Paul Halmos: In His Own Words

John Ewing

Paul Halmos died on October 2, 2006, at the age of 90. After his death, many people wrote about his career and praised both his mathematical and his expository skills. Paul would have complained about that: He often said he could smell great mathematicians, and he himself was not one of them.

But he was wrong. He was a master of mathematics in multiple ways, and he influenced mathematicians and mathematical culture throughout his career. Unlike most other master mathematicians, Paul's legacy was not merely mathematics but rather advice and opinion about mathematical life—writing, publishing, speaking, research, or even thinking about mathematics. Paul wrote about each of these topics with an extraordinary mixture of conviction and humility. Mathematicians paid attention to what he wrote, and they often quoted it (and still do—"every talk ought to have one proof"). They disagreed and frequently wrote rebuttals. They passed along his wisdom to their students, who passed it along to theirs. Paul Halmos's writing affected the professional lives of nearly every mathematician in the latter half of the twentieth century, and it will continue to influence the profession for years to come.

How does one write about great writing? Explanations of great exposition always fall flat, like analyses of great poems or elucidations of famous paintings. Art is best exhibited, not explained.

And so here is a collection of excerpts from the writing of Paul Halmos, giving advice, offering opinions, or merely contemplating life as a mathematician—all in his own words.

—J. E.

On Writing

Excerpts from:

"How to write mathematics", *Enseign. Math.* (2) 16 (1970), 123–152.

…I think I can tell someone how to write, but I can't think who would want to listen. The ability to communicate effectively, the power to be intelligible, is congenital, I believe, or, in any event, it is so early acquired that by the time someone reads my wisdom on the subject he is likely to be invariant under it. To understand a syllogism is not something you can learn; you are either born with the ability or you are not. In the same way, effective exposition is not a teachable art; some can do it and some cannot. There is no usable recipe for good writing.

Then why go on? A small reason is the hope that what I said isn't quite right; and, anyway, I'd like a chance to try to do what perhaps cannot be done. A more practical reason is that in the other arts that require innate talent, even the gifted ones who are born with it are not usually born with full knowledge of all the tricks of the trade. A few essays such as this may serve to "remind" (in the sense of Plato) the ones who want to be and are destined to be the expositors of the future of the techniques found useful by the expositors of the past.

The basic problem in writing mathematics is the same as in writing biology, writing a novel, or writing directions for assembling a harpsichord: the problem is to communicate an idea. To do so, and to do it clearly, you must have something to say, and you must have someone to say it to, you must organize what you want to say, and you must arrange it in the order you want it said in, you must write it, rewrite it, and re-rewrite it several times, and you must be willing to think hard about and work hard on mechanical details such as diction, notation, and punctuation. That's all there is to it.…

It might seem unnecessary to insist that in order to say something well you must have something to say, but it's no joke. Much bad writing, mathematical and otherwise, is caused by a violation of that first principle. Just as there are two ways for a sequence not to have a limit (no cluster points or

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too many), there are two ways for a piece of writing not to have a subject (no ideas or too many).

The first disease is the harder one to catch. It is hard to write many words about nothing, especially in mathematics, but it can be done, and the result is bound to be hard to read. There is a classic crank book by Carl Theodore Heisel [*The Circle Squared Beyond Refutation*, Heisel, Cleveland, 1934] that serves as an example. It is full of correctly spelled words strung together in grammatical sentences, but after three decades of looking at it every now and then I still cannot read two consecutive pages and make a one-paragraph abstract of what they say; the reason is, I think, that they don't say anything.

The second disease is very common: there are many books that violate the principle of having something to say by trying to say too many things. …

The second principle of good writing is to write for someone. When you decide to write something, ask yourself who it is that you want to reach. Are you writing a diary note to be read by yourself only, a letter to a friend, a research announcement for specialists, or a textbook for undergraduates? The problems are much the same in any case; what varies is the amount of motivation you need to put in, the extent of informality you may allow yourself, the fussiness of the detail that is necessary, and the number of times things have to be repeated. All writing is influenced by the audience, but, given the audience, the author's problem is to communicate with it as best he can.…

Everything I've said so far has to do with writing in the large, global sense; it is time to turn to the local aspects of the subject.

