward fashion from the premisses to the conclusion. On the other hand, it should not waste time, as, for instance, by undue restriction of conditions in the main argument, with the object of excluding exceptional cases, or by overloading the main argument with details; it should always be possible to distinguish the wood from the trees. The choice of notation is not to be despised; it may make all the difference to the ease with which a piece of work can be assimilated, or a new idea applied to new questions. It may be necessary to rewrite a memoir more than once or twice if these advantages are to be secured. It is worth while.