The Mentality of the Mathematician. A Characterization

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When I was asked to prepare a public address to the University for this solemn occasion, I thought immediately of the topic that I intend to deal with today. After all, in such a speech one usually discusses an issue in one's own discipline. However, purely mathematical topics are not suited for a general audience—the discipline of mathematics is virtually unaffected by daily life and the concerns of the educated public. This is easily borne out just by a look at audiences. In contrast to other specialties, it is seldom—and then only in the early semesters—that a mere admirer—in this case a nonmathematician—strays into a mathematical lecture. Never before have I addressed an audience like this, at least not in my capacity as a mathematician. This is why I propose to speak not about mathematics itself, but about and for mathematicians. I shall try to bring the working mathematician somewhat closer to you. However, this cannot be done by giving examples of mathematicians who were also excellent poets, painters, architects, lawyers, statesmen, philosophers, or theologians. Rather, I must bring before you the characteristics of a mathematician when he works in his field.

Very frequently, the layman is ignorant of the most characteristic quality of the creative mathematician—his great productivity. In fact, the layman often thinks that mathematics is by now a closed science, and gives little thought to the origin of the discipline he is familiar with from school. It is very likely that there is just one theorem that he associates with a particular scholar, namely the so-called theorem of Pythagoras. It is possible that he knows the jocular saying that since Pythagoras' time the oxen low whenever someone makes a great discovery, for Pythagoras so rejoiced at his discovery that he offered a hecatomb. Of course, the offering of a hundred animals is just a legend, but it is reasonable to assume that, out of gratitude to the gods, Pythagoras sacrificed oxen. And we think that it is a justified belief that only divine inspiration can give mathematical discoveries. That is why Eratosthenes and Perseus, in the manner of winners in an Olympic competition, made votive offerings out of joy at attaining their goals, their mathematical constructions—Eratosthenes with a historically significant poem that has come down to us and is greatly prized by philologists.

Unfortunately, the reports on ancient mathematicians are full of gaps. We have better knowledge of personalities only in the period after the middle of the 16th century. That is why the more recent times are more fruitful for our purposes. A beautiful example of a consciously experienced productive moment is the discovery of analytic geometry by René Descartes on November 10, 1619. Descartes tells us that one winter during the Thirty Years' War he was billetted in a small town (near Ulm) in great loneliness and cold, "in the fireplace", as he puts it. Then an illumination came upon him that led him with astounding speed to a multiplicity of geometric derivations. Beyond that, the method he discovered that evening became one of the most powerful impulses for the development of all of modern mathematics.

At this point we shall go, for a moment, somewhat deeper and emphasize a trait that is characteristic at least for mathematical output. Descartes himself believed that, through an illumination, he had discovered a new science—"*cum mirabilis scientiae fundamenta reperirem*". But this was hardly the case. His great contribution was not the discovery of a completely new idea—that of the unity of algebra and ge-

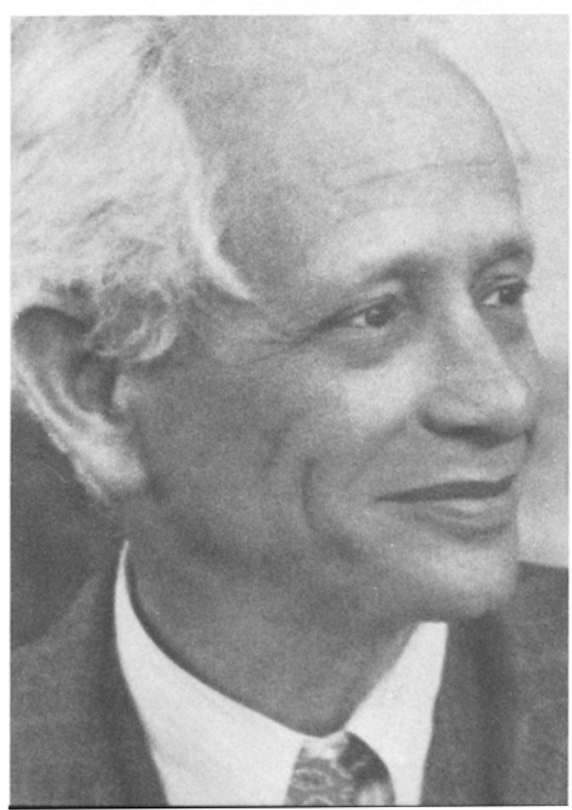
* Translated by Abe Shenitzer. The translator wishes to thank Hardy Grant for reading the translation and suggesting a number of improvements.

