THE FUTURE OF MATHEMATICS*

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"At one time," says Poincaré in his Rome conference on the future of mathematics, "there were prophets of misfortune; they reiterated that all the problems had been solved, that after them there would be nothing but gleanings left. . . . " "But," he added, "the pessimists have always been compelled to retreat . . . so that I believe there are none left to-day."

Our faith in progress, our belief in the future of our civilization are no longer as strong; they have been too rudely shaken by brutal shocks. To us, it hardly seems legitimate to "extrapolate" from the past and present to the future, as Poincaré did not hesitate to do. If the mathematician is asked to express himself as to the future of his science, he has a right to raise the preliminary question: what kind of future is mankind preparing for itself? Are our modes of thought, fruits of the sustained efforts of the last four or five millennia, anything more than a vanishing flash? If, unwilling to stumble into metaphysics, one should prefer to remain on the hardly more solid ground of history, the same questions reappear, although in different guise: are we witnessing the beginning of a new eclipse of civilization? Rather than to abandon ourselves to the selfish joys of creative work, is it not our duty to put the essential elements of our culture in order, for the mere purpose of preserving it, so that at the dawn of a new Renaissance, our descendants may one day find them intact?

These questions are not purely rhetorical; upon each man's answer, or rather (for such questions do not have answers), upon the attitude which he takes in front of them, depends in large measure the trend of his intellectual efforts. It was necessary, before writing about the future of mathematics, to formulate these questions, just as the faithful cleansed themselves before consulting the oracle. Let us now interrogate destiny.

Mathematics, as we know it, appears to us as one of the necessary forms of our thought. The archaeologist and the historian have shown us civilizations from which mathematics were absent. It is indeed doubtful whether they would ever have become more than a technique, at the service of technologies, if it had not been for the Greeks; and it is possible that, under our very eyes, a type of human society is being evolved in which they will be nothing but that. But for us, whose shoulders sag under the weight of the heritage of Greek thought and who walk in the paths traced out by the heroes of the Renaissance, a civilization without mathematics is unthinkable. Like the parallel postulate, the postulate that mathematics will survive has been stripped of its "evidence"; but, while the former is no longer necessary, we would not be able to get on without the latter.

The clinical student of ideas who limits his prognosis to the immediate fu-

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ture, and does not risk long-range prophecies, certainly observes more than one favorable symptom in contemporary mathematics. To begin with, while some sciences, conferring, as they now do, an almost unlimited power upon a ruthless possessor of their results, tend to become caste monopolies, treasures jealously guarded under a seal of secrecy which must of necessity become fatal to any genuine scientific activity, the real mathematician does not seem to be exposed to the temptations of power nor to the straight-jacket of state secrecy. "Mathematics" said G. H. Hardy in substance in a famous inaugural lecture, "is a useless science. By this I mean that it can contribute directly neither to the exploitation of our fellowmen, nor to their extermination."

It is certain that few men of our times are as completely free as the mathematician in the exercise of their intellectual activity. Even if some State ideologies sometimes attack his person, they have never yet presumed to judge his theorems. Every time that so-called mathematicians, to please the powers that be, have tried to subject their colleagues to the yoke of some orthodoxy, their only reward has been contempt. Let others besiege the offices of the mighty in the hope of getting the expensive apparatus, without which no Nobel prize comes within reach. Pencil and paper is all the mathematician needs; he can even sometimes get along without these. Neither are there Nobel prizes to tempt him away from slowly maturing work, towards a brilliant but ephemeral result. Mathematics is taught the world over, well here, badly there; the exiled mathematician—and who among us can to-day feel free from the danger of exile—can find everywhere the modest livelihood which allows him to pursue his work to some extent. Even in gaol one can do good mathematics if one's courage fail him not.