The English language can be a beautiful and powerful instrument for interesting, clear, and completely precise information, and I have faith that the same is true for French or Japanese or Russian. It is just as important for an expositor to familiarize himself with that instrument as for a surgeon to know his tools. Euclid can be explained in bad grammar and bad diction, and a vermiform appendix can be removed with a rusty pocket knife, but the victim, even if he is unconscious of the reason for his discomfort, would surely prefer better treatment than that.…

My advice about the use of words can be summed up as follows. (1) Avoid technical terms, and especially the creation of new ones, whenever possible. (2) Think hard about the new ones that you must create; consult Roget; and make them as appropriate as possible. (3) Use the old ones correctly and consistently, but with a minimum of obtrusive pedantry.…

Everything said about words, applies, mutatis mutandis, to the even smaller units of mathematical writing, the mathematical symbols. The best notation is no notation; whenever possible to avoid the use of a complicated alphabetic apparatus, avoid it. A good attitude to the preparation of written mathematical exposition is to pretend that it is spoken. Pretend that you are explaining the subject to a friend on a long walk in the woods, with no paper available; fall back on symbolism only when it is really necessary.

On Speaking

Excerpts from:
"How to talk mathematics", *Notices of AMS* 21 (1974), 155–158.

What is the purpose of a public lecture? Answer: to attract and to inform. We like what we do, and we should like for others to like it too; and we believe that the subject's intrinsic qualities are good enough so that anyone who knows what they are cannot help being attracted to them. Hence, better answer: the purpose of a public lecture is to inform, but to do so in a manner that makes it possible for the audience to absorb the information. An attractive presentation with no content is worthless, to be sure, but a lump of indigestible information is worth no more.…

Less is more, said the great architect Mies van der Rohe, and if all lecturers remember that adage, all audiences would be both wiser and happier.

Have you ever disliked a lecture because it was too elementary? I am sure that there *are* people who would answer yes to that question, but not many. Every time I have asked the question, the person who answered said no, and then looked a little surprised at hearing the answer. A public lecture should be simple and elementary; it should not be complicated and technical. If you believe and can act on this injunction ("be simple"), you can stop reading here; the rest of what I have to say is, in comparison, just a matter of minor detail.

To begin a public lecture to 500 people with "Consider a sheaf of germs of holomorphic functions…" (I have heard it happen) loses people and antagonizes them. If you mention the Künneth formula, it does no harm to say that, at least as far as Betti numbers go, it is just what happens when you multiply polynomials. If you mention functors, say that a typical example is the formation of the duals of vector spaces and the adjoints of linear transformations.

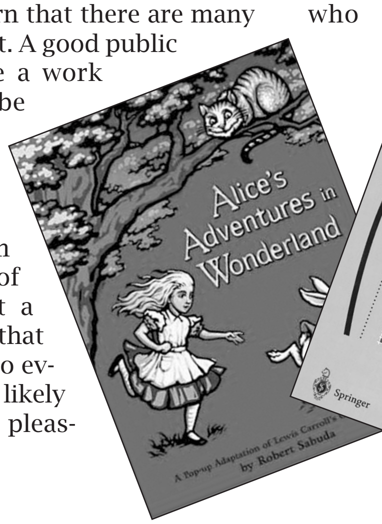
Be simple by being concrete. Listeners are prepared to accept unstated (but hinted) generalizations much more than they are able, on the spur of the moment, to decode a precisely stated abstraction and to re-invent the special cases that motivated it in the first place. Caution: being concrete should not lead to concentrating on the trees and missing the woods. In many parts of mathematics a generalization is simpler and more incisive than its special parent. (Examples: Artin's solution of Hilbert's 17th problem about definite forms via formally real fields; Gelfand's proof of

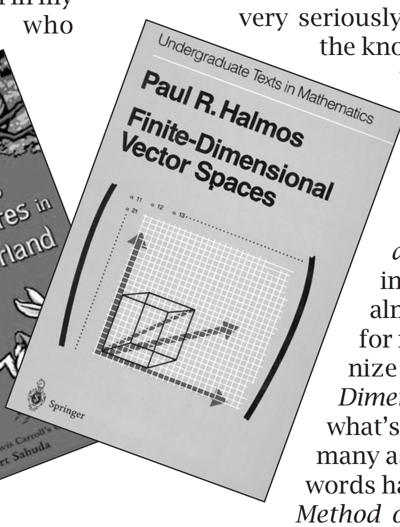
Wiener's theorem about absolutely convergent Fourier series via Banach algebras.) In such cases there is always a concrete special case that is simpler than the seminal one and that illustrates the generalization with less fuss; the lecturer who knows his subject will explain the complicated special case, and the generalization, by discussing the simple cousin.

Some lecturers defend complications and technicalities by saying that that's what *their* subject is like, and there is nothing they can do about it. I am skeptical, and I am willing to go so far as to say that such statements indicate incomplete understanding of the subject and of its place in mathematics. Every subject, and even every small part of a subject, if it is identifiable, if it is big enough to give an hour talk on, has its simple aspects, and they, the simple aspects, the roots of the subject, the connections with more widely known and older parts of mathematics, are what a non-specialized audience needs to be told.