ometry. It is even incorrect to say that he realized the existing idea in a manner more daring than his predecessors. What is true is that the illumination came to a man of surpassing algebraic talent that enabled him to solve the most difficult particular problems and, more importantly, to a thinker endowed with incomparable shaping power, who presented and applied the idea—seized in a vision—with admirable sharpness, clarity, and terseness, with almost rhetorical brilliance. The historical significance of his contribution lies, above all, in formulation. It was this that produced such fruitful effects on his contemporaries—who, not surprisingly, had for the most part the impression of something entirely new.

We find similar occurrences throughout the history of mathematics. The origin of ideas is often unclear, the roots reaching far back into time cannot be unravelled. But the form is always the property of one person, that which is truly individual, which happens but once. And so, in my opinion, when evaluating a contribution we should not attach decisive importance to priority. The usually provisional finding that an idea first turned up in the work of one or another person is seldom very significant. Trains of thought derived from economic life—for example, decisions concerning patent claims—are not applicable to the historical investigation of scientific development. On the other hand, the most beautiful crown of laurels should, of course, go to the man who first lifts an idea out of its dark early stage to the bright clear light and presents it completely—even if unfavourable circumstances deny him continued direct and fruitful influence.

Here it should be borne in mind that a natural tendency tempts each of us into making exaggerated historical evaluations. Yet these are somehow misplaced. For the historian, the purest joy is to relish the contemplation of the ups and downs of the development, of the connections, of the breaks and transitions, to try to see the divine spark in each of the creators and to relive their productive moments.

Priority arguments were very frequent in the espe-



cially productive centuries—the 16th and 17th. In the middle of the 16th century the Italian mathematicians conducted bitter feuds over whose merit was greatest in the rousing of algebra from its virtually uninterrupted thousand-year sleep. What was involved was, in the first place, the magnificent discovery that cubic equations can be solved by the extraction of roots. This discovery was the first great step forward beyond the results achieved by the mathematicians of antiquity. Thus its history is important; but unfortunately this history is submerged in impenetrable darkness. We know that the first discoverer, Scipione del Ferro, died as professor in Bologna in 1526. Since he published nothing, we are entirely ignorant of how he hit upon his solution. Knowledge of such a solution reached Tartaglia in a roundabout way; Tartaglia, however, claimed to have discovered it on his own. He communicated it to Cardano who published it—against Tartaglia's will—in his famous algebra book *Ars magna* without having understood its derivation.

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Geronimo Cardano, who died in 1576 at the age of 75, was a typical man of the Renaissance. In view of our present topic—the creative power of the mathematician—Cardano is of special interest to us. His productivity was unbelievably extensive. Ninety years after his death, ten large folios of his work appeared, and the publisher assured readers that this was only half of what Cardano had written. There is no area between heaven and earth that he left untreated. He wrote about all the natural sciences, medicine, astrology, theology, philosophy and history. His autobiography—which Goethe compared to Benvenuto Cellini's—has great charm. In it he describes with touching ingenuousness a life afflicted with manifold misfortunes. At times we are strongly reminded of Rousseau's *Confessions.* Goethe writes at length about Cardano in his history of the science of color—about his talent, his passion, his wild and confused state that always comes to the fore, and concludes with these words:

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> Finally, we note that Cardano treated the sciences in a more naive manner. He always considers them in connection with himself, his personality, and his life. And so his works speak to us with a naturalness and liveliness that attract, inspire, and refresh us, and set us into action. He is not a professor in his gown lecturing us *ex cathedra* but a man who goes this way and that, listens, is amazed, is seized by joy and pain, and forces upon us a passionate account of it all. If we rank him as superior among the renewers of the sciences, then he reached this distinguished position in equal measure through his character and his efforts.