To these "objective conditions," or rather, as the physician would say, to these external symptoms, must be added others revealed by a more penetrating clinical examination. In recent times mathematics has demonstrated its vitality by passing through one of these periods of growing pains, to which it has been accustomed for a long time, and which are designated by the strange name of "foundation crises." It has come through it, not only without damage, but with great gain. Whenever wide domains have been added to the field of mathematical reasoning, it is necessary to inquire what techniques are allowed in the exploration of the new territory. One wants certain objects to have certain properties, one wants certain modes of reasoning to be admissible and one behaves as if they were. But the pioneer who proceeds in this way knows very well that some day the police will come to put an end to the disorder and to bring everything under the control of the general law. Thus, when the Greeks defined the ratio of two magnitudes for the first time with enough precision to raise the problem of the existence of incommensurable magnitudes, they seem to have believed and to have wanted all ratios to be rational and to have based the first sketch of their geometrical reasonings on this provisional hypothesis; some of the greatest advances in Greek mathematics are connected with the discovery of their initial error at this point. In the same way, at the beginning of the era of the theory of functions and of the infinitesimal calculus, one wished every analytical expression to define a function, and every function to have a derivative; we know to-day that these requirements were incompatible. The last crisis, which grew out of the sophistries, for which the "naive" theory of sets opened a way in its early stages, has led for us to a no less happy result, which can now be considered as permanently established. We have learned to trace our entire science back to a single source, constituted by a few signs and by a few rules for their use; this is unquestionably an unassailable stronghold, inside which we could scarcely confine ourselves without risk of famine, but to which we are always free to retire in case of uncertainty or of external danger. Only a few backward spirits still maintain the position that the mathematician must forever draw on his "intuition" for new, alogical or "prelogical" elements of reasoning. If certain branches of mathematics have not yet been axiomatized, *i.e.*, reduced to a form of exposition in which all terms are defined, and all axioms made explicit, in terms of the basic notions of set theory, this is simply because there has not yet been the time to do it. It is of course possible that some day our successors will want to introduce into set-theory modes of reasoning which we do not admit. It is even possible that the germ of a contradiction, which we do not perceive to-day, may later be discovered in the modes of reasoning we now use, although the work of the modern logicians makes this very unlikely. A general revision will then become necessary; one can feel certain even now that this will not affect the essential elements of our science.

But, if logic is the hygiene of the mathematician, it is not his source of food; the great problems furnish the daily bread on which he thrives. "A branch of science is full of life," said Hilbert, "as long as it offers an abundance of problems; a lack of problems is a sign of death." They are certainly not lacking in our mathematics; and the present time might not be ill chosen for drawing up a list, as Hilbert did in the famous lecture from which we have just quoted. Even among those of Hilbert, there are still several which stand out as distant, although not inaccessible, goals which will continue to suggest research for perhaps more than a generation; an example is furnished by his fifth problem, on Lie groups. The Riemann hypothesis, after the attempts to prove it by function-theoretic methods had been given up, appears to-day in a new light, which shows it to be closely connected with the conjecture of Artin on the L-functions, thus making these two problems two aspects of the same arithmetico-algebraic question, in which the simultaneous study of all the cyclotomic extensions of a given number field will undoubtedly play a decisive role. Gaussian arithmetic was centered around the law of quadratic reciprocity; we know now that this law is only a first example, we might better say the pattern of, the laws of "class fields," which control the abelian extensions of algebraic number-fields; we know how to formulate these laws so as to make them look like a coherent set. But, pleasant as this facade may be to the eye, we do not know whether it might not hide deeper lying symmetries. The automorphisms induced in the class groups by the automorphisms of the field, the properties of the norm-residues in the non-cyclic cases, the passage to the limit (inductive or projective) when the base field is replaced by extensions, for example, cyclotomic extensions, of indefinitely increasing degree, all these are questions on which our ignorance is almost complete and in whose study the key to the Riemann hypothesis is perhaps to be found. Closely connected with these questions is the study of Artin's conductor and, in particular, in the local case, the search for the representation, whose trace can be expressed by means of simple characters with coefficients equal to the exponents of their conductors. These are some of the directions which can and must be followed up in order to penetrate the mystery of non-abelian extensions; it is not impossible that we are here close to principles of extraordinary fertility and that, once the first decisive step on this road will have been taken, we shall gain access to vast domains whose existence is hardly suspected. For, however wide our generalizations of Gauss' results may be, we can hardly claim to have as yet really moved beyond them.

