Reproduïm avui el capítol 3 del llibre de Morris Klein "Why the professor can't teach", del qual vem publicar el capítol 1 en el darrer Butlletí

The Nature of Current Mathematical Research

Even victors are by victory undone.

John Dryden

We have traced the widespread rise of vigorous mathematical research in this country. We have also observed that the flourishing of research promises not only direct benefits but also indirect beneficial influence on all levels of education. However, the values that might accrue from research depend on the quality of research being done. Let us therefore look into the nature of current mathematical research.*

In mathematics, research has a very special meaning. Specifically, it calls for the creation of new results, that is, either new theorems or radically different and improved proofs of older results. Expository articles, critiques of trends in research, historical articles or books, good texts at any level, and pedagogical studies do not count. Thus, the criterion of research in mathematics differs considerably from what is accepted in, for example, a subject such as English. In this area, in addition to the creation of fiction, essays, poetry, or other literature, criticism, biographies that shed fresh light on important or even unimportant literary figures, histories of literature, and texts that may be primarily anthologies are considered original work. Perhaps this distinction between what should be accepted in the respective fields is wise, but let us see what it has led to in mathematics.

Because the United States entered the world of mathematical research several hundred years after the leading Western European countries had been devoting themselves to it, our mathematicians, in an endeavor to compete, undertook special directions and types of investigations.

One move was to enter the newer fields, such as the branch of geometry now called topology. The advantage of a new field for tyros in research is that very little background is needed and the best concepts and methodologies are only dimly perceived. Hence, because criteria for value are lacking, almost any contribution has potential significance. Publication is almost assured.

Of course, the ease with which one can proceed in a new field is somewhat deceptive. New fields generally arise out of deep and serious problems in older fields, and anyone who wants to do useful work must know much about these problems and grapple with them at length in order to secure significant leads. On the other hand, if all one is trying to do is prove theorems, then it is sufficient to start with almost any potentially relevant concept and see what can be proved about it. And if one gets a result that the other fellow didn't get, one may proceed to publish it.

The United States was not the only country that took such a course. After World War I, Poland was reconstituted as a nation and the Polish mathematicians undertook a concerted effort to build up mathematics in their country. They decided to concentrate on a narrow field, the branch of topology called point set theory. Why point set theory? Because at that time the subject was still new. One could therefore start from scratch, introduce some concepts, lay down some axioms, and then proceed to prove theorems. This example is offered not to malign Polish mathematicians. There were and are some very good men among them, and good men, even starting from very shallow beginnings, will make progress and produce fine work. What is significant is the deliberate and openly stated decision to start with point set theory because one did not have to know much mathematics to work in it.

Generalization is another direction of research that promises easy victories. Whereas the earlier Greek and European mathematicians were inclined to pursue specific problems in depth, in recent years many researchers have turned to generalizing previous results. Thus, while the earlier mathematicians studied individual curves and surfaces, many twentieth-century mathematicians prefer to study classes of curves—and the more general the class, the more prized any theorem about it. Beyond generalizing the study of curves, mathematicians have also carried most geometric studies to n-dimensions in place of two or three.

Some generalizations are useful. To learn how to solve the general second degree equation $ax^2 + bx + c = 0$, where a, b, and c can be any real numbers, immediately disposes of the problem of solving the millions of cases wherein a, b, and c are specific numbers.

But generalization for the sake of generalization can be a waste of time. A lover of generalization will too often lose sight of desirable goals and indulge in endless churning out of more and more useless theorems. However, those for whom publication is the chief concern are wise to generalize.

Hermann Weyl, one of the foremost mathematicians of this century, expressed in 1951 his contempt for pointless generalizations, asserting: "Our mathematics of the last few decades has wallowed in generalities and formalizations." Another authority, George Polya, in his *Mathematics and Plausible Reasoning*, supported this condemnation with the remark that shallow, cheap generalizations are more fashionable nowadays.

Mathematicians of recent years have also favored abstraction, which, though related to generalization, is a somewhat different tack. In the latter part of the nineteenth century mathematicians observed that many classes of objects—the positive and negative integers and zero; transformations, such as rotations of axes; hypernumbers, such as quaternions (which are extensions of complex numbers); and matrices—possess the same basic properties.

Let us use the integers to understand what these properties are. There is an operation, which in the case of the integers is ordinary addition. Under this operation the sum of two integers is an integer. For any three integers, $a + (b+c) = (a+b) + c$. There is an integer, 0, such that $a + 0 = 0 + a = a$. Finally, for each integer, a, for example, there is another integer, $-a$, such that $a + (-a) = -a + a = 0$. These properties are more or less obvious in the case of the positive and negative integers.