Cardano has probably had a greater effect on mathematics than on the other sciences, thanks to the *Ars magna,* which was republished several times. His character shows itself throughout the book—in the exuberance with which he constructs hundreds of types of equations he can handle, in flashes of genius, such as the first tenuous connections between algebra and the theory of entire rational functions and, blended with this, in such confusion and nonsense as we should hardly expect to find in the work of even the mediocrities among his mathematical contemporaries. Cardano was brilliant but not discerning, productive but lacking constructive power. He had an astounding need to express himself. For example, he published a thick volume on his utterly unsuccessful attempts to treat a particularly difficult class of cubic equations. His feuds with Tartaglia were fought out by his student Ludovico Ferrari, who found the solution of quartic equations but who, strangely enough, like Scipione del Ferro, published nothing about his discovery.

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Later priority arguments often involved national jealousy. Thus in the middle of the 17th century the French mathematicians wrangled with the Italians whom they begrudged certain beautiful geometric discoveries; and various French schools fought one another. Then there is the famous dispute over the authorship of the infinitesimal calculus that raged at the end of the 17th century between the followers of Newton and Leibniz, between England and the European continent. It is hard to believe that as a result of this fight, the English spurned Leibniz's extremely effective method of computation and for 150 years al-

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most completely withdrew from the ranks of productive mathematicians.

The Marquis de L'Hospital, author of the first textbook on the differential calculus (1696), was a particularly congenial representative of the continental school. In the introduction, he expressed his enthusiasm for the new discipline in the following beautiful passage.

> Ordinary analysis treats only the finite magnitudes, but the new analysis advances to the infinite itself. We could even say that it extends beyond the infinite, for it discovers the relationship of (infinitesimal) differences of infinitesimal differences, as well as the differences of third and fourth order, and so on, without ever reaching a limit that would block its progress, and in this way encompasses not only the infinite, but the infinite of the infinite, or an infinity of infinite things.

I believe that in the 17th and 18th centuries there was hardly an educated layman who had doubts about the fertility of mathematics. But even today we witness the natural pleasure of producing that mathematics affords precisely to the non-professional. Young and old, students, and especially adults living in relative isolation—country clergymen, small-town teachers, foresters—busy themselves with famous ancient problems, such as the trisection of an angle, the quadrature of a circle, the mysterious properties of whole numbers. The latter are probably the oldest playground of mathematics. Here a few results were found in the distant past, and here, to this very day, the most accomplished mathematicians battle to achieve progress in problems that are as easy to state as they are difficult to master.

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Of course the creative power of the mathematician is not always restricted to his discipline. We saw this in Cardano's case. In some cases—for example, Bernhard Riemann—it takes the form of metaphysical speculations; in other cases it spurs revolutionary activity. Thus Evariste Galois, the astounding 19th-century mathematical genius, who conquered a new realm of knowledge at 21 and then died in a ridiculous duel, was very active in the "July revolution" and the subsequent disturbances. Moreover the early maturing of genius is characteristic of mathematicians and connected with their productivity. Pascal in the 17th, Clairaut in the 18th, and Abel in the 19th centuries are particularly well-known examples. Gauss tells us that all his life he exploited the ideas of his youth.

Another property of the mathematician that is familiar to most laymen is the rigor he insists on in his arguments. To be sure, extreme rigor has not always characterized mathematics. In the preclassical period of Greek mathematics speculation must have been quite reckless; and when Greek mathematics declined, its rigor gradually disappeared. And when—between the end of the 16th and the beginning of the 17th centuries—mathematics flourished once more, it was the very lack of rigor that furthered its development. Ignoring the canon of the great Greeks, the discoverers surged ahead. (Naturally, some of the discoveries were false!) Eventually, the intensification of rigor began. At first it was restricted to a few isolated spots, but it came very much to the fore in the 19th century, and is not yet concluded today.