Even in the realm of abelian extensions, we have not made any progress towards the generalization of the theorems of "Kronecker's youth dream," the generation of class fields, whose existence is known, by means of values of analytic functions. While it has been possible, without serious difficulties, to complete Kronecker's unfinished work and to obtain the solution of this problem, in the case of imaginary quadratic fields, by means of complex multiplication, the key to the general problem, considered by Hilbert as one of the most important of modern mathematics, still escapes us, in spite of the conjectures of Hilbert himself and the efforts of his pupils. Must we look for it perhaps in the new automorphic functions of Siegel, in his modular functions of several variables? Or can the theory of the endomorphisms of abelian varieties, which has now made considerable progress, be of some help here? It is too early to risk acceptable conjectures on these questions; but their closer examination is bound to produce interesting results, even though they should be negative in character.

The foregoing discussion shows clearly not only the vitality of modern arithmetic, but also the close ties which connect it, to-day as in the days of Euler and the days of Jacobi, with the most deep-lying parts of the theory of groups and of the theory of functions. This essential unity, which appears in so many and in such diverse ways, is also found in many other places. The introduction by Hermite of continuous variables into the theory of numbers has led to the systematic study of discontinuous groups of arithmetical nature by means of the continuous groups in which they can be imbedded, of the symmetric Riemannian spaces associated with these groups, of the differential and topological properties of their fundamental domains (or rather, in modern terminology, of their quotient spaces), and of the automorphic functions which belong to them. The work of Siegel, continuing the great tradition of Dirichlet, of Hermite and of Minkowski, has opened entirely new paths here. On the one hand, we connect with Fermat, Lagrange and Gauss, the representation of numbers by forms, and the genera of quadratic forms. At the same time, we begin to see in outline the fertile principle, according to which the global aspect of an arithmetical problem can, under certain circumstances, be reconstructed from its local aspects. For instance, in the work of Siegel we see repeatedly that the number of solutions of some arithmetical problem in the field of rational numbers is expressed by means of numbers defined by the corresponding local problems, density of solutions in the real field and in the p-adic fields for all values of the prime p. This is a principle, analogous to Cauchy's theorem for the Riemann surface of an algebraic curve, with which one may connect also the famous "singular series" which appear in the application of the method of Hardy-Littlewood to problems in the analytic theory of numbers. Is it possible to formulate this principle in a general statement, which would allow us to obtain at one stroke all results of this character, just as the discovery of Cauchy's theorem made it possible to calculate by a single method a number of integrals and of series which were formerly treated by special distinct processes? It looks as if this were not yet a problem for the immediate future; so much the more reason to prepare for its solution by the study of well-chosen particular cases. It may be that this same principle will one day reveal the deep reason for the existence of Eulerian products, of which the extreme importance for the theory of numbers and the theory of functions has only become clear through the work of Hecke. Here we deal with the classes of quadratic forms, and not merely, as in the work of Siegel, with their genera; at the same time, we find ourselves at the core of the theory of modular functions, which has been infused with new life by these studies, and of the theory of theta functions. This domain is still so full of mystery, the questions which it raises so numerous and fascinating, that it would be premature to try to arrange them in order of importance.

At the same time, Siegel has taught us to construct discontinuous groups and automorphic functions by arithmetical methods; in this field the theory of functions, by its own efforts, had been unable to move forward since Poincaré. Indeed it is very likely, that, just as in the case of functions of a single variable, the thorough study of special functions of several complex variables will have to prepare the ground for an attack on the general theory. In the work of Siegel, the local and global geometrical study of fundamental domains, in effect of manifolds with a complex-analytic structure, tends to occupy a dominating role. Along this road, connection is made with the immense work accomplished by E. Cartan and its various extensions; at the same time one gets into the center of modern topology, the theory of fibre-spaces; and the invariants of Sitefel-Whitney appear, along with their generalizations. The intimate connections between these two domains had been suspected for some time, but their actual merging was made possible only through the recent discoveries of Chern, stimulated in their turn, at least in part, by considerations of algebraic geometry. Indeed, algebraic varieties, at least varieties without singularities in the complex field, are nothing but a special, and particularly interesting, class of manifolds with a complex-analytic structure; more precisely, they are manifolds on which, at least in all known cases, one can define one of these remarkable Hermitian metrics which were introduced by Kähler in connection with functions of several complex variables, and of which results of S. Bergmann, not yet fully clarified, furnish other examples. By a systematic, although not explicit use of these metrics, Hodge has recently obtained the first existence theorems for this type of manifold, generalizing the classical results of Riemann. While it may be too much to hope that such methods may one day lead to the uniformization of algebraic varieties (which, contrary to what happens in the case of curves, can not be done in general by means of unramified functions), there is little doubt that they can be extended to integrals of the third kind. The analogous generalization of the methods of Hodge to differential forms with singularities in the real domain raises still more important problems. It appears to be connected, on the one hand, with local properties of the equations of elliptic type which harmonic forms satisfy; on the other hand, it seems to be linked with an extension of de Rham's theory which would make it possible to obtain the homologic torsion of a manifold by means of differential forms with singularities. De Rham's results have, as a matter of fact, definitely clarified a certain aspect of the relation between homology groups and multiple integrals, and this accounts for the fundamental role they play in the work of Hodge and of Chern; but until now they have only made the homology groups with real coefficients accessible to differential methods; moreover, the striking and fertile analogy between chains and differential forms, which is expressed in these results, remains a mere heuristic principle, until we succeed in finding a common basis for these two concepts. To convert this principle into a method of proof has thus far succeeded only in a few special cases, for example, in some of the beautiful papers with which Ahlfors has in recent years given fresh life to the theory of analytic functions.

