THE HEART OF MATHEMATICS

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Introduction. What does mathematics *really* consist of? Axioms (such as the parallel postulate)? Theorems (such as the fundamental theorem of algebra)? Proofs (such as Gödel's proof of undecidability)? Concepts (such as sets and classes)? Definitions (such as the Menger definition of dimension)? Theories (such as category theory)? Formulas (such as Cauchy's integral formula)? Methods (such as the method of successive approximations)?

Mathematics could surely not exist without these ingredients; they are all essential. It is nevertheless a tenable point of view that none of them is at the heart of the subject, that the mathematician's main reason for existence is to solve problems, and that, therefore, what mathematics *really* consists of is problems and solutions.

"Theorem" is a respected word in the vocabulary of most mathematicians, but "problem" is not always so. "Problems," as the professionals sometimes use the word, are lowly exercises that are assigned to students who will later learn how to prove theorems. These emotional overtones are, however, not always the right ones.

The commutativity of addition for natural numbers and the solvability of polynomial equations over the complex field are both theorems, but one of them is regarded as trivial (near the basic definitions, easy to understand, easy to prove), and the other as deep (the statement is not obvious, the proof comes via seemingly distant concepts, the result has many surprising applications). To find an unbeatable strategy for tic-tac-toe and to locate all the zeroes of the Riemann zeta function are both problems, but one of them is trivial (anybody who can understand the definitions can find the answer quickly, with almost no intellectual effort and no feeling of accomplishment, and the answer has no consequences of interest), and the other is deep (no one has found the answer although many have sought it, the known partial solutions require great effort and provide great insight, and an affirmative answer would imply many non-trivial corollaries). Moral: theorems can be trivial and problems can be profound. Those who believe that the heart of mathematics consists of problems are not necessarily wrong.

Problem Books. If you wanted to make a contribution to mathematics by writing an article or a book on mathematical problems, how should you go about it? Should the problems be elementary (pre-calculus), should they be at the level of undergraduates or graduate students, or should they be research problems to which no one knows the answer? If the solutions are known, should your work contain them or not? Should the problems be arranged in some systematic order (in which case the very location of the problem is some hint to its solution), or should they be arranged in some "random" way? What should you expect the reader to get from your work: fun, techniques, or facts (or some of each)?

All possible answers to these questions have already been given. Mathematical problems have quite an extensive literature, which is still growing and flowering. A visit to the part of the stacks labeled QA43 (Library of Congress classification) can be an exciting and memorable revelation, and there are rich sources of problems scattered through other parts of the stacks too. What follows is a quick review of some not quite randomly selected but probably typical problem collections that even a casual library search could uncover.

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Hilbert's Problems. The most risky and possibly least rewarding kind of problem collection to offer to the mathematical public is the one that consists of research problems. Your problems could become solved in a few weeks, or months, or years, and your work would, therefore, be out of date much more quickly than most mathematical exposition. If you are not of the stature of Hilbert, you can never be sure that your problems won't turn out to be trivial, or impossible, or, perhaps worse yet, just orthogonal to the truth that we all seek—wrongly phrased, leading nowhere, and having no lasting value.

A list of research problems that has had a great effect on the mathematical research of the twentieth century was offered by Hilbert in the last year of the nineteenth century at the International Congress of Mathematicians in Paris [3]. The first of Hilbert's 23 problems is the continuum hypothesis: is every uncountable subset of the set $\mathbb{R}$ of real numbers in one-to-one correspondence with $\mathbb{R}$? Even in 1900 the question was no longer new, and although great progress has been made since then and some think that the problem is solved, there are others who feel that the facts are far from fully known yet.

Hilbert's problems are of varying depths and touch many parts of mathematics. Some are geometric (if two tetrahedra have the same volume, can they always be partitioned into the same finite number of smaller tetrahedra so that corresponding pieces are congruent?—the answer is no), and some are number-theoretic (is $2^{\sqrt{2}}$ transcendental?—the answer is yes). Several of the problems are still unsolved. Much of the information accumulated up to 1974 was brought up to date and collected in one volume in 1976 [5], but the mathematical community's curiosity did not stop there—a considerable number of both expository and substantive contributions has been made since then.

