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The future of mathematics.

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The nature of mathematics as we know it in our time is such as to inspire us with a great curiosity concerning the future of our science. While our mathematical knowledge has continued to increase in a swelling tide which threatens sometimes to engulf us, we are almost daily made aware of new areas and new problems which intrigue and challenge our most highly developed mathematical talents. We feel that great discoveries lie but a few years or a few decades ahead, and perhaps we even tend to regard whatever we can accomplish ourselves as little more than stepping-stones to greater achievements in time to come. Thus for us mathematics lies quite as much in the future as in the present or the past, and we sometimes yearn for a glimpse of what the imagined triumphs of the mathematics of the future may be like. For a mathematician who, like myself, has rounded out his period of youthful energy and creativity this desire to peer into the future becomes all the stronger because he would like to see there the hidden answers to the problems which have defied his most intensive efforts at solution and which he can no longer hope to overcome himself. Of course, this desire is doomed to frustration. To a certain extent it may be possible to project current developments a little way into the future, but anyone would be very rash who would don the mantle of a prophet in a field such as mathematics where so much—one might say nearly all—depends on inspiration and the insights of genius. Nevertheless, we all gain a little by pausing to consider what is currently going on in mathematics and to estimate whither and how far it is likely to carry us in the immediate future. A little thoughtful speculation serves to suggest lines of research likely to be fruitful, or practical means for increasing the effectiveness of our general approach to the task of scientific investigation in the field of mathematics. In discussing here the future of mathematics, I set myself no goal more ambitious than this.

A traditional way of trying to throw light on the future of mathematics is to present a list of important unsolved problems. Of all such lists the most celebrated is the one given by David Hilbert at the Paris Congress of 1900. I do not know how many mathematicians have consulted Hilbert's list or how many have been directly stimulated by it, but everyone realizes that the list has exerted an important influence and will continue to do so as long as any of the problems stated in it remains unsolved. I must confess that the first time I read Hilbert's list or the address in which he incorporated it was in preparation for this lecture. The wisdom of Hilbert's introductory remarks about the role of problems in the life of the working mathematician has not been in any way diluted by the passage of time; and the importance of the problems which he mentions specifically in his famous list will readily be recognized, whether they be evaluated intrinsically or by reference to the role they have actually played in the development of mathematics over the intervening period of time. However, it is also interesting to note, on looking backward, the respects in which Hilbert's list has fallen short of suggesting the shape assumed by that development. There are few hints to be found there of the great advances which were to come, for example, in topology. We should therefore be under no illusions as to the prophetic value of any similar list which we might try to draw up today. A mathematician willing to remain close enough to his own special interests could easily prepare a list of challenging problems related to them, but he would have difficulty in pointing to those most likely to be important or fruitful for the development of the branch of mathematics concerned. He would even risk poor results in trying to classify problems according to their degree of difficulty, I believe. Since mathematicians greatly enjoy the challenge offered by a problem generally regarded as a particularly hard one, they sometimes attribute to a problem which has long resisted efforts at solution an importance quite out of proportion to its general mathematical significance. I suppose that the four-color map problem is a rather good example of this phenomenon, because it is difficult to see what consequences would flow from a definitive treatment of the problem. Conceivably a successful method of attack would turn out to be significant as a means for handling apparently unrelated problems; but of this we cannot be certain. Indeed, many problems of greater intrinsic importance than the four-color problem have perhaps been important for mathematics mainly because they have given an impetus to the creation of methods and theories of general interest. Fermat's last theorem seems to me to constitute a problem in this category, and perhaps also the Riemann hypothesis. Certainly the solutions to these problems, like that to Hilbert's fifth problem, seem quite as likely to close off certain chapters in mathematics as to open new ones. On the other hand, there are many problems which clearly point to full programs of research. Taken in context, the Riemann hypothesis originally presented just such a problem; and the chief reason for thinking that proof or disproof of the hypothesis might have relatively few reverberations is that this program had already been quite thoroughly explored and elaborated in the early decades of the present century. To my mind the problems which suggest rather broad programs are the really interesting ones. They seem also to be the ones most congenial to the current attitudes of the majority of active mathematicians. Indeed, a modern mathematician, if pressed to mention a particular problem as one for which a solution is urgently desired, would perhaps prefer to propose the execution of some program of research or the search for the key to some still mysterious domain of mathematics as a more rewarding goal for his own efforts and those of others. To whatever conclusions my readers may come concerning the relation of problems to the advancement of mathematics or of mathematicians, I shall not venture to offer here any general list of the sort given by Hilbert, because I have not the breadth of knowledge or of experience to do so with any confidence.

