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Some Books of Auld Lang Syne

P. R. HALMOS

§0. Preface

A committee was charged with the responsibility of assembling one or more historical volumes on the occasion of the hundredth birthday of the American Mathematical Society. The committee wrote to some possible authors and, hoping to present "some part of the mathematical history of the last 100 years," asked for an "autobiographically oriented historical article." My biography goes back a long way, but a hundred years is more than I can remember, and my first reaction was to decline the invitation by return mail.

The possibilities that the committee's letter mentioned frightened me. "Eyewitness" accounts of mathematicians or mathematical institutions was one way to go, but I didn't trust my memory. Yes, I have known many mathematicians, and I have been connected with many mathematical institutions, but, surely, inaccurate anecdotes would be worse than none, and I was scared to stick my neck out. The history of the development of certain fields of mathematics would be welcome, the letter said, but that was even more scary. I am far from being a trained historian, and the ones I know tell me that having lived through some history is very far from being enough to write about it. What to do, what to do?

The answer, when it descended on me, was to make use of my long years of experience with the literature of mathematics: I used to read some of it, I kept trying to write it, and I edited what sometimes seemed like an infinite amount of it. But that's not history — who would care to read about *that*? The rebuttal to my effort to get out of work arrived in the middle of one night:

books! When I was a junior member of the mathematical community, certain books were famous, were used at all the major institutions and by the less major ones that copied them, and constituted the backbone of mathematical education in this country. Some of those books are, mercifully, forgotten by now, and a few others are still very much alive; wouldn't people be interested in what the books of auld lang syne were, what was in them, and how they differ from the idols of today?

That's the story of how this article came into being. It tells about books, one of which was published more than eighty years ago and the newest only a little more than thirty, but in any event books that were influential in (definitely in the interior of) the hundred year period now being looked at. A necessary condition for a book to be discussed here is that I knew the book, that I had some direct and personal contact with it, but that condition is not sufficient — I made choices. This is a personal report, an autobiographically oriented one, and since I cannot always explain the basis on which I made choices, I won't try.

The books in the report are grouped by subject, an imprecise but suggestive classification, and within each subject they are arranged somehow—most often by date, but sometimes by level. Although the review of book X might refer to book Y (and usually when that happens the review of Y precedes that of X), the reviews are independent of each other. In other words this is not one longish essay, but 26 shortish ones, and they can be read in any order that the reader prefers.

Here they are, some books of auld lang syne.

Contents

§1. Calculus

1. William Anthony Granville, Percey F. Smith, and William Raymond Longley, *Elements of the differential and integral calculus* (1904)

The title page describes Granville as "formerly president of Gettysburg College," and Smith and Longley as "professors of mathematics, Yale University." Although the original edition was copyright 1904, the one I am looking at (1941) is not the last; the book was widely used in this country from the early years of the century well into the 40s and maybe later. I studied from it at Illinois in the 1930s, and it was the basic calculus text, at various times, at many universities, including many of the best.

The in thing is to sneer at this book, and I have done my share of that; it is frequently used as the standard horrible example of how bad calculus books can be. Actually, now that I have taken another look at it from the point of view not of a bewildered student, but of a jaded mathematician, I don't think it's bad at all — it just doesn't do what some of us think a good book should. It's a cookbook. It tells you the kinds of problems that arise in calculus and tells you how to solve them. It can be of tremendous help to the overloaded teacher who is teaching three sections of calculus. There are hundreds and hundreds of problems for homework and for exams; many of them are routine, minor variations of one another, and many of them tricky and difficult.

Chapter I is a harmless collection of formulas — the quadratic formula, the binomial theorem, the basic trigonometric identities, the equations of a line in space,etc. The work begins in Chapter II, with this sentence: "A *variable* is a quantity to which an unlimited number of values can be assigned in an investigation." A page later: "When two variables are so related that the value of the first variable is determined when the value of the second variable is given, then the first variable is said to be a function of the second." The definition of $v \to a$ says that "the numerical value of the difference $v - a$ ultimately becomes and remains less than any preassigned positive number, however small." Much later: "A *sequence* is a succession of terms formed according to some fixed rule or law." If you know what's going on, you can tell what's going on, but, harassed students or jaded mathematicians, most of us agree that if you don't know what's going on, this isn't the place to find out.