Pólya-Szegő. Perhaps the most famous and still richest problem book is that of Pólya and Szegő [6], which first appeared in 1925 and was republished (in English translation) in 1972 and 1976. In its over half a century of vigorous life (so far) it has been the mainstay of uncountably many seminars, a standard reference book, and an almost inexhaustible source of examination questions that are both inspiring and doable. Its level stretches from high school to the frontiers of research. The first problem asks about the number of ways to make change for a dollar, the denominations of the available coins being 1, 5, 10, 25, and 50, of course; in the original edition the question was about Swiss francs, and the denominations were 1, 2, 5, 10, 20, and 50. From this innocent beginning the problems proceed, in gentle but challenging steps, to the Hadamard three circles theorem, Tchebychev polynomials, lattice points, determinants, and Eisenstein's theorem about power series with rational coefficients.

Dörrie. "The triumph of mathematics" is the original title (in German) of Dörrie's book [1]. This is a book that deserves to be much better known than it seems to be. It is eclectic, it is spread over 2000 years of history, and it ranges in difficulty from elementary arithmetic to material that is frequently the subject of graduate courses.

It contains, for instance, the following curiosity attributed to Newton (Arithmetica Universalis, 1707). If "a cows graze b fields bare in c days, a' cows graze b' fields bare in c' days, a'' cows graze b'' fields bare in c'' days, what relation exists between the nine magnitudes a to c''? It is assumed that all fields provide the same amount of grass, that the daily growth of the fields remains constant, and that all the cows eat the same amount each day." Answer:

$$\det\begin{pmatrix} b & bc & ac \\ b' & b'c' & a'c' \\ b'' & b''c'' & a''c'' \end{pmatrix} = 0.$$

This is Problem 3, out of a hundred.

The problems lean more toward geometry than anything else, but they include also Catalan's question about the number of ways of forming a product of n prescribed factors in a multiplicative system that is totally non-commutative and non-associative ("how many different ways can a product of n different factors be calculated by pairs?," Problem 7), and the Fermat-Gauss impossibility theorem ("the sum of two cubic numbers cannot be a cubic number," Problem 21).

Two more examples should give a fair idea of the flavor of the collection as a whole: "every quadrilateral can be considered as a perspective image of a square" (Problem 72), and "at what point of the earth's surface does a perpendicularly suspended rod appear the longest?"(Problem 94). The style and the attitude are old-fashioned, but many of the problems are of the eternally interesting kind; this is an excellent book to browse in.

Steinhaus. My next mini-review is of a Polish contribution, Steinhaus [7], which (like Dörrie's) has exactly 100 problems, and they are genuinely elementary and good solid fun. When someone says "problem book" most people think of something like this one, and, indeed, it is an outstanding exemplar of the species. The problems are, however, not equally interesting or equally difficult. They illustrate, moreover, another aspect of problem solving: it is sometimes almost impossible to guess how difficult a problem is, or, for that matter, how interesting it is, till after the solution is known.

Consider three examples. (1) Does there exist a sequence $\{x_1, x_2, \ldots, x_{10}\}$ of ten numbers such that (a) x_1 is contained in the closed interval $[0,1]$, (b) x_1 and x_2 are contained in different halves of $[0,1]$, (c) each of x_1, x_2, and x_3 is contained in a different third of the interval, and so on up through $x_1, x_2, \ldots, x_{10}$? (2) If 3000 points in the plane are such that no three lie on a straight line, do there exist 1000 triangles (meaning interior and boundary) with these points as vertices such that no two of the triangles have any points in common? (3) Does there exist a disc in the plane (meaning interior and boundary of a circle) that contains exactly 71 lattice points (points both of whose coordinates are integers)?