Many lecturers, especially those near the foot of the academic ladder, anxious to climb rapidly, feel under pressure to say something brand new—to impress their elders with their brilliance and profundity. Two comments: (1) the best way to do that is to make the talk simple, and (2) it doesn't really have to be done. It may be entirely appropriate to make the lecturer's recent research the focal point of the lecture, but it may also be entirely appropriate not to do so. An audience's evaluation of the merits of a talk is not proportional to the amount of original material included; the explanation of the speaker's latest theorem may fail to improve his chance of creating a good impression.

An oft-quoted compromise between trying to be intelligible and trying to seem deep is this advice: address the first quarter of your talk to your high-school chemistry teacher, the second to a graduate student, the third to an educated mathematician whose interests are different from yours, and the last to the specialists. I have done my duty by reporting the formula, but I'd fail in my duty if I didn't warn that there are many who do not agree with it. A good public lecture should be a work of art. It should be an architectural unit whose parts reinforce each other in conveying the maximum possible amount of information—not a campaign speech that offers something to everybody, and more likely than not, ends by pleasing nobody.





Make It Simple, and You Won't Go Wrong.…

Excerpt from:

***I Want to Be a Mathematician*, p. 401, Springer-Verlag, New York (1985).**

…As for working hard, I got my first hint of what that means when Carmichael told me how long it took him to prepare a fifty-minute invited address. Fifty hours, he said: an hour of work for each minute of the final presentation. When many years later, six of us wrote our "history" paper ("American mathematics from 1940…"), I calculated that my share of the work took about 150 hours; I shudder to think how many man-hours the whole group put in. A few of my hours went toward preparing the lecture (as opposed to the paper). I talked it, the whole thing, out loud, and then, I talked it again, the whole thing, into a dictaphone. Then I listened to it, from beginning to end, six times—three times for spots that needed polishing (and which I polished before the next time), and three more times to get the timing right (and, in particular, to get the feel for the timing of each part.) Once all that was behind me, and I had prepared the transparencies, I talked the whole thing through one final rehearsal time (by myself—no audience). That's work.…

On Exposition

Excerpt from:

Response from Paul Halmos on winning the Steele Prize for Exposition (1983).

Not long ago I ran across a reference to a publication titled *A Method of Taking Votes on More Than Two Issues*. Do you know, or could you guess, who the author is? What about an article titled "On automorphisms of compact groups"? Who wrote that one? The answer to the first question is C. L. Dodgson, better known as Lewis Carroll, and the answer to the second question is Paul Halmos.

Lewis Carroll and I have in common that we both called ourselves mathematicians, that we both strove to do research, and that we both took very seriously our attempts to enlarge the known body of mathematical truths. To earn his living, Lewis Carroll was a teacher, and, just for fun, because he loved to tell stories, he wrote *Alice's Adventures in Wonderland*. To earn my living, I've been a teacher for almost fifty years, and, just for fun, because I love to organize and clarify, I wrote *Finite Dimensional Vector Spaces*. And what's the outcome? I doubt if as many as a dozen readers of these words have ever looked at either *A Method of Taking Votes…* or "On automorphisms…" but Lewis Carroll is immortal for the Alice stories, and I got the Steele Prize for exposition. I don't know what the Reverend Mr. C. L. Dodgson thought about his fame, but, as for me, I was brought up with the Puritan ethic: if something is fun, then you shouldn't get recognized and rewarded for doing it. As a result, while, to be sure, I am proud and happy, at the same time I can't help feeling just a little worried and guilty.

I enjoy studying, learning, coming to understand, and then explaining, but it doesn't follow that communicating what I know is always easy; it can be devilishly hard. To explain something you must know not only what to put in, but also what to leave out; you must know when to tell the whole truth and when to get the right idea across by telling a little white fib. The difficulty in exposition is not the style, the choice of words—it is the structure, the organization. The words are important, yes, but the arrangement of the material, the indication of the connections of its parts with each other and with other parts of mathematics, the proper emphasis that shows what's easy and what deserves to be treated with caution—these things are much more important. …

On Publishing

Excerpts from:

"Four panel talks on publishing", ***American Mathematical Monthly*** **82 (1975), 14–17.**

…Let me remind you that most laws (with the exception only of the regulatory statutes that govern traffic and taxes) are negative. Consider, as an example, the Ten Commandments. When Moses came back from Mount Sinai, he told us what to be by telling us, eight out of ten times, what not to do. It may therefore be considered appropriate to say what not to publish. I warn you in advance that all the principles that I was able to distill from interviews and from introspection, and that I'll now tell you about, are a little false. Counterexamples can be found to each one—but as directional guides the principles still serve a useful purpose.