Differentiation begins in Chapter III, and soon after the official definitions there is a displayed and italicized "General rule for differentiation"; a footnote informs us that it is "also called the *Four-Step Rule.*" The four steps (I am compressing the language) are: (1) add Δx to x, (2) subtract to find Δy, (3) divide by Δx, and (4) find the limit. Many homework and examination questions in my calculus course under Roy Brahana went like that: "use the four-step rule to find the derivative of...." An avant-garde graduate assistant these days might express the same problem by saying "use the definition of differentiation to find the derivative of...." A big difference?

The copy of Granville (which is the accepted abbreviated way of referring to the book) that I am looking at has 27 chapters, and their titles (and their contents) contain everything that I could ever dream of putting into a calculus book. Differentiation of algebraic forms, applications, higher derivatives, transcendental functions, parametric equations,..., curvature, Rolle's theorem, indefinite and definite sintegrals, a whole chapter on the constant of integration, "integration a process of summation," formal integration,..., centroids, ..., Maclaurin series, ..., differential equations, partial derivatives, multiple integrals — and, at the end, as the last two chapters, "Curves for reference" (seven pages of pictures such as the spiral of Archimedes and the two-leaved rose lemniscate) and "Table of integrals" (fourteen pages of them, including

$$\int u\sqrt{2au - u^2}\, du$$

and

$$\int \operatorname{arc\,csc} u\, du).$$

This book was useful once, and I no longer hold against it that it is not a "rigorous" book and that it is not on the subject that some of us elitists call mathematics. Many of us remember it with nostalgia even if not with fondness. R.I.P.

2. Richard Stevens Burington and Charles Chapman Torrance, *Higher mathematics* (1939)

Are books like this still being written, and, more to the point, are books like this still being used in undergraduate courses? The authors' university affiliation is given on the title page as The Case School of Applied Science (an institution that has become a part of the conglomerate now called Case Western Reserve University), and the book's subtitle is "with applications to science and engineering." These facts would seem to slant the book toward the real world, and, sure enough, the first sentence of the preface tells us that the book is "designed primarily to meet the growing needs of students interested in the applications of mathematics to physics and engineering." A few lines later, however, we are told that "In keeping with the growing demand for rigor, ..., stress has been placed on the precise mathematical interpretations of the concepts studied, ..., [and] thus, the present treatment is suited to students of pure mathematics."

It sure is, both in 1939 and in 1989. The book came out after my student days were over, so that I never studied from it, but I did have occasion to use it in courses I taught, or, to be precise, to use something like 20% of it. It's a big book (844 pages), and it covers a lot. The authors do not explicitly say what audience they are writing for, but it seems clear that the work is not a "calculus book" but, rather, a book on "advanced calculus." (I have been looking for an exact definition of both those phrases for a long time.) In any event, Part A of Chapter I is called Elementary Review, and is a lightning calculus course. It begins with a gentle motivation of the concept of limit. Thus, for instance, it offers a short table of values of $(1 + x)^{1/x}$, and then urges the student "to compute more values of this function, using a large table of logarithms." (Don't tell me that today of all days I left my calculator at home!) On p. 9 it offers a tentative definition. (We define the word "approach" to mean "become and remain arbitrarily close to"), and then meanders leisurely, till at the bottom of p. 16 it reaches the ε-δ definition of limit.

At this point the pace becomes a bit more brisk. Differential calculus is covered efficiently, through Rolle's theorem, Taylor's theorem, and infinitesimals. An infinitesimal is defined as a "variable" with certain properties, but, except for what some people regard as an unfortunate word, there is nothing wrong, and in all the illustrations and applications an infinitesimal is nothing but a function.