Of course judgments of difficulty and interests are subjective, so all I can do is record my own evaluations. (1) is difficult and uninteresting, (2) is astonishingly easy and mildly interesting, and (3) is a little harder than it looks and even prima facie quite interesting. In defense of these opinions, I mention one criterion that I used: if the numbers $(10, 1000, 71)$ cannot be replaced by arbitrary positive integers, I am inclined to conclude that the corresponding problem is special enough to be dull. It turns out that the answer to (1) is yes, and Steinhaus proves it by exhibiting a solution (quite concretely: $x_1 = .95$, $x_2 = .05$, $x_3 = .34$, $x_4 = .74$, etc.). He proves (the same way) that the answer is yes for 14 instead of 10, and, by three pages of unpleasant looking calculation, that the answer is no for 75. He mentions that, in fact, the answer is yes for 17 and no for every integer greater than 17. I say that's dull. For (2) and (3) the answers are yes (for all n in place of 1000, or in place of 71).

Glazman-Ljubič. The book of Glazman and Ljubič [2] is an unusual one (I don't know of any others of its kind), and, despite some faults, it is a beautiful and exciting contribution to the problem literature. The book is, in effect, a new kind of textbook of (finite-dimensional) linear algebra and linear analysis. It begins with the definitions of (complex) vector spaces and the concepts of linear dependence and indepedence; the first problem in the book is to prove that a set consisting of just one vector x is linearly independent if and only if $x \neq 0$. The chapters follow one another in logical dependence, just as they do in textbooks of the conventional kind: Linear operators, Bilinear functionals, Normed spaces, etc.

The book is not expository prose, however; perhaps it could be called expository poetry. It gives definitions and related explanatory background material with some care. The main body of the book consists of problems; they are all formulated as assertions, and the problem is to prove them. The proofs are not in the book. There are references, but the reader is told that he will not need to consult them.

The really new idea in the book is its sharp focus: this is really a book on functional analysis, written for an audience who is initially not even assumed to know what a matrix is. The ingenious idea of the authors is to present to a beginning student the easy case, the transparent case, the motivating case, the finite-dimensional case, the purely algebraic case of some of the deepest analytic facts that functional analysts have discovered. The subjects discussed include spectral theory, the Toeplitz-Hausdorff theorem, the Hahn-Banach theorem, partially ordered vector spaces, moment problems, dissipative operators, and many other such analytic sounding results. A beautiful course could be given from this book (I would love to give it), and a student brought up in such a course could become an infant prodigy functional analyst in no time.

(A regrettable feature of the book, at least in its English version, is the willfully unorthodox terminology. Example: the (canonical) projection from a vector space to a quotient space is called a "contraction", and what most people call a contraction is called a "compression". Fortunately the concept whose standard technical name is compression is not discussed.)

Klambauer. The last addition to the problem literature to be reviewed here is Klambauer's [4]. Its subject is real analysis, and, although it does have some elementary problems, its level is relatively advanced. It is an excellent and exciting book. It does have some faults, of course, including some misprints and some pointless repetitions, and the absence of an index is an exasperating feature that makes the book much harder to use than it ought to be. It is, however, a great source of stimulating questions, of well known and not so well known examples and counterexamples, and of standard and not so standard proofs. It should be on the bookshelf of every problem lover, of every teacher of analysis (from calculus on up), and, for that matter, of every serious student of the subject.

The table of contents reveals that the book is divided into four chapters: Arithmetic and combinatorics, Inequalities, Sequences and series, and Real functions. Here are some examples from each that should serve to illustrate the range of the work, perhaps to communicate its flavor, and, I hope, stimulate the appetite for more.

The combinatorics chapter asks for a proof of the "rule for casting out nines" (is that expression for testing the divisibility of an integer by 9 via the sum of its decimal digits too old-fashioned to be recognized?), it asks how many zeroes there are at the end of the decimal expansion of 1000!, and it asks for the coefficient of x^k in $(1+x+x^2+\cdots+x^{n-1})^2$. Along with such problems there are also unmotivated formulas that probably only their father could love, and there are a few curiosities (such as the problem that suggests the use of the well ordering principle to prove the irrationality of $\sqrt{2}$). A simple but striking oddity is this statement: if m and n are distinct positive integers, then