The Greeks owe the rigorous development of their science to a discovery which, so to say, obliged to be rigorous. They discovered the existence of irrational ratios of segments; they realized that the side and diagonal of a square have no common measure, that is, there is no segment of which these two lengths are whole multiples. In other words, they found ratios of lengths that could not be expressed by common fractions. Thus their earlier carefree practice—calculating with ratios of segments as with ordinary fractions—was an unconscious walk along the edge of a precipice. Why should the rules for computing with whole numbers or fractions that admit of simple direct derivation hold for irrational, "inexpressible" ratios? That $3 \cdot 4 = 4 \cdot 3$ and, more generally, $m \cdot n = n \cdot m$, is easy to perceive by looking at a table of four rows of three points each, and so on. From this it is easy to derive the result that the product of fractions does not depend on the order of the factors. But why should this hold for products of irrational ratios of segments that cannot be obtained by the process of subdivision of a whole number into equal parts? To overcome this great difficulty the Greeks erected a remarkable structure, contained in the fifth book of Euclid's *Elements*, a structure whose stability we cannot fault to this very day. The foundation stone of this structure—as well as any other rigorous theory that proposes to harmonize arithmetic and geometric phenomena—is the following theorem, which we state in Archimedes' formulation: Given two different magnitudes, or, more specifically, segments, one can multiply their difference so many times that it becomes greater than any third given magnitude or segment. The Greeks, most explicitly Archimedes, clearly recognized the importance and uniqueness of this basic theorem (also referred to as the exhaustion principle)—it does not derive directly from intuition, and its transcendental character becomes obvious upon closer examination—and they took great pains to use it in their syllogisms. They realized that without it they could not derive their most beautiful results, such as the philosopher Democritus' insight that the volume of a pyramid is one third of the volume of a parallelepiped with the same base and height, or Archimedes' splendid discovery that the surface area of a sphere is four times the area of its equatorial cross section. And so they included this basic theorem in their canon.

Almost all of the great discoverers of the 17th century—in particular, Kepler, Cavalieri and Leibniz—argued naively, much as the Greek mathematicians had argued before the fall, that is, before they had plucked the fateful fruit from the tree of knowledge and tasted it. In the 18th century no one respected Greek rigor. The new race of titans found the rigorous rules of the ancients too particular and inhibiting. One might say that productivity defeated strict propriety. For modern mathematicians, the existence of irrational numbers was not an intense personal experience. They knew them from their elementary education, and so this knowledge could not bring them to soul-searching. It was only at the end of the 18th century that progress in the theory of functions revealed more complicated phenomena that demonstrated the consequences of recklessness. Thanks to a number of mathematicians, Greek rigor gradually became respectable again, especially in connection with attempts to provide an independent logical basis for a part of geometry that blossomed anew, namely, projective geometry. And so it happened that in the first development of projective geometry by the still living Giessen mathematician Pasch, (a development rigorous by today's standards), we find once more the old Greek basic theorem of exhaustion applied to segments. In some respects, Pasch's development of geometry—as far as it goes—is more rigorous than the development of the corresponding parts of geometry by the ancients, for Pasch decided to spell out all the intuitive assumptions. To some extent, his system of axioms is complete, whereas the Greeks used the intuitively obvious freely and without acknowledgement, and only spelled out ideas at the boundary of the intuitively given. Experience with irrational number taught the Greeks that in mathematics one must not rely uncritically on one's feelings, that it is dangerous to generalize formally, for connections in the more general and non-intuitive take us back to relations in the intuitive and the particular. The false method of merely formal generalization has been used repeatedly through the centuries. We come across it even today in decisive places in many textbooks.

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While we must admit that mathematics has not always met reasonable standards of rigor, we must also concede that the layman is entirely right when he thinks that mathematical knowledge is more securely based than all other knowledge. By and large, results remain true regardless of whether they were deduced two thousand years ago or yesterday. Only isolated general assertions, especially those in the most advanced or abstract parts of mathematics, or listings of all possible cases of a certain phenomenon, must sometimes be revised. The more particular the result, the smaller the likelihood that it is false, or that its incorrectness will not be immediately discovered. Most results are so involved in the general web of theorems, they can be reached in so many ways, that their incorrectness is simply unthinkable. This is the characteristic difference between mathematics and all systems of knowledge that arose before it, or even all compilations of knowledge that do not derive directly from Greek thought.