However, I should like to make my bow to tradition by citing a few problems from fields which I do know fairly well—problems which have intrigued me beyond others or seem to held much interest for future mathematical research. First I may mention the study of harmonic and related forms on differential manifolds. What is already known about them strongly suggests that further investigation will disclose many important relations among analysis, algebra, number theory and topology. Many interesting results along these lines have already been obtained by A. Selberg and have been reported in lectures at Göttingen and at the Bombay Symposium on Zeta-Functions. The study of partial differential equations from a general point of view may also be expected to show significant progress over the next few decades, but its success must depend on the posing of the "right" problems. The literature has been greatly enriched by the publication of many substantial results in the last twenty years, and important new papers about partial differential equations continue to appear in increasing numbers. While great advances have been made possible by the theory of distributions and other techniques in the case of linear equations with constant coefficients, we are still a long way from understanding the nature of even slightly modified versions of the classical special cases. Thus relatively little is known about the ultrahyperbolic equation. A first step towards founding a general theory of partial differential equations must be the treatment of the linear equations with constant coefficients. In the case of linear equations with variable coefficients the theory of the elliptic and hyperbolic types has been pushed very far indeed, and a start is being made on a similar advance for the parabolic type. The non-linear hyperbolic case has also yielded significantly to the efforts of a series of skillful investigators. It seems to me that attention will have to be paid in the future to the Cauchy problem for non-hyperbolic equations, even though it is not "well-posed" in the sense of Hadamard and must therefore show some pathological features. Conditions on the boundary data necessary or sufficient for the Cauchy problem to have a solution would appear to be a matter of fundamental interest. In general the theory of partial differential equations is still a wilderness in which we lack any general principles of orientation. Physics has provided us with a certain amount of guidance in the past and will continue to do so, without doubt, but we must look elsewhere as well, perhaps especially to the field of differential geometry, for other indications of the directions in which our main efforts should be made.

The most important clues to the future of mathematics are perhaps to be found by considering the main tendencies and directions revealed by the current growth of the subject. If we first look at the internal developments alone, we are at once struck by the progressive mutual interpenetration of the different mathematical disciplines which began many decades ago and continues at an accelerated pace.

This phenomenon has created a great need for a conscious effort to unify modern mathematics at the level of exposition. The most ambitious and far-reaching program along these lines has certainly been the great work launched by N. Bourbaki. We may feel certain that the process of interpenetration and unification, far from having reached its end, will be a characteristic feature of the mathematics of the future. It is therefore inevitable that mathematicians must press further in the direction of abstraction and generalization if they are to obtain the deepest and most satisfying results. No mathematician with a good understanding of his field has ever valued abstraction or generalization merely for its own sake, I suppose. At this juncture there is possibly a certain danger that the great role played by these essential processes in the understanding and the unification of mathematics will mislead inexperienced or unperceptive mathematicians into overrating their intrinsic value and importance. As a corrective I would suggest that the young student of mathematics who admires the treatises of Bourbaki should make a point of reading also some of the important research papers and monographs written by Bourbaki's associates—Weil, Chevalley, Schwartz, and Serre, to mention some of the best known. At the same time there exists the opposite danger that too many mathematicians, particularly the specialists in analysis and the applied fields, will continue to ignore the trends toward unification because of a personal distaste for the abstract and the general in mathematics. While tastes are not easily altered, I think that the younger generation already shows clear signs of being much more at ease with the multiplying contacts between classical analysis and modern functional analysis, geometry, and algebra. The power of the new methods which are created through these contacts should eventually attract the attention of the theoretical physicists, who find themselves confronted at the present moment with grave mathematical difficulties of a fundamental nature.