$$m^{n^m} \neq n^{m^n}.$$

The chapter on inequalities contains many of the famous ones (Hölder, Minkowski, Jensen), and many others that are analytically valuable but somewhat more specialized and therefore somewhat less famous. A curiosity the answer to which very few people are likely to guess is this one: for each positive integer n, which is bigger

$$\sqrt{n}^{\sqrt{n+1}} \quad \text{or} \quad \sqrt{n+1}^{\sqrt{n}}\,?$$

The chapter on sequences has the only detailed and complete discussion that I have ever seen of the fascinating (and non-trivial) problem about the convergence of the infinite process indicated by the symbol

$$x^{x^{x^{\cdot^{\cdot^{\cdot}}}}}$$

Students might be interested to learn that the result is due to Euler; the reference given is to the article *De formulis exponentialibus replicatis*, Acta Academica Scientiarum Imperialis Petropolitanae, 1777. One more teaser: what is the closure of the set of all real numbers of the form $\sqrt{n}-\sqrt{m}$ (where n and m are positive integers)?

The chapter on real functions is rich too. It includes the transcendentality of e, some of the basic properties of the Cantor set, Lebesgue's example of a continuous but nowhere differentiable function, and F. Riesz's proof (via the "rising sun lemma") that every continuous monotone function is differentiable almost everywhere. There is a discussion of that vestigial curiosity called Osgood's theorem, which is the Lebesgue bounded convergence theorem for continuous functions on a closed bounded interval. The Weierstrass polynomial approximation theorem is here (intelligently broken down into bite-size lemmas), and so is one of Gauss's proofs of the fundamental theorem of algebra. For a final example I mention a question that should be asked much more often that it probably is: is there an example of a series of functions, continuous on a closed bounded interval, that converges absolutely and uniformly, but for which the Weierstrass M-test fails?

Problem Courses. 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.

Covering Material. 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 else was given short shrift in class—well, books and journals do exist, and students have been known to read them in a pinch.

Problem Seminars. While a problem course might be devoted to a sharply focused subject, it also might not—it might just be devoted to fostering the questioning attitude and improving technique by discussing problems widely scattered over several fields. Such technique courses, sometimes called Problem Seminars, can exist at all levels (for beginners, for Ph.D. candidates, or for any intermediate group).

The best way to conduct a problem seminar is, of course, to present problems, but it is just as bad for an omniscient teacher to do all the asking in a problem seminar as it is for an omniscient teacher to do all the talking in a lecture course. I strongly recommend that students in a problem seminar be encouraged to discover problems on their own (at first perhaps by slightly modifying problems that they have learned from others), and that they should be given public praise (and grade credit) for such discoveries. Just as you should not tell your students all the answers, you should also not ask them all the questions. One of the hardest parts of problem solving is to ask the right question, and the only way to learn to do so is to practice. On the research level, especially, if I pose a definite thesis problem to a candidate, I am not doing my job of teaching him to do research. How will he find his next problem, when I am no longer supervising him?

There is no easy way to teach someone to ask good questions, just as there is no easy way to teach someone to swim or to play the cello, but that's no excuse to give up. You cannot swim for someone else; the best you can do is to supervise with sympathy and reinforce the right kind of fumble by approval. You can give advice that sometimes helps to make good questions out of bad ones, but there is no substitute for repeated trial and practice.

An obvious suggestion is: generalize; a slightly less obvious one is: specialize; a moderately sophisticated one is: look for a non-trivial specialization of a generalization. Another well-known piece of advice is due to Pólya: make it easier. (Pólya's dictum deserves to be propagated over and over again. In slightly greater detail it says: if you cannot solve a problem, then there is an easier problem that you cannot solve, and your first job is to find it!) The advice I am fondest of is: make it sharp. By that I mean: do not insist immediately on asking the natural question ("what is ...?", "when is ...?", "how much is ...?"), but focus first on an easy (but nontrivial) yes-or-no question ("is it ...?").

Epilogue. I do believe that problems are the heart of mathematics, and I hope that as teachers, in the classroom, in seminars, and in the books and articles we write, we will emphasize them more and more, and that we will train our students to be better problem-posers and problem-solvers than we are.

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