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This singular feature of his discipline must, of course, influence the character of the mathematician. If he is to be successful in his work the mathematician must be extremely scrupulous. Some carry over the lessons learned in their discipline to other areas. They demand absolute certainty in other areas of knowledge and tend to look down upon them if they do not find it. This attitude is quite common among those who have just begun to devote themselves to mathematical work. Most mathematicians find it especially difficult to come to terms with the thought-constructions of the philosophers. The structure of knowledge in these two disciplines is so different that the mathematician arrives quickly at the conclusion that the philosopher works exclusively with magic incantations, while the philosopher thinks the mathematician superficial and simplistic. Other mathematicians use their acquired dialectical skill—their experience in the construction of mutually exclusive categories—to regard as possible completely paradoxical solutions of problems in other sciences or in daily life. In the latter case they fail to grasp the differently-oriented logic of things social, of the life of men with one another.

For example, involvement with mathematics was dangerous for Descartes, and possibly for his effect upon philosophy. His successes in geometry made him somewhat presumptuous. He had the justified feeling that he had achieved something truly great by means of his penetrating reason. Blinded by this, he believed that he could comprehend the whole world by means of pure reason, that he could, so to say, explain it mathematically. Descartes' delusions may well have had an unfortunate effect on the views of subsequent philosophers.

Mathematics is the only instructional material that can be presented in an entirely undogmatic way.

There is no doubt that mathematics exerts a strong influence on anyone who occupies himself with it—that it shapes his character in a unique way. Mathematics is the only instructional material that can be presented in an entirely undogmatic way. That is why instruction in mathematics has played a prominent role in institutions of higher learning since antiquity. Taught properly, mathematics enables the student to think clearly and independently within the limits of his aptitude. He can certainly take complete responsibility for his mathematical work.

Goethe once said that mathematicians have no conscience. He probably meant that they need no conscience, for they build on a solid foundation and cannot possibly be led into temptation. But he overestimates mathematicians or mathematics. Very recently, we have again seen that we too can stumble. The taste for generalization, for ever more comprehensive concepts, of which we spoke above, has made us construct things that are very far removed from common intuition, such as the general concepts of set theory set up by Cantor around 1880. It turned out that, while it is possible to obtain beautiful results in this new realm of pure thought, these constructions lead to contradictions—the so-called paradoxes of set theory to which much attention is also being paid in the philosophical literature. Much as in old Greece after the discovery of the irrationals, or at the beginning of the 19th century after the refinement of the infinitesimal calculus, there has arisen a critical movement. I shall try to describe—if only schematically—the results obtained so far by this criticism.