But, while algebraic geometry, as we have just seen, receives a fresh stimulus from the most recent developments in topology and in differential geometry, this field does not lack purely algebraic problems; and, thanks to the methods of modern algebra, our understanding of them no longer need depend upon flashes of intuition of a few privileged mortals. At present, the theory of surfaces, brilliantly but too rapidly developed by the Italian school, must yield place to a general theory of algebraic varieties, freed from restrictive assumptions as to the nature of the base field and as to the absence of singularities. The structure of the groups of divisor classes, with respect to the different known concepts of equivalence (linear, continuous, numerical), the study of unramified extensions of a field of algebraic functions, both abelian and non-abelian, these are the questions that call for solution first of all. Thanks to the results obtained, or at least made plausible, by the Italian geometers, we can more or less guess the answers; and their solution, perhaps already within our reach, must open the road for important advances. On the other hand, the study of algebraic geometry over various special fields of constants is still gropingly taking its first steps. In view of the fact that algebraic geometry over the complex field, studied for almost a century, has arrived by its own methods (topological and transcendental) at well-known important results, it is probable that other fields, finite fields, p-adic fields, fields of algebraic numbers, deserve to be studied, each by itself, by methods suited to their purpose. From this point of view, geometry over a finite field appears somewhat like a turntable, from which one may at will direct one's further progress either towards algebraic geometry proper, with the powerful tools already at its disposal, or towards the theory of numbers; it is precisely in that way that we are beginning to get a better insight into the nature of the Zeta-function and into the true nature of the Riemann hypothesis. In the same way, before undertaking the determination of the extensions of a field of algebraic numbers by means of their local properties, it might be indicated to solve the analogous problem, already difficult enough, concerning algebraic functions of one variable over a finite field, *i.e.*, to extend Riemann's existence theorem to such functions. To mention merely a particular case, one might ask whether the modular group, whose structure determines the fields of functions of a complex variable with only three points of ramification, plays the same role, at least with respect to the extensions of degree prime to the characteristic, when the field of constants is finite. It is not impossible that all questions of this kind can be treated by a uniform method, which would make it possble to deduce, from a result established (for instance by topological methods) for characteristic 0, the corresponding result for characteristic p; the discovery of such a principle would constitute an advance of the greatest importance. Of the same character, but still more difficult, are the problems arising in the modern study of finite groups. Is the theory of finite simple groups an analogue of the theory of simple Lie groups? It would probably be premature to make a frontal attack on this question at the present stage; by means of indirect procedures, in particular the study of p-groups, some progress has been made in this direction in recent years. As in many other questions of algebra and of the theory of numbers, so also here a new element has recently been introduced by the definition of the homology groups of an abstract group. The discovery of this concept, which generalizes the fruitful concepts of character and factor-set, is due to Eilenberg and MacLane, who introduced it in connection with H. Hopf's studies in pure combinatorial topology; it will have to be subjected for some time to systematic study, before its scope and possibilities of application can be estimated.