First, then, do not publish fruitless speculations: do not publish polemics and diatribes against a friend's error. Do not publish the detailed working out of a known principle. (Gauss discovered exactly which regular polygons are ruler-and-compass constructible, and he proved, in particular, that the one with 65537 sides—a Fermat prime—is constructible; please do not publish the details of the procedure. It's been tried.)

Do not publish in 1975 the case of dimension 2 of an interesting conjecture in algebraic geometry, one that you don't know how to settle in general, and then follow it by dimension 3 in 1976, dimension 4 in 1977, and so on, with dimension $k-3$ in 197k. Do not, more generally, publish your failures: I tried to prove so-and-so; I couldn't; here it is—see?!

Adrian Albert used to say that a theory is worth studying if it has at least three distinct good hard examples. Do not therefore define and study a new class of functions, the ones that possess left upper bimeasurably approximate derivatives, unless you can, at the very least, fulfill the good graduate student's immediate request: show me some that do and show me some that don't.

A striking criterion for how to decide not to publish something was offered by my colleague John Conway. Suppose that you have just finished typing a paper. Suppose now that I come to you, horns, cloven hooves, forked tail and all, and ask: if I gave you $1,000.00, would you tear the paper up and forget it? If you hesitate, your paper is lost—do not publish it. That's part of a more general rule: when in doubt, let the answer be no.…

On Research

Excerpt from:

***I Want to Be a Mathematician*, pp. 321–322, Springer-Verlag, New York (1985).**

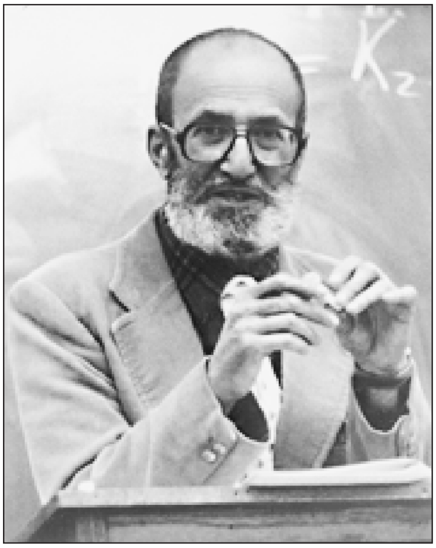
Can anyone tell anyone else how to do research, how to be creative, how to discover something new? Almost certainly not. I have been trying for a long time to learn mathematics, to understand it, to find the truth, to prove a theorem, to solve a problem—and now I am going to try to describe just how I went about it. The important part of the process is mental, and that is indescribable—but I can at least take a stab at the physical part.

Mathematics is not a deductive science—that's a cliché. When you try to prove a theorem, you don't just list the hypotheses, and then start to reason. What you do is trial and error, experimentation, guesswork. You want to find out what the facts are, and what you do is in that respect similar to what a laboratory technician does, but it is different in the degree of precision and information. Possibly philosophers would look on us mathematicians the same way we look on the technicians, if they dared.

I love to do research, I want to do research, I have to do research, and I hate to sit down and begin to do research—I always try to put it off just as long as I can.

It is important to me to have something big and external, not inside myself, that I can devote my life to. Gauss and Goya and Shakespeare and Paganini are excellent, their excellence gives me pleasure, and I admire and envy them. They were also dedicated human beings. Excellence is for the few but dedication is something everybody can have—and should have—and without it life is not worth living.

Despite my great emotional involvement in work, I just hate to start doing it; it's a battle and a wrench every time. Isn't there something I can (must?) do first? Shouldn't I sharpen my pencils, perhaps? In fact I never use pencils, but pencil sharpening has


From the Paul Halmos Collection.

Paul Halmos

become the code phrase for anything that helps to postpone the pain of concentrated creative attention. It stands for reference searching in the library, systematizing old notes, or even preparing tomorrow's class lecture, with the excuse that once those things are out of the way I'll really be able to concentrate without interruption.

When Carmichael complained that as dean he didn't have more than 20 hours a week for research I marveled, and I marvel still. During my productive years I probably averaged 20 hours of concentrated mathematical thinking a week, but much more than that was extremely rare. The rare exception came, two or three times in my life, when long ladders of thought were approaching their climax. Even though I never was dean of a graduate school, I seemed to have psychic energy for only three or four hours of work, "real work", each day; the rest of the time I wrote, taught, reviewed, conferred, refereed, lectured, edited, traveled, and generally sharpened pencils all the ways I could think of. Everybody who does research runs into fallow periods. During mine the other professional activities, down to and including teaching trigonometry, served as a sort of excuse for living. Yes, yes. I may not have proved any new theorems today, but at least I explained the law of sines pretty well, and I have earned my keep.