But if in place of the integers we now speak of a set of objects, which might be transformations, quaternions, or matrices, though the particular set is not specified; and of an operation, whose nature depends on the particular set of objects but is also not specified, we can state in abstract language that the elements of the set and operation possess the same four properties as those of the integers. The abstract formulation defines what is called technically a group. A group, then, is a concept that describes or subsumes the basic properties of many concrete mathematical collections and their respective operations under one abstract formulation. If one can prove, on the basis of the four properties of the abstract group, that additional properties necessarily hold, then these additional properties must hold for each of the concrete interpretations or representations of the group.

The concept of a group, very important for both mathematics and physics, is only one of dozens of abstract systems or structures—the latter is the fashionable word—and many mathematicians devote themselves to studying the properties of these structures. In fact, the study of structures is flourishing; the work done on groups alone fills many volumes.

Abstraction does have its values. One virtue, as already noted, is precisely that one can prove theorems about the abstract system and know at once that they apply to many concrete interpretations instead of having to prove them separately for each interpretation. Further, to abstract is to come down to essentials. Abstracting frees the mind from incidental features and forces it to concentrate on crucial ones. The selection of these truly fundamental ones is not a simple matter and calls for insight. Nevertheless, there can be shallow and useless abstractions as well as deep and powerful ones. The former are relatively easy to make, and one must distinguish this type of creation from that involved in solving a new and difficult problem—such as proving, as Newton did, that the path of each planet, moving under the gravitational attraction of the sun, is an ellipse or the far more difficult problem, which has still not been solved, of finding the paths of three bodies when each attracts the others under the force of gravitation. Unfortunately, many recent abstractions have been shallow.

Beyond the shallowness of some abstractions, there are other negative features of all abstractions. Although unification through abstraction may be advantageous, mathematics pays in loss of resolution for the broadened abstract viewpoint. An abstraction omits concrete details that may be vital in the solution of specific problems. Thus, the manner of executing the processes of adding whole numbers, fractions, and irrational numbers is not contained in the group concept. The more abstract a concept is, the emptier it is. Put another way, the greater the extension, the less the intension.

Abstraction introduces other objectionable features. As a theory grows abstract it usually becomes more difficult to grasp because it uses a more specialized terminology, and it requires more abstruse and recondite concepts. Moreover, unrestrained and unbridled abstraction diverts attention from whole areas of application whose very investigation depends upon features that the abstract point of view rules out. Concentration on proofs about the abstraction becomes a full-time occupation, and contact with one or more of its interpretations can be lost. The abstraction can become an end in itself, with no attempt made to apply it to significant concrete situations. Thus, the abstraction becomes a new fragment of mathematics, and those fields that were to receive the benefits of unification and insight are no longer attended to by the unifiers.

Weyl spoke out against unrestrained abstraction, maintaining that "in the meantime our advance in this direction [abstraction] has been so uninhibited with so little concern for the growth of problematics in depth that many of us have begun to fear for the mathematical substance."

The inordinate attention given to the study of abstract structures caused another mathematician to warn, "Too many mathematicians are making frames and not enough are making pictures."

Another popular direction of research may be described roughly as axiomatics. To secure the foundations of their subject the late-nineteenth-century mathematicians turned to supplying axiomatic bases for various mathematical developments, such as the real number system, and to improving those systems of axioms where deficiencies had been discovered, notably in Euclidean geometry. Since there are dozens of branches of mathematics, there are dozens of systems of axioms. Quite a few of these contain ten, fifteen, or twenty axioms. The existence of such systems suggests many new problems. For instance, if a system contains fifteen axioms, is it possible to reduce the number and still deduce the same body of theorems? Given a system of axioms, what would be the effect of changing one or more of them? The classic and notable instance of this last-mentioned type of investigation is, of course, the change in the Euclidean parallel axiom and the resulting creation of hyperbolic non-Euclidean geometry. Changes in several of the axioms led to elliptic non-Euclidean geometry. Clearly, if a system contains as many as fifteen axioms, the changes that can be considered are numerous.

The investigation of the consequences of changing the Euclidean parallel axiom was indeed sagacious. By contrast present-day mathematicians, with little reason to do so, pursue all sorts of axiomatic investigations so that in the eyes of many practitioners, mathematics has become the science of axiomatics. The current activity in this area is enormous and overstressed. When axioms were believed to be self-evident truths about the constitution of the physical world, it was laudable to simplify them as much as possible so that their truth could be more apparent. But now that axioms are known to be rather arbitrary assumptions, the emphasis on deducing as much as possible from, say, a minimum number of axioms, which are often flagrantly artificial and chosen merely to reduce the number, is not warranted. The objective seems to be to produce more theorems per axiom, no matter how distorted and unnatural the axioms may be. Consequently, one finds long papers with tedious, boring, and ingenious but sterile material. Nevertheless, the popularity of axiomatics is readily understood. It does not call for the imaginative creation of new ideas. It is essentially a reordering of known results and offers many minor problems.