While arithmetic, in the widest sense, is for its devotees always the queen of mathematics, and while for that reason we have allowed ourselves to dwell on it with predilection, this is not to say that other branches of mathematics do not offer as many problems worthy of sustained attention. The work of a Cartan alone contains enough material to keep busy several generations of geometers. His general theory of systems in involution has not been carried to its conclusion by its author, who appears not to have been able to overcome all the difficulties of algebraic character which it involves. Concerning the theory of "infinite Lie groups," undoubtedly very important, but for us very obscure, we know nothing beyond what is found in the memoirs of Cartan, a first exploration into an almost impenetrable jungle; this jungle threatens to overgrow the paths which already have been marked out, if the indispensable task of clearing is not undertaken very soon. The modern theory of Lie groups proper, studied by a combination of Cartan's methods with those of modern topology, is far from complete; even in the theory of semi-simple groups and in the theory of the symmetric Riemannian spaces associated with them, a good many results are attainable only by *a posteriori* verification, making use of our knowledge (also due to Cartan) of all simple groups. But, as has already been suggested, it is principally in the topological theory of fibre spaces, in the theorems of de Rham and in the notion of homotopy group, that we now find the tools best suited to the global study of the generalized geometries of Cartan. To give but a single example, the classical Gauss-Bonnet formula, until recently the only result in which a topological invariant was expressed by means of the integral of an invariant differential form, appears to us now as but the first term of a whole sequence of formulas, to which Chern's methods give access and whose systematic study has barely been started.

But, even though involutory systems should, in principle, enable us to obtain everything which can be reduced to the local problem of Cauchy-Kowalewski in the theory of partial differential equations, this is merely one aspect of the existence problem for solutions of these equations; and, from several points of view, it is not its most interesting aspect. Beyond this, we find important results concerning equations of very special types, chiefly elliptic and hyperbolic, some of which are of very recent date; but, although the study of these types, to which our predecessors were led by mathematical physics more than a century ago, is far from complete, it will not do to stop indefinitely at this point. The system which is satisfied by the real part of an analytic function of several complex variables does not belong to any of these simple types; however, function-theory has taught us, for example, that the most general singularities which they can have are, in a sense which is still not easy to specify, made up out of elementary singularities which are characteristic varieties; at any rate, one can interpret the theorems of Hartogs and of E. E. Levi in this manner. In this form, they present an obvious analogy to known results concerning hyperbolic equations; it is this analogy which suggests that we look for the germ of a general theory in a fuller development of the concepts of characteristic variety and of elementary solution. On the other hand, in the work of Delsarte, and in that of S. Bergmann and of his pupils, we find the first examples of the transformation of differential equations by means of integral or of integro-differential operators. It looks as if we have here the germ of entirely new developments and of a classification of systems of partial differential equations, which falls entirely outside the framework of classical methods. In particular, as was shown by Delsarte, the series of orthogonal functions to which elliptic problems lead naturally, are found to be transformed into series of much more general types; some isolated examples of these are found in classical analysis, but their general study presents problems of the greatest interest. The mathematician can no longer be satisfied here with Hilbert space, with which he has become as familiar as with the Taylor series or the Lebesgue integral; must he look for the most appropriate tool in the theory of Banach spaces, or must he have recourse to more general spaces? It must be admitted that Banach spaces, interesting and useful as they have already proved to be, have not yet brought about the revolution in analysis which some people expected of them; but it would be a counsel of despair to abandon their study now before the various possibilities of application have been more fully explored. However it is possible that they are both too general to be suited to as exact a theory as that of Hilbert spaces, and too special to lend themselves to the study of the most significant operators. They do not include, for instance, the space of indefinitely differentiable functions; and it is only in that space that the operators of L. Schwartz can be defined, which represent formally the derivatives of all orders of arbitrary functions. Perhaps the basis for a new calculus, founded on the generalized Stokes theorem, is to be looked for here; and this may give access to the relations between differential operators and integral operators. Ideas of this character have already proved very useful in special problems, for instance in the calculus of variations under the name of Haar's lemma as well as in certain papers of Friedrichs. Similarly, the well-known theorem, which asserts that the mean of a harmonic function on a circle is equal to its value at the center, expresses the fact that a certain operator, defined by a mass distribution in the plane, is, in a certain sense, a linear combination of the values of the Laplacian in the closed domain bounded by the circle. Also connected with these questions is the problem, mentioned above, of the representation of differential forms as sums of chains, which arises from the theory of de Rham. It is possible that we have in these researches the dim outlines of an operational calculus, destined to become in one or two centuries as powerful an instrument as the differential calculus has been for our predecessors and for ourselves.