Why do mathematicians do research? There are several answers. The one I like best is that we are curious—we need to know. That is almost the same as "because we want to," and I accept that—that's a good answer too. There are, however, more answers, ones that are more practical.

On Teaching

Excerpt from:

"The problem of learning to teach", ***American Mathematical Monthly*** **82 (1975), 466–476.**

The best way to learn is to do; the worst way to teach is to talk.

About the latter: did you ever notice that some of the best teachers of the world are the worst lecturers? (I can prove that, but I'd rather not lose quite so many friends.) And, the other way around, did you ever notice that good lecturers are not necessarily good teachers? A good lecture is usually systematic, complete, precise—and dull; it is a bad teaching instrument. When given by such legendary outstanding speakers as Emil Artin and John von Neumann, even a lecture can be a useful tool—their charisma and enthusiasm come through enough to inspire the listener to go forth and do something—it looks like such fun. For most ordinary mortals, however, who are not so bad at lecturing as Wiener was—not so stimulating!—and not so good as Artin—and not so dramatic!—the lecture is an instrument of last resort for good teaching.

My test for what makes a good teacher is very simple: it is the pragmatic one of judging the performance by the product. If a teacher of graduate students consistently produces Ph.D.'s who are mathematicians and who create high-quality new mathematics, he is a good teacher. If a teacher of calculus consistently produces seniors who turn into outstanding graduate students of mathematics, or into leading engineers, biologists, or economists, he is a good teacher. If a teacher of third-grade "new math" (or old) consistently produces outstanding calculus students, or grocery store check-out clerks, or carpenters, or automobile mechanics, he is a good teacher.

For a student of mathematics to hear someone talk about mathematics does hardly any more good than for a student of swimming to hear someone talk about swimming. You can't learn swimming techniques by having someone tell you where to put your arms and legs; and you can't learn to solve problems by having someone tell you to complete the square or to substitute sin u for y.

Can one learn mathematics by reading it? I am inclined to say no. Reading has an edge over listening because reading is more active—but not much. Reading with pencil and paper on the side is very much better—it is a big step in the right direction. The very best way to read a book, however, with, to be sure, pencil and paper on the side, is to keep the pencil busy on the paper and throw the book away.

Having stated this extreme position, I'll rescind it immediately. I know that it is extreme, and I don't really mean it—but I wanted to be very emphatic about not going along with the view that learning means going to lectures and reading books. If we had longer lives, and bigger brains, and enough dedicated expert teachers to have a student/teacher ratio of 1/1, I'd stick with the extreme views—but we don't. Books and lectures don't do a good job of transplanting the facts and techniques of the past into the bloodstream of the scientist of the future—but we must put up with a second best job in order to save time and money. But, and this is the text of my sermon today, if we rely on lectures and books only, we are doing our students and their students, a grave disservice. ...

Excerpt from:

"The heart of mathematics", *American Mathematical Monthly* 87 (1980), 519–524.

... How can we, the teachers of today, use the problem literature? Our assigned task is to pass on the torch of mathematical knowledge to the technicians, engineers, scientists, humanists, teachers, and, not least, research mathematicians of tomorrow: do problems help?

Yes, they do. The major part of every meaningful life is the solution of problems; a considerable part of the professional life of technicians, engineers, scientists, etc., is the solution of mathematical problems. It is the duty of all teachers, and of teachers of mathematics in particular, to expose their students to problems much more than to facts. It is, perhaps, more satisfying to stride into a classroom and give a polished lecture on the Weierstrass *M*-test than to conduct a fumble-and-blunder session that *ends* in the question: "Is the boundedness assumption of the test necessary for its conclusion?" I maintain, however, that such a fumble session, intended to motivate the student to search for a counterexample, is infinitely more valuable.

I have taught courses whose entire content was problems solved by students (and then presented to the class). The number of theorems that the students in such a course were exposed to was approximately half the number that they could have been exposed to in a series of lectures. In a problem course, however, exposure means the acquiring of an intelligent questioning attitude and of some technique for plugging the leaks that proofs are likely to spring; in a lecture course, exposure sometimes means not much more than learning the name of a theorem, being intimidated by its complicated proof, and worrying about whether it would appear on the examination.

... Many teachers are concerned about the amount of material they must cover in a course. One cynic suggested a formula; since, he said, students on the average remember only about 40% of what you tell them, the thing to do is to cram into each course 250% of what you hope will stick. Glib as that is, it probably would not work.

Problem courses do work. Students who have taken my problem courses were often complimented by their subsequent teachers. The compliments were on their alert attitude, on their ability to get to the heart of the matter quickly, and on their intelligently searching questions that showed that they understood what was happening in class. All this happened on more than one level, in calculus, in linear algebra, in set theory, and, of course, in graduate courses on measure theory and functional analysis.