In his 1951 critique of current features of mathematical research, Weyl included axiomatics:

> One very conspicuous aspect of twentieth century mathematics is the enormously increased role which the axiomatic approach plays. Whereas the axiomatic method was formerly used merely for the purpose of elucidating the foundations on which we build, it has now become a tool for concrete mathematical research.... [However] without inventing new constructive processes no mathematician will get very far. It is perhaps proper to say that the strength of modern mathematics lies in the interaction between axiomatics and construction.

Still another questionable activity in modern axiomatics, derogatively termed "postulate piddling," involves the adoption of axioms merely to see what consequences can be derived. A prominent mathematician of our time, Rolf Nevanlinna, has cautioned: "The setting up of entirely *arbitrary* axiom systems as a starting point for logical research has never led to significant results.... The awareness of this truth seems to have been dulled in the last few decades, particularly among younger mathematicians."

Felix Klein, a leading German mathematician who was active from about 1870 to 1925, remarked that if a mathematician has no more ideas, he then pursues axiomatics. Another distinguished professor once remarked that when a mathematical subject is ready for axiomatization it is ready for burial and the axioms are its obituary.

The several directions research has taken point up the fact that there are soft and hard problems—or soft and hard research. In the days when the density of good mathematicians was high, soft problems were not often tackled. Moreover, nineteenth-century mathematicians, who were the first to grasp the advantage of abstract structures, faced a higher order of difficulty than present-day mathematicians face in that type of research. In recent times soft problems have been the ones most often tackled, and even if the proofs are complicated, the results may still be merely difficult trifles.

There is still another feature of mathematical research that affects seriously the interaction of research and teaching—the chief concern of this book. Whether it involves generalization, abstraction, or axiomatics or pursues some other direction, modern research is commonly acknowledged to be almost entirely pure—as opposed to applied. Pure research may be characterized as mathematics for mathematics' sake. That is, however the theme or problem is obtained, the reasons for undertaking it may be aesthetic interest, intellectual challenge, or sheer curiosity: "Let's see what we can prove." This is the motivation in axiomatics when a researcher rather arbitrarily decides to change an axiom just to find out what changes this entails in the resulting theorems. Applied mathematics, on the other hand, is concerned with problems raised by scientists, or with a theme that a researcher believes is potentially applicable.

There is no doubt that the problems of applied mathematics are more difficult. The branches of mathematics customarily associated with applied mathematics are now several hundred years old, and the giants of mathematics have worked in them. Anyone who wants to do something significant today in partial differential equations, for example, must have quite a background. And for the processes of idealization and model building in applied mathematics one must have intimate knowledge of the relevant physical field in order not to miss the essence of the phenomenon under study. (See also Chapter 7.)

Pure mathematics is more accessible for another reason. Whereas in applied work the problem is set by scientific needs and cannot be altered, the pure mathematician tackling problem A may, if unable to solve it, convert it to problem B, which could be A with more hypotheses or a related but actually different problem suggested by the work on A. He may end up solving problem B, or while working on it he may find unexpectedly that he can solve problem C. In any case he has a result and can publish it. In other words, the applied mathematician is required to climb a rugged, steep mountain, whereas the pure mathematician may attempt such a climb, but if he finds the going tough he can abandon it and settle for a walk up some nearby gentle hill.

Traditionally mathematics had been concerned with problems of science. But these, as we have noted, are far more difficult to solve. Only relatively few men today pursue them. The abandonment of tradition and of the rich source of problems has been justified by a new doctrine: Mathematics is independent of science, and mathematicians are free to investigate any problems that appeal to them. The research done today, so it is claimed, will be useful ten, fifty, and one hundred years from now. To support this contention the purists distort history and point to alleged examples of such happenings. But a correct reading of history belies the contention. Practically all of the major branches of mathematics were developed to solve scientific problems, and the few that today are pursued for aesthetic satisfaction were originally motivated by real problems. For example, the theory of numbers, if one dates its beginning with the Pythagoreans, was undertaken for the study of nature. Nevertheless, the break from science has widened sharply since about 1900, and today most mathematicians no longer know any science or even care whether their work will ever have any bearing on real problems.

Marshall Stone, formerly a professor at Yale, Harvard and Chicago, in an article "Mathematics and the Future of Science" (1957) admits that generality and abstraction—pure mathematics generally—are the chief features of modern mathematics in our country. The best applied mathematics, he concedes, is done by physicists, chemists, and biologists. He might well have added that mathematics developed in a vacuum proves to be vacuous.