All of this has to do only with the local or semi-local study of partial differential equations; indeed, apart from the simple cases which can be treated by means of the theory of Hilbert spaces or by the direct methods of the calculus of variations, the study of partial differential equations in the large, for instance on a compact analytic manifold, appears too difficult to justify any hope that it can be attacked for a long time to come. On the other hand, the study in the large of ordinary differential equations raises a large number of interesting problems; they are difficult, but within our reach. It will suffice to mention as an example the recent beautiful proof, by E. Hopf, of the ergodic character of the geodesics on every compact Riemannian manifold of everywhere negative curvature. Related to this subject is also the study of van der Pol's equation and of relaxation oscillations, one of the few interesting problems which contemporary physics has suggested to mathematics; for the study of nature, which was formerly one of the main sources of great mathematical problems, seems in recent years to have borrowed from us more than it has given us.

But, incomplete as the foregoing enumeration can not fail to seem to our colleagues, it has undoubtedly exhausted the attention of more than one reader;

and yet, for lack of space and for lack of necessary competence, we have not spoken of the geometry of numbers, nor of diophantine approximations, of the calculus of variations, of the calculus of probabilities, or of hydrodynamics, neither have we mentioned at all several problems, to-day in the background of interest, which could be reactivated by a new idea and restored to the vital stream of mathematics. As a matter of fact, we neither can nor want to lay out a route for the future development of our science; this would be a futile task, indeed it would be a ridiculous enterprise, for the great mathematicians of the future, like those of the past, will flee from the beaten track. They will solve the great problems which we shall bequeath to them, through unexpected connections, which our imagination will not have succeeded in discovering, and by looking at them in a new light. It was our purpose, in passing some of the principal branches of our mathematics in review, to draw attention to their robust vitality and to their fundamental unity. We believe to have shown, not only that there are large numbers of problems, but also that there are very few really important problems which are not intimately related to others which, at first sight, seem to be far removed from them. When a branch of mathematics ceases to interest any but the specialists, it is very near to its death, or at any rate dangerously close to a paralysis, from which it can be rescued only by being plunged back into the vivifying sources of the science. "Mathematics," said Hilbert at the end of his 1900 lecture (and it would be quite in order to quote the conclusion of this lecture in full), "is an organism for whose vital strength the indissoluble union of the parts is a necessary condition."

Does this mean that mathematics is becoming a science for erudites, and that it will no longer be possible to do creative work in mathematics until one has grown gray in the harness, and exhausted from burning the midnight oil for many years in the company of dusty tomes? This would at the same time be a sign of its decline; for, be it strength or weakness, mathematics is not a science that prospers on details, painstakingly collected in the course of a long career, on patient reading, on observations or on filing cards, amassed one by one so as to form a bundle from which an idea will ultimately come forth. Perhaps it is more true in mathematics than in any other branch of knowledge that the idea comes forth in full armor from the brain of the creator. Moreover, mathematical talent usually shows itself at an early age; and the workers of the second rank play a smaller role in it than elsewhere, the role of a sounding board for sounds in whose production they had no part. There are examples to show that in mathematics an old person can do useful work, even inspired work; but they are rare, and each case fills us with wonder and admiration. Therefore, if mathematics is to continue to exist in the way in which it has manifested itself to its votaries until now, the technical complications with which more than one of its subjects is now studded, must be superficial or of only temporary character; in the future, as in the past, the great ideas must be simplifying ideas, the creator must always be one who clarifies, for himself and for others, the most complicated tissues of formulas and concepts. Hilbert indeed asked himself: "Is it not going to become impossible for the individual worker to embrace all the branches of our science?" and he justified his negative answer, not only by his example, but by remarking that every important advance in mathematics is related to the simplification of methods, to the disappearance of old procedures which have lost their usefulness, and to the unification of branches which were until then foreign to each other. It is quite likely that the contemporaries of Apollonius for example, or those of Lagrange, were familiar with the same feeling of growing complexity which tends to overwhelm us to-day. It is undoubtedly true that the modern mathematician does not know certain details of the theory of conic sections as well as Apollonius did, or as a candidate for a French competitive examination, but this does not lead any one to think that the theory of conic sections should form an autonomous science. Perhaps the same fate is in store for some of the theories of which we are proudest. The unity of mathematics would not be threatened by such an occurrence.