Why must we cover everything that we hope students will ultimately learn? Even if (to stay with an example already mentioned) we think that the Weierstrass *M*-test is supremely important, and that every mathematics student must know that it exists and must understand how to apply it—even then a course on the pertinent branch of analysis might be better for omitting it. Suppose that there are 40 such important topics that a student must be exposed to in a term. Does it follow that we must give 40 complete lectures and hope that they will all sink in? Might it not be better to give 20 of the topics just a ten-minute mention (the name, the statement, and an indication of one of the directions in which it can be applied), and to treat the other 20 in depth, by student-solved problems, student-constructed counterexamples, and student-discovered applications? I firmly believe that the latter method teaches more and teaches better. Some of the material doesn't get *covered* but a lot of it gets *discovered* (a telling old pun that deserves to be kept alive), and the method thereby opens doors whose very existence might never have been suspected behind a solidly built structure of settled facts. As for the Weierstrass *M*-test, or whatever was given short shrift in class—well, books and journals do exist, and students have been known to read them in a pinch. ...

On Mathematics

Excerpt from:

"Mathematics as a creative art", *American Scientist* 56 (1968), 375–389.

Do you know any mathematicians—and, if you do, do you know anything about what they do with their time? Most people don't. When I get into a conversation with the man next to me in a plane, and he tells me that he is something respectable like a doctor, lawyer, merchant or dean, I am tempted to say that I am in roofing and siding. If I tell him that I am a mathematician, his most likely reply will be that he himself could never balance his check book, and it must be fun to be a whiz at math. If my neighbor is an astronomer, a biologist, a chemist, or any other kind of natural or social scientist, I am, if anything, worse off—this man *thinks* he knows what a mathematician is, and he is probably wrong. He thinks that I spend my time (or should) converting different orders of magnitude, comparing binomial coefficients and powers of 2, or solving equations involving rates of reactions.

C. P. Snow points to and deplores the existence of two cultures; he worries about the physicist whose idea of modern literature is Dickens, and he chides the poet who cannot state the second law of thermodynamics. Mathematicians, in converse with well-meaning, intelligent, and educated laymen (do you mind if I refer to all nonmathematicians as laymen?) are much worse off than physicists in converse with poets. It saddens me that educated people don't even know that my subject exists. There is something that they call mathematics, but they neither know how the professionals use the word, nor can they conceive why anybody should do it. It is, to be sure, possible that an intelligent and otherwise educated person doesn't know that egyptology exists, or haematology, but all you have to tell him is that it does, and he will immediately understand in a rough general way why it should and he will have some empathy with the scholar of the subject who finds it interesting.

Usually when a mathematician lectures, he is a missionary. Whether he is talking over a cup of coffee with a collaborator, lecturing to a graduate class of specialists, teaching a reluctant group of freshman engineers, or addressing a general audience of laymen—he is still preaching and seeking to make converts. He will state theorems and he will discuss proofs and he will hope that when he is done his audience will know more mathematics than they did before. My aim today is different—I am not here to proselytize but to enlighten—I seek not converts but friends. I do not want to teach you what mathematics is, but only *that* it is.

I call my subject mathematics—that's what all my colleagues call it, all over the world—and there, quite possibly, is the beginning of confusion. The word covers two disciplines—many more, in reality, but two, at least two, in the same sense in which Snow speaks of two cultures. In order to have some words with which to refer to the ideas I want to discuss, I offer two temporary and ad hoc neologisms. Mathematics, as the work is customarily used, consists of at least two distinct subjects, and I propose to call them *mathology* and *mathophysics.* Roughly speaking, mathology is what is called pure mathematics, and mathophysics is called applied mathematics, but the qualifiers are not emotionally strong enough to disguise that they qualify the same noun. If the concatenation of syllables I chose here reminds you of other words, no great harm will be done; the rhymes alluded to are not completely accidental. I originally planned to entitle this lecture something like "Mathematics is an art," or "Mathematics is not a science," and "Mathematics is useless," but the more I thought about it the more I realized that I mean that "Mathology is an art," "Mathology is not a science," and "Mathology is useless." When I am through, I hope you will recognize that most of you have known about mathophysics before, only you were probably calling it mathematics; I hope that all of you will recognize the distinction between mathology and mathophysics; and I hope that some of you will be ready to embrace, or at least applaud, or at the very least, recognize mathology as a respectable human endeavor.

In the course of the lecture I'll have to use many analogies (literature, chess, painting), each imperfect by itself, but I hope that in their totality they will serve to delineate what I want delineated. Sometimes in the interest of economy of time, and sometimes doubtless unintentionally, I'll exaggerate; when I'm done, I'll be glad to rescind anything that was inaccurate or that gave offense in any way. ...