Quite a different feature of modern research is specialization. The worldwide spread of scientific and technological pursuits has made it impossible for any individual to keep pace with a broad spectrum, and the desire to avoid being beaten to results by an ever-increasing number of competitors, and thus lose the fruit of months of activity, has almost forced mathematicians to seek out corners of their own. Mathematics is now fragmented into over a thousand specialties, and the specialties multiply faster than amoebas. The many disciplines have become autonomous, each featuring its own terminology and methodology. A general meeting of mathematicians resembles the populace of Babel after God had confounded their efforts. Pure mathematicians are unable to communicate with applied mathematicians, specialists with other specialists, mathematicians with teachers, and mathematicians with scientists. It is almost a certainty that if any two mathematicians were chosen at random and shut up in a room they would be so unintelligible to one another as to be reduced to talking about the weather. Consequently, general meetings are now far less numerous than colloquia and conferences on particular topics.

Illustrations of the narrowness of modern research are so abundant that almost any article in any journal can serve as an example. Let us note one or two simple ones. One article treats powerful integers. An integer is powerful if whenever it is divisible by a prime p it is divisible by p^2. Several papers on this less-than-enthralling theme have already appeared and more are sure to follow. Would that the papers be more powerful than the concept. Still another theme deals with admirable numbers. The Pythagoreans of the sixth century B.C. had introduced the concept of a perfect number. A number is perfect if it equals the sum of its divisors (other than the number itself). Thus $6 = 1 + 2 + 3$. If the sum of the divisors exceeds the number, the number is called abundant. Thus, 12 is abundant because the sum $1+2+3+4+6$ is 16. One can, however, ask about the *algebraic* sum of the divisors; that is, one can consider adding and subtracting divisors. Thus $12 = 1+3+4+6-2$. Numbers that are the algebraic sum of their divisors are called admirable. One can now seek admirable numbers and establish properties about them, which no doubt are equally admirable. In this same vein are a superabundance of theorems on superabundant numbers.

These very trivial examples are, of course, chosen merely because they can be presented quickly to illustrate the narrowness and pointlessness of much modern-day research. Just as everyone who daubs paint on canvas does not necessarily create art, so words and symbols are not necessarily mathematics.

Specialization began to be common in the late nineteenth century. Now most mathematicians work only in small corners of mathematics, and quite naturally each rates the importance of his area above all others. His publications are no longer for a large public but for a few colleagues. The articles no longer contain any indication of a connection with the larger problems of mathematics, are hardly accessible to many mathematicians, and are certainly not palatable to a large circle. Mathematical research today is spread over so many specialties that what was once incorrectly said of the theory of relativity does apply to the research: Any one topic is understood by no more than a dozen people in the world.

Each mathematician today seeks to isolate himself in a domain that he can work for himself and resents others who might infringe on his domain and secure results that might rob him of the fruits of his work. Even Norbert Wiener, one of the great mathematicians of recent times, admitted that he "did not like to watch the literature day by day in order to be sure that neither Banach nor one of his Polish followers had published some important result before me." And the late Jacques Hadamard, the dean of French mathematicians until his death at the age of ninety-eight in 1963, said, "After having undertaken a certain set of problems and seeing that several other authors had begun to follow the same line, I would drop it and investigate something else."

There is a way of joining the crowd and yet keeping aloof from the hurly-burly. A favorite device is to introduce some new concept and develop endless theorems whose significance is, to say the least, questionable. The creator of such contrived material may even train doctoral students who, young in the ways and judgment of mathematics, may really believe in the worth of the material and so spread the name of the master.

Most of those working in specialties no longer know why the class of problems they are working on was originally proposed and what larger goals their work is supposed to aim at. The modern topologist may not know Riemann's and Poincaré's work. The modern worker in Lie algebras is not likely to know what purpose Lie algebras serve. Of course, these specialists are putting the cart before the horse. The limited problems should contribute to and illuminate the area in which they lie. But the specialists would seem to be taking the position that the major areas exist in order to provide problems on which to exercise their ingenuity. Nor do they recognize that specialization promotes one's degeneration into a narrow, uncultured person, a craftsman but nothing more. The specialist becomes what José Ortega y Gasset called a "learned ignoramus."

As the process of subdivision progresses, specialized research makes less and less provision for synthesis, for pulling strands together, for asking the basic, overriding questions, for stepping back from the easel and looking at the whole picture. Indeed, specialized research does not concern itself with synthesis. Though it may foster localized competence, it may simultaneously rationalize, and even glorify, general ignorance and deliberate unconcern for those questions that transcend the narrow bounds of specialism. Yet these questions are the ones that make sense of the whole enterprise.

Rampant specialization turns out to be a misfortune for the specialized pursuits themselves, although it seems to arise through concern for their exclusive needs. One obvious reason is that specialization encourages uninhibited intellectual inbreeding and it is a law, not only of human genetics, that inbreeding increases the incidence of undesirable characteristics. Furthermore, the process of unlimited specialization tends to bar a subject from the interest and participation of anyone outside, even when the outsider could make an essential contribution toward maintaining relevance in the questions asked and the methods used to pursue them. It also dims awareness of the fact that the pursuit of truth is indivisible, that all creative scholars, writers, and artists are ultimately engaged in one great common enterprise—the search for truth. In other words, specialization curtails the basic commitment of the scholar.