The danger lies elsewhere. Although it is more contingent in character, it does not strike us as less serious; and it seems to us that we cannot bring our reflections on the future of mathematics to a conclusion without saying something of it. We have already said that our civilization itself seems to be under attack from all sides; but this remark was couched in too general terms. "Ne sutor ultra crepidam": it is as mathematicians that we must look at the contemporary world. Our tradition is healthy; are we assured of transmitting it undamaged? In some European countries, particularly in Germany until the start of the Hitler regime, there existed, still a short time ago, university instruction, based on a solid secondary education, which made sure that the mathematical apprentice acquired specific subject-matter knowledge and also the general culture without which nothing of importance can be accomplished. What do we see to-day? In France, none of the essential parts of modern mathematics is taught in our universities, except by a lucky chance. One looks in vain for a university course which puts the advanced student in contact with any one of the great problems which we have listed.* Even the elements of the science are too frequently taught in such a way that the student has to learn everything over again if he wants to push on; the extreme rigidity of a mandarin-caste founded on obsolete academic institutions is the cause of the fact that every attempt at modernization is doomed to failure, unless it remains restricted to verbal changes. Italy, which had formerly a flourishing mathematical school, seems to have fallen into a state of sclerosis analogous to that with which France is threatened, but which has had there still more immediate and destructive effects. We do not know what principles guide, at present, secondary and higher education in the U.S.S.R. There are in that country a number of first-rate mathematicians, but they seem to be absolutely prohibited from crossing the frontiers; and, if such practice should persist, it can hardly in the long run have

* (Author's footnote) This was written in 1946; the same would not be unqualifiedly true to-day.

any other result than the slow choking off of all scientific life. The most remote, as well as the most recent history of our science, shows sufficiently to what extent the contacts between one country and another, prolonged sojourns of students and of teachers at foreign universities, not official sessions at which one drinks toasts while waiting for the next airplane, are an indispensable condition for all progress. We believe that more favorable conditions are found in England and in some of those nations of western Europe which are small only in military statistics. As to Germany, only the future can show whether she will find within herself the necessary elements for linking up with the brilliant tradition interrupted by fifteen years of organized stupidity. Beyond the Atlantic finally, we find a large country, which counts its universities by the hundreds, its students by the hundred thousands, and where, in the words of H. Morrison, the great American specialist in educational problems, "one wanted the education of the masses, one has mass production in education." Thorstein Veblen once sketched in a small book, too little read, the plan of higher education in the United States, and he has done it in a masterly manner; let us merely indicate how the future mathematician is formed in this country which produces more "mathematicians" than perhaps all the rest of the world. In the most favorable cases, one sees a student who, towards the end of his stay at the University, has three or four years at his disposal in which to acquire at the same time the knowledge, the method of work and the elementary intellectual apprenticeship, for which nothing that he has experienced before has in the least prepared him. His only way out under these circumstances is to seek his salvation in the most narrow specialization; in this way, he can often, if he is intelligent and has good guidance, do useful work. Beyond this, he runs the great risk of not being able to survive the stupefying effects of the purely mechanical teaching which he will have to inflict on others, in order to earn his living, after having undergone it himself for too long a time. Whether, in other fields, mass production, thus understood, may produce good results, we are not qualified to determine; we hope to have made it clear that this can not be the case in mathematics. If, unfortunately, the plausible doctrine of making education available to all has had such consequences in a country which lacks, it is true, a strong intellectual tradition, do we not have reason to fear the spread of the contagion to a Europe enfeebled by a catastrophe without precedent?

But if, as Panurge, we ask the oracle questions which are too indiscreet, the oracle will answer us as it did Panurge: Trinck! This advice the mathematician follows gladly, pleased as he is to believe that he will be able to slake his thirst at the very sources of knowledge, convinced as he is that they will always continue to pour forth, pure and abundant, while others have to have recourse to the muddy streams of a sordid reality. If he be reproached with the haughtiness of his attitude, if he be summoned to do his part, if he be asked why he persists on the high glaciers whither no one but his own kind can follow him, he will answer, with Jacobi: For the honor of the human spirit.