Mathematics is abstract thought, mathematics is pure logic, mathematics is creative art. All these statements are wrong, but they are all a little right, and they are all nearer the mark than "mathematics is numbers" or "mathematics is geometric shapes". For the professional pure mathematician, mathematics is the logical dovetailing of a carefully selected sparse set of assumptions with their surprising conclusions via a conceptually elegant proof. Simplicity, intricacy, and above all, logical analysis are the hallmark of mathematics.

The mathematician is interested in extreme cases—in this respect he is like the industrial experimenter who breaks lightbulbs, tears shirts, and bounces cars on ruts. How widely does a reasoning apply, he wants to know, and what happens when it doesn't? What happens when you weaken one of the assumptions, or under what conditions can you strengthen one of the conclusions? It is the perpetual asking of such questions that makes for broader understanding, better technique, and greater elasticity for future problems.

Mathematics—this may surprise or shock you some—is never deductive in its creation. The mathematician at work makes vague guesses, visualizes broad generalizations, and jumps to unwarranted conclusions. He arranges and rearranges his ideas, and he becomes convinced of their truth long before he can write down a logical proof. The conviction is not likely to come early—it usually comes after many attempts, many failures, many discouragements, many false starts. It often happens that months of work result in the proof that the method of attack they were based on cannot possibly work and the process of guessing, visualizing, and conclusion-jumping begins again. A reformulation is needed and—and this too may surprise you—more experimental work is needed. To be sure, by "experimental work" I do not mean test tubes and cyclotrons. I mean thought-experiments. When a mathematician wants to prove a theorem about an infinite-dimensional Hilbert space, he examines its finite-dimensional analogue, he looks in detail at the 2-and 3-dimensional cases, he often tries out a particular numerical case, and he hopes that he will gain thereby an insight that pure definition-juggling has not yielded. The deductive stage, writing the result down, and writing down its rigorous proof are relatively trivial once the real insight arrives; it is more like the draftsman's work, not the architect's. ...

The mathematical fraternity is a little like a self-perpetuating priesthood. The mathematicians of today train the mathematicians of tomorrow and, in effect, decide whom to admit to the priesthood. Most people do not find it easy to join—mathematical talent and genius are apparently exactly as rare as talent and genius in paint and music—but anyone can join, everyone is welcome. The rules are nowhere explicitly formulated, but they are intuitively felt by everyone in the profession. Mistakes are forgiven and so is obscure exposition—the indispensable requisite is mathematical insight. Sloppy thinking, verbosity without content, and polemic have no role, and—this is to me one of the most wonderful aspects of mathematics—they are much easier to spot than in the nonmathematical fields of human endeavor (much easier than, for instance, in literature among the arts, in art criticism among the humanities, and in your favorite abomination among the social sciences).

Although most of mathematical creation is done by one man at a desk, at a blackboard, or taking a walk, or, sometimes, by two men in conversation, mathematics is nevertheless a sociable science. The creator needs stimulation while he is creating and he needs an audience after he has created. Mathematics is a sociable science in the sense that I don't think it can be done by one man on a desert island (except for a very short time), but it is not a mob science, it is not a team science. A theorem is not a pyramid; inspiration has never been known to descend on a committee. A great theorem can no more be obtained by a "project" approach than a great painting: I don't think a team of little Gausses could have obtained the theorem about regular polygons under the leadership of a rear admiral anymore than a team of little Shakespeares could have written Hamlet under such conditions. ...

On Pure and Applied

Excerpt from:

"Applied mathematics is bad mathematics", pp. 9–20, appearing in *Mathematics Tomorrow*, edited by Lynn Steen, Springer-Verlag, New York (1981).

It isn't really (applied mathematics, that is, isn't really bad mathematics), but it's different.

Does that sound as if I had set out to capture your attention, and, having succeeded, decided forthwith to back down and become conciliatory? Nothing of the sort! The "conciliatory" sentence is controversial, believe it or not; lots of people argue, vehemently, that it (meaning applied mathematics) is not different at all, it's all the same as pure mathematics, and anybody who says otherwise is probably a reactionary establishmentarian and certainly wrong.

If you're not a professional mathematician, you may be astonished to learn that (according to some people) there are different kinds of mathematics, and that there is anything in the subject for anyone to get excited about. There are; and there is; and what follows is a fragment of what might be called the pertinent sociology of mathematics: what's the difference between pure and applied, how do mathematicians feel about the rift, and what's likely to happen to it in the centuries to come. ...