The evils of specialization have been noted by many wise men. In his history of nineteenth-century mathematics (1925), Felix Klein said that academic mathematicians grow up in company with others like trees in a woods, which must remain narrow and grow straight up in order even to exist and reach some of the light and air.

Weyl said in 1951, "Whereas physics in its development since the turn of the century resembles a mighty stream rushing in one direction, mathematics is more like the Nile delta, its waters fanning out in all directions." In the preface to his book, *The Classical Groups* (2nd ed., 1946), he expressed concern about too much specialization in mathematics: "My experience has seemed to indicate that to meet the danger of a too thorough specialization and technicalization of mathematical research is of particular importance for mathematics in America."

David Hilbert, the greatest mathematician of this century, was also concerned about specialization. He wrote:

> "The question is forced upon us whether mathematics is once to face what other sciences have long ago experienced, namely, to fall apart into subdivisions whose representatives are hardly able to understand each other and whose connections for this reason will become ever looser. I neither believe nor wish this to happen; the science of mathematics as I see it is an indivisible whole, an organism whose ability to survive rests on the connection between its parts.

The trend to specialization has already caused mathematics departments to split into four or more departments—pure mathematics, applied mathematics, statistics and probability (with antagonism between the two groups in this area portending a future split), and computer science. Communication among these departments is, of course, almost nonexistent, and competition for money, faculty, and students is keen.

Clearly, the evils of specialization lead to inferior work. Specialists define their own area of interest and, as we have already noted, choose areas in which they can avoid competition and the larger, more vital problems. Publication is the goal, and whatever results can be published are published. Ortega y Gasset remarked in his *The Revolt of the Masses* that specialization provides what the biologist would call ecological niches for mediocre minds.

Since specialization is the order of the day, why not journals for specialists? These are now by far the most numerous, and specialists read only the journals in their own areas, thus precluding even their awareness of anything outside their specialty. There are few journals that cover—and none that unify—developments in several fields, to say nothing about all fields of mathematics.

Mathematical research has always suffered from another evil: faddism. Like all human beings mathematicians yield to their personal enthusiasms or are ensnared by the fashions of their times. The directions of research are often determined by mathematicians with prestige and power who themselves are subject to whims or the search for novelty. In the nineteenth century, for example, the study of subjects such as elliptic functions, projective geometry, algebraic invariants, and special properties of higher-degree curves was carried to extremes. Most of this work, considered remarkable in its time, would be considered insignificant today and has left almost no trace in the body of mathematics.

It is no criticism of mathematicians that an area of research pursued vigorously for a time should prove unimportant in the long run. Mathematicians must use judgment as to what may be worthwhile, and even the wisest can make mistakes. Research is a gamble and one can't be sure that the work will pay off. However, faddism tends to carry a subject beyond any promise of significance.

Fads flourish today because usefulness to science is no longer a standard, and the standard of beauty is purely subjective. The most pointed criticism of faddism was made by Oscar Wilde: A fad is the fantastic which for the moment has become universal.

Another evil of faddism is that possibly valuable but nonfashionable ideas are disparaged. Hence, brilliant work is often neglected, though it is sometimes belatedly and often posthumously recognized. The classic example is found in the work of Gauss. Gauss, though already acknowledged as great when still a young man, feared to publish his work on non-Euclidean geometry because he would have been condemned by his fellow mathematicians or, as he put it, because he feared the clamor of the Boeotians, a reference to a dull-witted ancient Greek tribe. Fortunately, Gauss's work on non-Euclidean geometry was found among his papers after his death. By that time his reputation was so great that his ideas were accorded the utmost respect.

Researchers who place high value on their work should be obliged to read a somewhat detailed history of mathematics, a subject most mathematicians do not know. They would be amazed to find how much that was regarded as vital and central in the past has been dropped so completely that even the names of those activities or branches are no longer known. Though the lesson of history is rarely learned, fads do not, fortunately, dominate the directions of research for long. What individuals create is destined to live only insofar as it is related to the evolutionary development of mathematics and proves fruitful in its consequences.

One additional source of research papers of dubious value should be mentioned—Ph.D. theses and their offspring. The students, beginners in research, cannot tackle a major problem; what they do tackle is not only suggested by a professor but is performed with his help. The results are generally minor and, in fact, usually the professor can see in advance how to solve the problem. If he could not, he might worry about whether he is assigning too difficult a problem to a beginner.

The new Ph.D. is, in today's world, forced to produce low quality research. If he enters or seeks to enter the university field, where researchers are now more sought after, he is under pressure to publish. Under these conditions what will he publish? He is at a stage in life where he is really not prepared to publish a paper of quality. Typically, his one experience in research was his doctoral thesis, in which he was guided by a professor and gained only enough knowledge to produce an acceptable thesis. Hence, all he actually is prepared to do is add tidbits to his thesis. But he cannot afford to be deterred by the knowledge that his publications may be insignificant. Some publication is better than none. Were he to try to solve a deep problem requiring extensive background and several years to complete, with the danger of failure all the greater, he would have nothing to show for quite some time, if ever. Hence, he must tackle and publish what can be done readily, even if the solution is labored and the result pointless.