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In the last third of the past century it was thought—largely on the basis of the extraordinary achievements of the Berlin mathematician Weiertrass—that analysis had been given an independent and unassailable foundation, and that all constructions arise on this foundation from simple logical arguments. The Weierstrass school took little interest in geometry, but at the time there appeared the book of Pasch, that achieved for a large part of geometry the same sort of justification that—largely through the efforts of Weierstrass—had been achieved in analysis. The present critique of Weierstrass' viewpoint is the following: (1) The natural foundation of analysis is geometry. In fact, the analytic derivation of the simplest concepts of analysis—for example, the proper fractions—from the whole numbers is artificial. (2) Arguments in analysis are not based on simple logic alone. In fact, it is precisely in analysis that we require complete induction—inference from n to $n + 1$—for every theorem. The ancients did not formulate this procedure; to some extent they used it unconsciously. Actually, its flawless formulation occasions great difficulties. One also used in analysis modes of deduction that failed to be generally accepted after they had been fully understood. Here I have in mind, in the first instance, the so-called axiom of choice, first clearly formulated by Zermelo in 1904. (3) In order to attain the alleged absolute certainty it is necessary to establish the incontrovertibility of the assumptions. In fact, as we mentioned above, there are contradictions in set theory that, unfortunately, have not been satisfactorily resolved. Thus there is no doubt that it is necessary to develop the foundations of analysis with greater care. But even if this task is accomplished satisfactorily, no modern mathematician will want to claim that no contradictions will ever arise on this new foundation. It was primarily Hilbert who clearly formulated and tackled the problem of giving a direct proof of consistency. A complete resolution of the issue is very unlikely for a variety of reasons. I myself think it may be that the reason for this difficult state of affairs is that in the case of very general conceptual constructs there arise contradictions between geometry—the world of extensive magnitudes—and the world of counting, and that analysis—the bridge between the two—will surely remain completely free of contradictions as long as it is intuitive, that is, as its concepts are at the same time intuitive geometric concepts. In this view, the true foundation of mathematics is not the object of pure thought in Plato's beautiful belief—the γοητόν that, in his view, must under no circumstances be confused with perception the αἴσφησιζ—but also the ordinary, if somewhat refined, visualization that the creator of the realm of ideas was somewhat contemptuous of. Earlier, one regarded all of the mathematics—the basic theorems and the mode of reasoning—as logically necessary; mathematics was unique. Since the beginning of the 19th century it has become more and more apparent that different systems can be visualized, that, say, different types of spaces are compatible with experience. This is not to imply that the mathematician can now choose his assumptions at will. Not only is such arbitrariness likely to result in developments without beauty, but it is also likely to lead to contradictions that make all of the work an illusion.

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This somewhat skeptical attitude of some modern mathematicians is reinforced by what is happening in the neighboring area of physics. It seems that it is no longer possible to consistently interpret physical phenomena in a mathematical 4-dimensional space-time continuum. Until now we have been able to supply physics with somewhat loosely built scaffoldings for its ever bolder constructions. It may now be about to emancipate itself from mathematics in its important investigations on the finest structure of matter.

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All this inclines some of us to greater scepticism in more general questions as well. The fundamental belief of every philosopher that the world can be consistently comprehended by human reason is now open to doubt. Beyond the *ignorabimus,* he no longer strongly believes in man's ability to bring together different insights in a satisfying harmony. To be sure, this is not an entirely original attitude. It reminds one of the view taken by the late Eleatic philosophers at the time of the foundational crisis in ancient Greece.

This scepticism gives rise to certain resignation, a kind of distrust in man's intellectual power. But quite apart from the modern developments just described, this attitude has always characterized the greatest mathematicians. For even the greatest researchers learn the sad truth that while they can discern an infinity of new and beautiful problems, they can only tackle the least of them. Newton put it in these words:

> I do not know what I may appear to the world; but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.

I should like further to comment on why it is that productive involvement must lead to just such resignation. To this end, we must look at the growth of the individual mathematical disciplines. The destinies of different branches vary greatly. Consider, for example, projective geometry, which may well have been treated for the first time by Euclid in his lost work on porisms, and was extensively developed—for the most part by Pappus of Alexandria—in the third century A.D. It deals with the remarkable and beautiful properties of figures that remain invariant under projection. But in antiquity the problem was not formulated in full generality. After the Renaissance, when interest in projective geometry was inspired by the use of perspective in painting, the problem was first treated in typically modern fashion by the 17th century French geometer Desargues (an architect by profession). Projective geometry continued to flourish and—as a result of a number of brilliant works by French, and later also German mathematicians—was brought virtually to conclusion in the 19th century: It was built up along canonical lines, and its problems were reduced to algebraic problems over which one had complete mastery. One could almost say that after two thousand years of work the solution of problems in projective geometry was virtually as trivial for a professional mathematician as the solution of problems of counting with whole numbers for a 10-year-old child.