The pure and applied distinction is visible in the arts and in the humanities almost as clearly as in the sciences: witness Mozart versus military marches, Rubens versus medical illustrations, or Virgil's *Aeneid* versus Cicero's *Philippics*. Pure literature deals with abstractions such as love and war, and it tells about imaginary examples of them in emotionally stirring language. Pure mathematics deals with abstractions such as the multiplication of numbers and the congruence of triangles, and it reasons about Platonically idealized examples of them with intellectually convincing logic.

There is, to be sure, one sense of the word in which all literature is "applied". Shakespeare's sonnets have to do with the everyday world, and so does Tolstoy's *War and Peace*, and so do Caesar's commentaries on the wars he fought; all start from what human beings see and hear, and all speak of how human beings move and feel. In that same somewhat shallow sense all mathematics is applied. It all starts from sizes and shapes (whose study leads ultimately to algebra and geometry), and it reasons about how sizes and shapes change and interact (and such reasoning leads ultimately to the part of the subject that the professionals call analysis).

There can be no doubt that the fountainhead, the inspiration, of all literature is the physical and social universe we live in, and the same is true about mathematics. There is no doubt that the physical and social universe daily affects each musician, and painter, and writer, and mathematician, and that therefore a part at least of the raw material of the artist is the work of facts and motions, sights and sounds. Continual contact between the work and art is bound to change the latter, and perhaps even to improve it.

The ultimate goal of "applied literature", and of applied mathematics, is action. A campaign speech is made so as to cause you to pull the third lever on a voting machine rather than the fourth. An aerodynamic equation is solved so as to cause a plane wing to lift its load fast enough to avoid complaints from the home owners near the airport. These examples are crude and obvious; there are subtler ones. If the biography of a candidate, a factually correct and honest biography, does not directly mention the forthcoming election, is it then pure literature? If a discussion of how mathematically idealized air flows around moving figures of various shapes, a logically rigorous and correct discussion, does not mention airplanes or airports, is it then pure mathematics? And what about the in-between cases: the biography that, without telling lies, is heavily prejudiced; and the treatise on aerodynamics that, without being demonstrably incorrect, uses cost-cutting rough approximations—are they pure or applied? ...

To confuse the issue still more, pure mathematics can be practically useful and applied mathematics can be artistically elegant. Pure mathematicians, trying to understand involved logical and geometrical interrelations, discovered the theory of convex sets and the algebraic and topological study of various classes of functions. Almost as if by luck, convexity has become the main tool in linear programming (an indispensable part of modern economic and industrial practice), and functional analysis has become the main tool in quantum theory and particle physics. The physicist regards the applicability of von Neumann algebras (a part of functional analysis) to elementary particles as the only justification of the former; the mathematician regards the connections as the only interesting aspect of the latter. *De gustibus non disputandum est?*

Just as pure mathematics can be useful, applied mathematics can be more beautifully useless than is sometimes recognized. Applied mathematics is not engineering; the applied mathematician does not design airplanes or atomic bombs. Applied mathematics is an intellectual discipline, not a part of industrial technology. The ultimate goal of applied mathematics is action, to be sure, but, before that, applied mathematics is a part of theoretical science concerned with the general principles behind what makes planes fly and bombs explode. …

The deepest assertion about the relation between pure and applied mathematics that needs examination is that it is symbiotic, in the sense that neither can survive without the other. Not only, as is universally admitted, does the applied need the pure, but, in order to keep from becoming inbred, sterile, meaningless, and dead, the pure needs the revitalization and the contact with reality that only the applied can provide. …

On Being a Mathematician

Excerpt from:

***I Want to Be a Mathematician*, p. 400, Springer-Verlag, New York (1985)**

It takes a long time to learn to live—by the time you learn your time is gone. I spent most of a lifetime trying to be a mathematician—and what did I learn? What does it take to be one? I think I know the answer: you have to be born right, you must continually strive to become perfect, you must love mathematics more than anything else.

Born right? Yes. To be a scholar of mathematics you must be born with talent, insight, concentration, taste, luck, drive, and the ability to visualize and guess. For teaching you must in addition understand what kinds of obstacles learners are likely to place before themselves, and you must have sympathy for your audience, dedicated selflessness, verbal ability, clear style, and expository skill. To be able, finally, to pull your weight in the profession with the essential clerical and administrative jobs, you must be responsible, conscientious, careful, and organized—it helps if you also have some qualities of leadership and charisma.

You can't be perfect, but if you don't try, you won't be good enough.

To be a mathematician you must love mathematics more than family, religion, money, comfort, pleasure, glory. I do not mean that you must love it to the exclusion of family, religion, and the rest, and I do not mean that if you do love it, you'll never have any doubts, you'll never be discouraged, you'll never be ready to chuck it all and take up gardening instead. Doubts and discouragements are part of life. Great mathematicians have doubts and get discouraged, but usually they can't stop doing mathematics anyway, and, when they do, they miss it very deeply. …