The results of pressure on faculty and young scholars to publish, the natural expansion of research in our scientifically oriented culture, the entry of the Soviet Union, China, and Japan during this century into the group of countries leading in research, and the expansion of Ph.D. training to meet the needs of universities and colleges (which in recent years has meant 750 to 1,000 mathematics Ph.D.'s per year in the United States alone) are reflected in the volume of publication. There are now over a thousand journals devoted wholly or partially to mathematical research. About five hundred are devoted solely to mathematics, and new ones are appearing almost weekly. Summaries of the articles are published in *Mathematical Reviews*, which does not cover all articles and in fact neglects pedagogy and much applied mathematics. In 1970 there were 16,570 reviews; in 1973, about 20,000. If all applied mathematics had been covered there would have been about 40,000 reviews in 1973. The expansion of publications has been going on at the rate of 5 percent annually. In the period 1955 to 1970 the volume of publication equaled the volume in all of the rest of recorded history. The published papers are about one-fourth of those submitted to journals. Hence, one can see how much effort is put forth by faculty to climb the ladder of research.

To help mathematicians keep track of what has been published, secondary and even tertiary aids, such as indices and lists of titles, have been developed. There is an *Author Index of Mathematical Reviews*, which lists by author and subject the summaries published in *Mathematical Reviews*. For the years 1940 to 1959 *The Index* has 2,207 pages. For 1965 to 1972 it has 3,032 pages and 127,000 items, whereas the *Indexes* of the previous twenty-five years, 1940-1964, covered 156,000 items in all. There is also a journal, *Contents of Contemporary Mathematical Journals* (biweekly), that offers an index classified by subject of all current papers and books in mathematics. About 1,200 journals are covered and these do not include some in applied mathematics. We may await momentarily an index of the *Contents* and an index of all indices.

The volume of publication has evoked critical comments from prominent mathematicians. One of them, Peter Hilton, has written, "...we are all agreed that far too many papers are being written and published. We are turning into a community of writers who do not read simply because we have no time to do so. It is a terrifying thought that if we were to spend eighteen hours a day reading new mathematics we would have substantially more to read at the end than at the beginning." In addition to zero population growth this country should aim for zero publication growth.

It was generally agreed in the 1930s, when the pace of research was much slower, that nine out of ten papers had little to say and had no impact on mathematics. Some significant quantitative information was supplied by Kenneth O. May, a professor at the University of Toronto, who studied the nearly two thousand publications from the seventeenth century to 1920 on the limited topic of determinants. He presents the following data:

New ideas and results	234	14%
Duplication (beyond independent simultaneous publication)	350	21%
Texts and education	266	15%
Applications of results	208	12%
Systematization and history	199	12%
Trivia	737	43%
Totals including overlap	1994	117%

The explanation of the 117 percent is that some papers fell into two or more categories; actually there were 1,707 separate papers. Professor May estimated that the significant information about determinants, including the main historical accounts, is contained in less than 10 percent of the papers. He also mentions that in 1851 there were ten duplications of a paper published in a leading journal.

Today, with far more papers published and far less concern for the significance of the research, one might estimate that no more than 5 percent of the publications offer new material. The duplication is endless. Some of it is noted in *Mathematical Reviews*. *The American Mathematical Monthly* occasionally reports duplications and errors and even cites instances of purportedly new research material that has already appeared in texts.* This is not to say that all of the other 95 percent are wasted. A few have educational value. Nevertheless, the journals are filled with papers of flea-sized significance, and these pollute the intellectual world as noxiously as the automobile pollutes the air we breathe.

Authors deliberately publish minor variants of older research or repeat older results in new terminology. Unfortunately, the introduction of new terminology is a never-ending game, and a translation of old material can pass undetected, just as a French paper must be accepted as new by one who can't determine whether it has appeared in German. One famous nineteenth-century German mathematician did simply translate English papers into German and publish them as his own. Some researchers take one reasonably coherent paper and break it up into three or four smaller ones. This stratagem permits much repetition, thus resulting in more published pages and giving the impression of a teeming mind.

The profusion of articles and the ever-increasing number of journals make it impossible for even the specialist to read what is published in his own area. Hence, though he may pretend to know what has been done, he actually ignores the literature except for the few papers that he happens to know bear directly on his immediate goal—publication of his own paper. Months or years later some observer may note a duplication and call attention to it.

* See, for example, the issue of December 1976, pp. 798-801.