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Now consider another branch of mathematics: number theory, of which I said earlier that its first results date back to an ancient past. A brilliant array of men, such as Euclid, Diophantus, Fermat, Euler, Lagrange, Gauss, and Dirichlet, have obtained results that will forever delight all mathematicians. But almost all of these problems are isolated problems, and other very old, simple-looking problems remained unsolved. For example, Euclid investigated perfect numbers—that is, numbers equal to the sum of their divisors, exemplified, say, by $6 = 1 + 2 + 3$ or $28 = 1 + 2 + 4 + 7 + 14$—and proved a remarkable result about such numbers. To this day, in spite of great efforts, this result is virtually all we know about perfect numbers. Whether there are infinitely many such numbers (we know just 12), and whether there exist odd perfect numbers are questions we cannot even tackle.

As the last case we consider analysis situs or topology, the branch of mathematics that deals with the most general properties of the shape of a figure. It was developed only in the 19th century, and largely through the work of the Göttingen mathematician Riemann, who identified the topological core of many function-theoretic questions. At the end of the 19th century Henri Poincaré gave topology another strong impulse. At the present time there appear very many topological papers, but when it comes to fundamental problems we have hardly gone beyond Poincaré or, strictly speaking, Riemann—this in spite of the fact that such progress would be of great significance for, among other things, the theory of algebraic functions of two variables. Here the failure is not due to the fact that—as in number theory—the problems cannot be tackled, but to the fact that they are so intricate that the power of the human intellect, the ability to imagine different things at the same time, is not sufficient for mastery.

I could consider many disciplines from this viewpoint but I think that the above examples illustrate the three most important types: the discipline that reduces to triviality; the one that is forever hindered in its development and beset on all sides with

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seemingly unassailable problems; and the one that—after a longer or shorter period of development—is brought to a halt by the complexity and difficulty of its problems. The last type inspires the feeling that mathematics develops like a tree. There is a limit to the growth of a tree, for the ability of the tree to transport nourishing substances from the earth to the crown does not extend to arbitrary heights. Similarly the continued development of a branch of mathematics require connection with the ground of intuition, and man's limited intellectual power sets a boundary on the distance between abstraction and intuition. No development is possible beyond this bound. But modern mathematics is certainly not dead, and someone may, and (we hope) will, so simplify processes in, say, topology—that is, provide so much shorter a connection with the ground—that a new development will begin. This is the case, for example, in projective geometry, which could not advance further by means of the methods and ideas of antiquity, but entered an era of new development through the brilliant ideas of Desargues and, later, the mathematical power of the geometers of the first half of the 19th century, before reaching the conclusion described above. Again, hitherto unassailable problems of number theory are

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being solved as a result of the discovery of new connections. We must not be hampered by resignation. On the contrary, we must realize that the progress of science depends not on comfortable plodding but on perceiving and forming new ideas. The progress of our discipline depends not on mass efforts, not on a flood of papers filled with investigations of insignificant special cases or generalizations, but on individual creative achievements. Such achievements can hardly come about in a factory-like setting. But if the mathematician complains that modern development has organized even the pursuit of his own science, then he must, above all, tell himself: *mea maxima culpa*. For it is through mathematics that man's constructive power, that brought forth the age of technology, first blossomed. And when he is gripped by despair at the sight of the evil he has brought about he is saved, for the third time, by resignation. He is aware of the power of man's thought that can penetrate to the limits of the macrocosm and microcosm and back into the depths of time; but he also knows that this very same human thought is powerless to shape fate, that it is just a small force in a wondrously contorted development, a force that works blindly and has no access to the riddles of the future. Such humility induces profound religiosity in many mathematicians. In spite of the boldness and acuteness of their speculations, it was especially marked in Pascal, Newton, Euler, Cauchy, Gauss, and Riemann. The trait of modesty is also found in Euclid, the oldest mathematician of whose character we have a description. It is said of Euclid that he was distinguished by mildness and good will to all, and especially to those who could in any way enrich mathematics; and though he might be stern, he was not in the least quarrelsome, and had no desire to claim anything for himself.