By following the path of abstraction, mathematicians have not only been able to bring greater unity and cohesion into their subject—they have also been able to isolate fundamental mathematical entities, such as groups, rings, fields, vector spaces, topological spaces, differential manifolds, fiber bundles, sheaves, and so on; to explore the roles of these entities in the general structure of mathematics; and to discover a certain number of general principles, among which

I might mention as an example the duality between a mathematical system and its extremal homomorphisms. While these successes have been most noteworthy and certainly foreshadow others of even greater depth and interest, we have to recognize that the extension of our mathematical knowledge has made us aware of a growing number of more or less pathological situations. In fact, a good deal of our progress has been possible only because we have judiciously put to one side inconvenient or complicated pathologies which would otherwise have have diverted our attention and our efforts in unfruitful directions. The emergence of some of these pathologies can be taken as a sign that abstraction and generalization have possibly gone far enough along certain lines—or, for that matter, even too far. On the other hand, certain of these pathologies are of such a kind that sooner or later they must be dealt with. Thus algebraists have had to come to grips with the properties of the radical and with those of non-associative phenomena in ring-theory; topologists with the bizarre behavior of plane sets and of other topological spaces; and analysts with the peculiarities of operator rings in Hilbert spaces. The mathematician is today faced with such a wide range of general abstract problems involving known or putative pathological aspects that it is frequently very difficult for him to pick out an angle of attack which will eventually prove successful. This observation is well illustrated by the theory of topological linear spaces. The work of Mackey and Šmulian succeeded in developing the theory well beyond the stage to which the initial investigations of Banach and others had carried it; but the discovery of distributions by Schwartz was, I think, a prerequisite to tracing the path of progress among the many pathological pitfalls which abound in the theory, as has been done rather recently by Dieudonné, Schwartz, Bourbaki, and Grothendieck. It is very often the case that some contact with another field is necessary before fruitful directions of development can be chosen. This is something which the young mathematician needs to keep in mind. He also needs to recall, as he ventures into the rarefied atmosphere of the loftier domains of modern mathematics, Hilbert's good advice that he should frequently test his results by specializing them to concrete cases.

Needless to say, not all contemporary mathematics has the abstract character which we have been discussing. The ancient fields of geometry and number theory, as well as analysis and to a lesser extent even algebra, are very much concerned with concrete problems. Even when a very general or abstract point of view is adopted, as frequently happens in algebraic topology for example, the primary objective may nevertheless be one which is both quite specific and easily stated in concrete terms. Great advances have been made in these fields, particularly in number theory and topology, during recent years. The discovery of elementary proofs for deep number-theoretic results, such as the prime number theorem, and of powerful methods for defining and calculating topological invariants may be cited here. We may expect these advances to lead to others of equally concrete character in the foreseeable future. Moreover, there is every indication that the current resumption of progress in geometry—both differential and algebraic—is but the start of important new developments, in which the new topological and algebraic insights will play a significant role.

The growth of mathematics does not depend on internal forces alone. It has always been strongly influenced by the development of those fields of knowledge to which it is bound by the existence of successful applications of mathematical reasoning. The influences in question have always been mutual. For instance, not only has physics stimulated the study of important types of mathematical problem, but advances in pure mathematics have opened the way to the treatment of physical problems which would otherwise have remained unmanageable. Thus we have on the one hand the application of group theory to quantum mechanics made by Weyl and Wigner, which led to a strong revival of interest in the theory of group representations and to a successful attack on Hilbert's fifth problem (as reformulated in terms of topological algebra); and, on the other, the development of the tensor calculus by Ricci and Levi-Civitả, which prepared the way for Einstein's formulation of the general theory of relativity. We may also mention that the development of the spectral theory in Hilbert space similarly provided a clue to a workable non-relativistic wave-mechanics. The development of mathematics in the coming decades will be subject to very much broader influences of this kind, because of the striking penetration of mathematical modes of thought into a growing number of disciplines. The physical sciences will find that the biological and social sciences are becoming formidable rivals for the services and resources of modern mathematics. The theory of games and the theory of linear programming are new branches of mathematics which have grown out of attempts to treat problems in economics and management and which have already found important applications in the social sciences. I am convinced that they are only the first among new branches of mathematics which will develop in a similar way and have a similar significance for various parts of biology and social theory. Inevitably any such multiplication as I foresee of the connections between mathematics and the real world will exert strong and somewhat unpredictable influences upon the development of mathematics itself. In principle we may be justified in hoping for strictly mathematical theories of important fragments of the various sciences—physical, biological, and social—to emerge out of investigations of the unsolved problems which we see around us. Nevertheless, it would be rash to attempt specific predictions of the directions in which progress may be realized. As an example, we may consider Hilbert's proposal of 1900 that the problem of treating classical mechanics on the basis of appropriately formulated postulates should be undertaken by the mathematicians of his time. In fact the emergence of the special theory of relativity and the quantum theory within a few years of the day when Hilbert spoke deprived his suggestion of its relevance and replaced his problem by another which is beset with even more serious mathematical difficulties. I think we must resign ourselves to the probability that as the complexities of the real world unfold they will condemn similar suggestions to a similar fate. This is not to say that a complete postulational treatment of classical mechanics or of any other approximate model of a portion of reality would not retain great theoretical interest, especially for mathematics, despite its lack of precise application to the real world; but as a practical matter it would be too much to expect the task to command the energy and devotion which might be required for its execution when new and more satisfactory models demand study and investigation. The postulational method may, however, have a somewhat different and considerably more important role to play in the exploration of reality. Some of the most remarkable discoveries of modern physics —I have in mind particularly the discovery of the positron and that of the meson—have resulted from what can only be described as an application of the postulational method. When suitable assumptions were made about certain parts of the physical world in harmony with accepted general principles, it appeared that these elementary particles must be granted existence if the theory were to retain its symmetry. In each case the actual detection of the postulated particle followed the development of the general theory. It seems to me almost certain that analogous applications of the postulational method will be made more and more frequently in all fields of applied mathematics.