Apart from the expense involved, the flood of papers seriously hampers research. A conscientious researcher will try to keep abreast of what is being done in his area, partly to utilize the results already obtained and partly to avoid duplication. He must then wade through a vast number of papers at the expense of considerable time and effort, and at that he will not cover all the relevant literature.

The problem of keeping abreast of the literature had already begun to bother Christian Huygens in the late seventeenth century. In 1670 he complained, "...it is necessary to bear in mind that mathematicians will never have enough time to read all the discoveries in geometry (a quantity that is increasing from day to day and seems likely in this scientific age to develop to enormous proportions) if they continue to be presented in a rigorous form according to the manner of the ancients." Leibniz at the end of the seventeenth century deplored "this horrible mass of writing which continually increases" and which can only "drive away from the science those who might be tempted to indulge in it."

In mathematics, where the newness of a result should be readily recognized and the difficulties overcome in proof readily apparent, it would seem that papers would be easily and accurately evaluated. Most journals do send manuscripts to referees before accepting them. But the good mathematicians who might serve as referees are so busy doing their own research, and the volume of publication they must follow is so enormous, that most do nothing about judging work in their own specialty, to say nothing of other areas of mathematics. Moreover, most papers are so sparse in explanation that their correctness is hard to judge.

The narrowness of mathematicians also renders them unfit to discriminate between what is fundamental and what is trivial, between basic insight and mere technical byplay. For interdisciplinary papers it is almost impossible to find competent referees. Personal factors also intervene. Individuals favor friends and discriminate against rivals.

The state of refereeing is revealed by the reactions to a recent decision of the American Mathematical Society. Up to 1975 all papers submitted for publication in the several journals supported by the Society were sent to referees with the names and affiliations of the authors recorded on the papers. The Society decided to try, for one of its journals, blind refereeing, that is, submitting the paper to the referee without the name and affiliation of the author. The protests of referees and even of two of the associate editors of that journal were vehement. They pointed to the thanklessness of the work, the difficulty in finding competent referees, and the problem of judging the correctness and worth of a paper. In the ensuing debate, partly through published letters, the opponents of blind refereeing admitted that the name and affiliation of the author helped immensely in the refereeing process. What these opponents were really saying is that they were not judging papers on their merits but were relying on the reputation of the author and his institutional affiliation to aid in determining the correctness and value of his work. If one may judge by the protests, many referees used no more than this information to make their decisions. This debate brought into the open all the weaknesses of the refereeing process.

Moreover, today many papers are published without judgment by referees. There are countless symposia each year, and the papers read there are published automatically in the proceedings. Some universities produce their own journals, in which faculty members can publish at will. Publication in the *Proceedings of the National Academy of Sciences* is automatic not only for members but also for nonmembers whose papers are submitted through a member. The extent of the Academy's publications may be judged by the fact that in 1970 the editors decided to restrict each member to no more than ten papers per year.

The present situation contrasts sharply with what prevailed in the seventeenth, eighteenth, and nineteenth centuries. Of course, there were fewer publications. But papers were sent to referees who were not only distinguished mathematicians but also broad scholars. Even then there were slips in both acceptance and rejection. R.J. Strutt, the son of one of the greatest mathematical physicists, Lord Rayleigh, relates in his life of his father that a paper by Lord Rayleigh that did not have his name on it was submitted to the British Association for the Advancement of Science and was rejected as the work of one of those curious persons called paradoxers. However, when the authorship was discovered, the paper was judged to have merit. Nevertheless, on the whole the refereeing of earlier times was competent and critical. Moreover, the editors took pride in the quality of the work published in their journals and were anxious to maintain excellent reputations. They therefore took pains to secure competent criticism of articles submitted. It is also relevant that usefulness to science served as the major standard by which most papers were judged.

Actually, what is major or minor in research can be very difficult to determine. François Vieta, who first taught us to use letters to stand for a class of numbers, as in $ax^2 + bx + c = 0$, an idea that now seems trivial but was not advanced until after two thousand years of first-class mathematics had been created, gave mathematics the basis for all proof in algebra and analysis. Surely this idea was as valuable as any major result of Newton.

The assertion that quality of research is difficult to judge may seem to contradict our earlier assertion that most papers have little, if any, value. The worth of a few papers—for example, those that solve a long-standing problem that had baffled great minds—is certainly great. In other cases the authors state why the results they have obtained are important, so that the work can be more readily judged. When Vieta introduced letters for classes of numbers he stated that he could now make the distinction between numerical algebra and a science of algebra (to use modern terminology). Perhaps many a seemingly worthless paper has merit, but if that merit is not apparent to knowledgeable mathematicians, only an adverse judgment is in order.

Some sociologists of science are trying to measure the quality of research papers by the number of times a given paper is cited by later papers. Toward this end they invented and use the *Science Citation Index*. But this measure is almost childish. Very good papers are often soon superseded by ones that advance the subject still further. Even when the advances are minor, the later papers will surely be the ones cited. A fad will be cited many times over a period of years. Many young researchers cite their professors, even at the expense of the true creator, in order to curry favor. Accepting citations, then, would seem to require first measuring the honesty of scientists.