I wish to say that, contrary to a widespread notion, the mathematician is not an otherworldly eccentric; at any rate, he is not eccentric because of his science. He stands between areas of study, especially between the

... contrary to a widespread notion, the mathematician is not an otherworldly eccentric; at any rate; he is not eccentric because of his science.

humanities and the natural sciences, spheres that unfortunately are disjoint in our country. His method is only a particularly distinct version of the general scientific method. In view of the importance of the principle of the excluded middle, it is related to the juridical method. The object of his research is more spiritual than that of the natural scientist and more sentient than that of the humanist. He is linked to the latter by the whole history of his science, and this has intimate connections with the history of philosophy. Connection with the natural sciences goes beyond the applications that permeate all the exact natural sciences. The mathematician knows that he owes to the natural sciences his most important stimulations. Thus the method of infinitesimals that so inspired L'Hospital, and became a pillar of the infinitesimal calculus, arose out of Galileo's arduous research on the decomposition of the motion of a projectile into motions due to inertia and gravity respectively. Or we remember periodic series, rightly named for the physicist Fourier and directly related to all the important extensions of analysis in the 19th century.

We have now reached the end. I dare not decide whether the topic of my lecture was correctly chosen, that is, whether the mathematician has a certain spiritual uniqueness or should be included as a mere variety without essential distinction in a much larger class. At any rate, let me conclude with a summary of the image obtained in the course of our considerations. At times the mathematician has the passion of a poet or a conqueror, the rigor of his arguments is that of a responsible statesman or, more simply, of a concerned father, and his tolerance and resignation are those of an old sage; he is revolutionary and conservative, sceptical and yet faithfully optimistic. These qualities do sometimes appear together in one person, but if you find them somewhat contradictory remember what C. F. Mayer makes Ulrich von Hutten say, and what the mathematician may claim for himself:

Ich bin kein ausgeklügelt Buch,
Ich bin ein Mensch mit seinem Widerpruch.

(I am not a contrived book, but a human being with all its contradictions).

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Smiles

Edited by Gary Cornell

Erdös once remarked that every theorem has at least one "right" proof. This proof would be sufficiently elegant so as to strike some responsive chord, while remaining short enough to be easily comprehensible. It might even force a smile. This column will try to present such proofs. It will appear irregularly; as often as material presents itself.

Anyone knowing a proof of a result that fulfills the above (admittedly vague) criteria is urged to submit it to:

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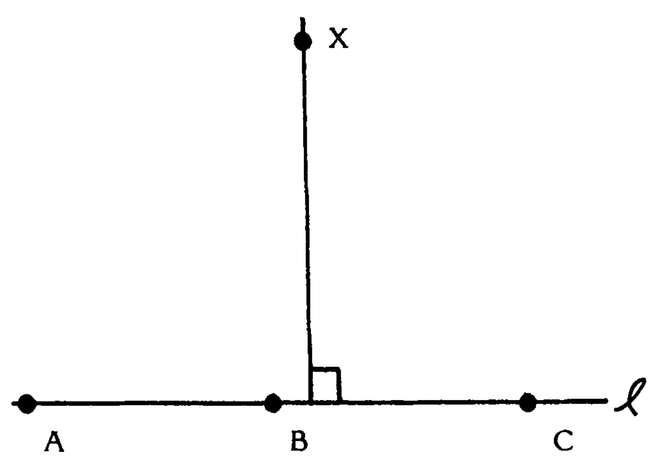
Credit for the following proof is uncertain—it is possibly due to Erdos. This particular version was transmitted to *The Intelligencer* by Marty Isaacs.

THM (Sylvester). Let S be a finite set of points in a plane such that every line which meets two points of S, meets a third point. Then S is colinear.

Proof. If not, let L be the set of all lines determined by pairs of points of S. Some point of S is not on some line of L so choose $X \in S$ and $\ell \in L$ with X not on ℓ and the distance from X to ℓ as small as possible. (By finiteness of S and L.)



Now ℓ contains two, hence three points of S, say A, B, C. Two of these must lie on the same side of the foot of the perpendicular from X to ℓ. (A and B in the diagram. We allow one of these to *be* the foot of the perpendicular.) Now in the diagram, the distance from point B to line AX is less than the distance from X to ℓ. This is a contradiction.