If the applications of mathematics are important to its future, the new developments in mathematics are equally important for its fields of application. We mathematicians have an obligation to make these developments accessible not only to the young mathematician who is beginning his scientific career, but also to the young scientist whose work will lie in some field other than mathematics itself. Scientists generally need to pay increasing attention to their own mathematical equipment, seeing to it that they are in a position to use the newer and more powerful tools provided by modern mathematics as well as the tried and familiar ones which have served them well enough in the past. Indeed, only a slight acquaintance with the current situation in such a highly mathematical discipline as quantum field theory strongly suggests that the critical difficulties will not be overcome without a much closer collaboration between physicists and mathematicians than has existed for twenty years or so. Eventually the complexities of the real world are likely to produce similar situations in most fields where mathematics can be applied. It would plainly be irrational for anyone to suppose that either the current situation in physics or the analogous situations likely to arise in the future can be resolved by ignoring the rapid day-by-day growth of mathematics. It is a sad truth that such ignorance exists today in many places where it should not, and inevitably works to retard the proper growth of science.

In discussing at this time the various external influences which are shaping the future of mathematics it is impossible for me to disregard certain influences of a practical rather than an intellectual character. Let me consider first the role which is beginning to be played by the modern computing machine, as well as the part which has been performed by the printing press for a long time without much notice being taken of it. Several years ago I listened with deep interest to an analysis by the distinguished physicist Professor I. I. Rabi of the profound influence which advances in instrumentation had exerted upon the development of physics. By contrast we mathematicians have been blissfully carefree in this respect. Even though we have unconsciously grown dependent upon the typewriter and the printing press, we still feel—and rightly, too—that our principal instruments are the pencil and the sheet of paper. Essentially our working practices can hardly be very different from those of Archimedes. But now our happy situation is undergoing a radical modification in at least two respects. The development of computing machines, particularly those of the modern high-speed electronic type, and the economic factors which are beginning to arouse us to a realization of our dependence upon the printing press are matters with which we shall have to reckon in forecasting the future of our science. The computing machine has already reached a stage of perfection where it is clearly affecting mathematics in two different ways. On the one hand it protects the mathematician against the complaint that his solutions are too often of little practical use because they fail to be easily computable. On the other, it is opening up a new and interesting field of mathematics concerned with the elaboration of the computing programs best suited to the different types of machine and the different kinds of mathematical problem. This field has essential contacts with logic, for a number of reasons. In the main the more advanced and powerful machines have been used to handle practical problems in applied fields, though tabulations of special functions and a few algebraic and number-theoretic questions have been treated by them. However, there are undoubtedly many ways in which the machines will prove themselves useful for pure mathematics. Professor von Neumann has suggested, for instance, that we may obtain much useful but hitherto inaccessible knowledge about the nature of partial differential equations by studying numerical solutions supplied in quantity by powerful machines. It is also conceivable that many enumerative questions of algebra and number theory could be treated either by using existing machines or by developing special machines. When suggestions of this kind are made, we need to be mindful of the economic factors involved: mathematics has to compete with other sciences and with various engineering enterprises for the use of these still somewhat costly and overburdened machines.