Modern mathematical research seems impressive. There is a vast and growing structure. Recent work has delineated more sharply the nature of the older subjects and has pointed the way to almost endless paths of new developments. Abstractions and generalizations have linked apparently unrelated subjects, giving mathematics some measure of unity, and have put some difficult classical theorems in a new setting where they become more natural and meaningful, at least to a trained mathematician. Mathematics now has a more qualitative character, in contrast to the manipulative and quantitative character of much of classical mathematics. Many new subjects have been created; and areas of older subjects that no longer seem significant have been discarded. We no longer learn all 467 theorems in Euclid's *Elements* or all 487 theorems in Apollonius' *Conic Sections.*

But a critical look produces dismay. The proliferation of new themes, generalization, abstraction, axiomatics, and specialization may yield easy successes, but they divert attention from more concrete and difficult problems concerned with ideas of substance. Abstractions and specialties abandon reality to enter clouds of thin and diffused themes. An overweeningly arrogant antipathy to papers that do not follow the modern fashions also encourages less valuable activity.

Mathematicians today care less and less about why mathematics should be created and pursued. They pay far less attention to what is worth knowing or what benefits society; nor do they question why society should support them. One of the most disturbing facts about current research is that graduate students, young Ph.D.'s, and even many established mathematicians no longer ask, Why should I undertake this particular investigation? Any inquiry that promises to produce answers and publication is regarded as worthwhile. No commendable purpose need be served except, perhaps, to advance the career of the researcher. A problem is a problem is a problem, and that suffices. Though criticism is rarely voiced, one past president of the American Mathematical Society and the Mathematical Association of America did have the courage to deprecate much modern research.

No doubt much worthless research is done in all academic fields. But remoteness and pointlessness are far more prevalent in mathematics. The reason stems from the nature of the subject, especially as it is currently pursued.

Mathematics deals not with reality but with limited abstractions. In past centuries these did come largely from real situations, and the prime motivation for the mathematics was to learn more about physical reality. It was recognized that the pursuit of well-chosen problems in mathematics proper must directly or indirectly pay dividends in scientific work, and mathematicians were obliged to keep at least one eye on the real world. But today mathematicians know better what to do than why to do it. The pointlessness of much current research is evident in the very introductions to papers. Students and professors seeking themes for investigation scan the publications and tag onto them. Many a paper begins with the statement, "Mr. X has given the following result. . . . We shall generalize it," or, "Mr. X has considered the following question. . . . A related question is. . . ." There may be no point to either the generalization or the related question. Another common introduction states, "It is natural to ask. . . ."; a most unnatural and far-fetched question follows. The consequence is a wide variety of worthless papers.

Mathematical research is also becoming highly professionalized in the worst sense of that term. Research performed voluntarily and sincerely by devoted souls, research as a relish of knowledge, is to be welcomed even if the results are minor. But hothouse-grown research, which crowds the journals and promotes only promotion, is a drag on science. Intellectual curiosity and the challenge of problems may still provide some motivation, but publication, status, prizes, and awards such as election to the National Academy of Sciences are the goals, no matter how attained. Deep problems that call for the acquisition of considerable background, years of effort, and the risk of failure are shunted aside in favor of artificial ones that can be readily tackled and almost as readily solved.

This indictment of current research may surprise many people. Surely mathematicians are men of intellect and would not write poor or worthless papers. But the quality of the intellects engaged in research runs the gamut from poor to excellent. Francis Bacon in his *Novum organum* (1620) sought to mechanize research and was rightly and severely criticized. His own contemporary Galileo demonstrated through his work the extent to which originality and serendipity must enter. Bacon may indeed have oversimplified the task of research, but his expectation that anyone can do it, even "men of little wit," is not far from what happens today in mathematical research.

Professor Clifford E. Truesdell, an authority in several applied fields and a man of vast knowledge, has had the courage to speak caustically. In his *Six Lectures on Natural Philosophy* he says:

> Just as the university has changed from a center of learning to a social experience for the masses, so research, which began as a vocation and became a profession, has sunk to a trade if not a racket. We cannot fight the social university and mass-produced research. Both are useful—useful by definition, since they are paid, if badly.... The politician, the lawyer, the physician, the general, the university official are all modest men, more modest than most mathematicians. ...Research has been overdone. By social command turning every science teacher into a science-making machine, we forget the reason why research is done in the first place. Research is not, in itself, a state of beatitude; research aims to discover something worth knowing. With admirable liberalism, the social university has declared that every question any employee might ask is by definition a fit object of academic research; valorously defending its members against attacks from the unsympathetic outside, it frees them from any obligation to intellectual discipline....