If computing machines are instruments which are opening up new possibilities for mathematics, the printing press is an instrument which, we are beginning to realize, is becoming inadequate to our requirements. We perceive this inadequacy in economic terms, but meeting it will involve developments in the printing process itself. While the amount of publishable mathematical material grows by leaps and bounds, without any sign of relaxation, printing becomes more costly and harder to arrange because of general conditions in the labor market. Because of the heavy technical demands it makes on the printer, mathematics is losing its ability to compete effectively against more easily satisfied customers of the presses. In the United States costs have risen to such a point that substitutes for the printing press are being sought and systematically used. In Russia the state intervenes to give scientific publication, including the mathematical, needed relief from the burden of printing costs, and protection against competing demands upon the state presses. At the present time it would be possible to shift a good deal of the business of mathematical printing to those countries where printing costs are now low, but this would afford only temporary relief at best. In the long run the only solution lies in making such improvements in the printing process itself that the productivity of the corresponding labor force will be considerably multiplied. This will undoubtedly require developments in the field of automation and a revision of the role of labor in the printing process. Thus we see that in this connection, too, instrumentation really has an important bearing upon the future of our science.

Turning now to other practical circumstances which are significant for mathematics, I feel that it would not be amiss to utter a note of caution concerning the perils of professionalization, if I can do so without appearing unduly pessimistic. In the half century which had passed since Hilbert's Paris address, mathematical research has gradually lost its amateur character and now tends to be pursued in a highly professional spirit. The tendency toward professionalism has been particularly marked in the last two or three decades. It is quite evident that mathematical activities are far more highly organized than they were fifty or even twenty five years ago. I do not need to compare the mathematics department of a contemporary university with its predecessor of 1900 or to enumerate the many journals and societies which have been founded in the interests of mathematics since the turn of the century. The vast and growing organization of mathematics is designed to stimulate and encourage the production of mathematical research in an increasingly professional atmosphere. How well this design has succeeded may be measured by noting the upsurge of mathematical publication as registered by the reviewing journals and the creation of new outlets for mathematical papers. While we welcome this increased scientific activity in our field, we should also pause to take stock of some of the perils which we may have to face—and if not we, then our successors. To some extent we have, of course, to reconcile ourselves to the fact that the price of progress is the assumption of administrative burdens which cannot be avoided if wider opportunities for research are to be provided. There is, however, another concomitant of increased activity in research upon which we would be unwise to look with complete resignation. I refer to the increased difficulty of communication as the profession becomes more numerous and more proficient in turning out mathematical results and as our subject itself becomes more ramified and complex. What we risk, despite the apparent tendencies of current mathematical thought, is a relapse into specialization of a rather sterile sort, accelerated by a general decline in mathematical standards. The rate of mathematical production, which is surely far from having reached its peak, is now so high that it is very difficult for the working mathematician to keep abreast of progress in his own field, unless it lies well outside the main currents. Added to the difficulties of sheer volume are those arising out of the growing number of vernaculars in which significant mathematical publication is made. At the same time the working mathematician is more and more put under a compulsion to publish his results, not so much because they may be significant but because he is obliged to repay the solicitous support of one sponsor or to enlist that of another. The inevitable reaction to these factors is for the majority of mathematicians to concentrate their efforts on limited fields with a view to maintaining a steady flow of papers, and without indulging in too much self-criticism as to their quality or originality. There are some signs that this reaction has already become rather too significant, and that we should already begin to take counsel as to how it may be confined within appropriate bounds. If there is any answer to this problem, it must be found in improving the means of communication and increasing the density of ideas per page in our journals. We mathematicians are notorious among scholars for our reliance on secondary sources. It is in the nature of our subject that such sources are particularly useful for our work. Hence an important element in improving our communications is to pay more attention to providing good, up-to-date secondary sources in the shape of modern text-books and general expository monographs, which can serve to keep the profession abreast of the recent developments in outline if not in all detail. At the same time teachers and editors have an obligation to insist that the young mathematician acquire a broad enough general knowledge, a clear enough expository style, and a sufficiently high standard of what is significant and important in mathematics that we may keep our meetings and our journals free of dull or trivial contributions.

But enough of these practical questions, vital though some of them may prove to be! In closing I would like to consider, however inadequately, a fundamental aspect of mathematics which is very far from being mundane. We all know that the logical foundations of our subject have been examined in a remarkably thorough way during the first half of the twentieth century, with results that have very disquieting features. In 1900 Hilbert formulated a program of research upon the foundations, aimed optimistically at establishing the consistency and completeness of certain parts of mathematics. This program, despite much work done by logicians in other directions, does not appear to have been advanced very far until Hilbert himself returned to it in the early twenties. Stung by the challenge of Brouwer's intuitionist "heresies"—for such he considered them—Hilbert launched a powerful and sustained effort to prove the consistency of arithmetic. Hilbert's papers provided an important stimulus to new work, leading very soon to the spectacular results of Gödel, showing that a certain formulation of arithmetic and the accompanying logical apparatus required to handle it adequately must, if consistent, permit the statement of undecidable propositions. Gödel's incompleteness theorem showed that Hilbert had been indeed too optimistic and opened up new possibilities of foundational inadequacies which were vigorously explored in the ensuing period. We mathematicians now find ourselves in the very uneasy position of not knowing which parts of mathematics are consistent or what problems are effectively solvable. While it is possible to establish the consistency of certain parts of mathematics, including various fragments of arithmetic, we live under the threat that all our proposals for a consistent theory of classes adequate for mathematics may collapse at any moment. More than one theory offered in recent years has been shown to be inconsistent, and the status of those which remain is a matter of speculation. On the other hand, we know that there are problems of mathematical interest which cannot be solved effectively—among them the word problems for semi-groups and for groups—and are therefore forced to realize that in studying a problem we must not only look for a positive or a negative solution but must also contemplate the possibility that it is instead unsolvable. Until our techniques for discussing solvability questions are somewhat more developed, we shall be embarrassed by the suspicion that certain problems, such as Fermat's last theorem, are not merely recalcitrant but are indeed unsolvable in some sense or other. The existence of undecidable propositions creates, it seems to me, a potential difficulty in treating consistency questions. Let us consider how one might attempt to prove the consistency of one of the standard formulations of the theory of classes, relative to some designated apparatus of proof. The most natural procedure is to seek a model of the theory of classes which can be proved non-contradictory. The Löwenheim-Skolem theorem shows that we can try to construct a model which will be denumerable relative to the apparatus of proof. If this apparatus includes as much of logic and arithmetic as Gödel needed to assume, then propositions about the model—in particular, the proposition that it is consistent—may be undecidable. Thus it is conceivable that the theory of classes, in a form adequate for mathematics, will defy all attempts at the construction of models which can be proved consistent in terms of an acceptable logical procedure. In a situation so studded with subtle difficulties, it is necessary either to choose some philosophical attitude which promises to hold something more than a temporary or unsatisfactory solution, or to ignore the difficulties until they have been shown to have a specific bearing on one's own field of mathematical inquiry. Most mathematicians, of course, do the latter; and many of those who have followed with the closest attention the critical discoveries in modern logic have been driven to take very pessimistic points of view. Brouwer and Weyl, in particular, felt that the difficulties were so grave that no solution short of abandoning aristotelian principles and accepting the weaker and more complex logic of intuitionism, with the drastic limitations implied for mathematics, would provide the security and strength which are needed. At an early stage, Poincaré, who was wary of Cantor's excursions into the realm of the transfinite, was ready to express his conviction that mathematics transcends logic. He felt in particular that a complete logical analysis of mathematical induction would not be possible, but was not prepared to abandon its use on that account. I do not feel it necessary to choose between the cramped security of Brouwer's intuitionism and the generous transcendentalism suggested by Poincaré. It does seem to me to be necessary to give up the nineteenth century dream of a complete and demonstrably consistent philosophical system for our Universe, or even for that fragment of our Universe which we call mathematics. Instead, we may have to live in a more adventurous spirit, never expecting to be given complete certainty about the foundations of our subject. Indeed, it seems to me philosophically presumptuous to imagine that we should be able to demonstrate the perfection of our logic, and absurd to suppose that whatever system of logic we may formulate will unfailingly turn out to be inconsistent. In any case, we mathematicians must see clearly the great interest which the logical study of the foundations of our subject has for us, and we must welcome with understanding the contributions which the future may bring in this difficult domain of investigation.

In looking ahead, as I have tried to do here, I think that the strongest impression one receives is an overwhelming sense of the tremendous potentialities of mathematics. It seems to me that we stand on the threshold of mathematical discoveries which will shortly dwarf our proudest historic achievements. What those discoveries may be or how they may be related to what we now know is hidden from our eyes. We can only hope that the opportunity will be granted to us and to those who come after us to explore undisturbed and whole-heartedly the paths of progress which are opening out before us.