Part B of Chapter I covers partial differentiation, including a definition of differentials (of functions of several variables). The definition is clear and correct, and without saying it in so many words the authors make plain that a differential is a certain linear function. Chapter II is the integral calculus (through numerical integration). In an effort to stay near to the standard material without talking nonsense the authors use some unusual symbols: I_x denotes indefinite integration, I_x is the result of evaluating an indefinite integral between indicated upper and lower limits, and the definite integral (limit of Riemann sums) is denoted by **S**. The standard notation is mentioned and used, but grudgingly. Chapter III is ordinary differential equations, Chapter IV is series, and Chapter V is complex function theory, including Cauchy's integral formula, residues, Liouville's theorem, analytic continuation, and elliptic functions (!). Chapter VI begins with a bird's eye view of linear algebra (including a treatment of determinants clearer and more honest than Bôcher's), and goes on to vector analysis, differential geometry, and tensor analysis. Chapter VII is partial differential equations, Chapter VIII is the calculus of variations and dynamics, and the end is reached with Chapter IX, a short "introduction to real variable theory" that contains a mention of Dedekind cuts and presents the basic consequences of the local compactness of the line (Heine-Borel theorem, Bolzano-Weierstrass theorem, intermediate value theorem, etc.).

Are higher level, good books like this still being written, and, more to the point, are high level, good, tough, honest books like this still being used to put some spine into undergraduate courses?

3. **R. Courant**, *Differential and integral calculus* (revised edition) (1938)

With an author such as Courant and a translator such as McShane, how could the book fail? It didn't fail; for quite a few years it was a conspicuous success. Partly as a reaction to the egregious books of the preceding decades (like Granville, Smith, and Longley), the pendulum swung to rigor, and this excellently translated and improved version rode to success partly on the coattail of the good reputation of Courant's original German lectures. The book was adopted by many of the high quality undergraduate colleges in the

U.S., as well as by some of the leading research universities; it was the elite way to go. It was used at Chicago when I was there, but not often and not for long.

Burington and Torrence is an advanced calculus book, aimed at people who are not frightened by

$$\int \frac{dx}{1+x^2}$$

and are ready to face the complications of the implicit function theorem. Courant is more modest in a sense, but really more ambitious; he addresses the mathematically innocent reader, but he hopes to turn that reader into a sophisticate who knows that differentials must not be used and knows how to use them anyway.

The preface emphasizes that the treatment is different from the traditional one; the aim, it says, is to make the subject easier to grasp, "not only by giving proofs step by step, but also by throwing light on the interconnexions and purposes of the whole." The next paragraph seems to be intended to reassure the worrier. "The beginner should note that I have avoided blocking the entrance to the concrete facts of the differential and integral calculus by discussions of fundamental matters, for which he is not yet ready."

The sophistication begins in the first section (The continuum of numbers) of the first chapter. The natural numbers are assumed known (the commutative, associative, and distributive laws are mentioned in a hasty footnote); rational numbers are described (not defined) as the ones obtained from natural numbers by subtraction and division, and real numbers are described as infinite decimals. The first substantial mathematical result in the book, the climax of the first section, is a proof of the Schwarz inequality. Complex numbers come some 60 pages later; the following two sentences appear in the first paragraph of the section that introduces them. "If, for example, we wish the equation

$$x^2 + 1 = 0$$

to have roots, we are obliged to introduce new symbols i and $-i$ as the roots of this equation. (As is shown in algebra, this is sufficient to ensure that *every* algebraic equation shall have a solution.)" In *algebra*?

Calculus proper begins in Chapter II, and (this is the highly touted break away from out-of-date tradition) it begins with integration. The definite integral is motivated by the concept of area and then an "analytical definition" of it is offered as a limit of sums. There is some fudging going on about just what "limit" means here. Courant has my sympathy. This is obviously not the place to enter into the delicacies of the Moore-Smith theory, but since the integral *is* a Moore-Smith limit, a description of it without those delicacies is bound to be fudging. Immediately after the definition, the integrals of x, and x^2, and in fact of x^α for any rational α (except -1), are evaluated, and so are the integrals of $\sin x$ and $\cos x$. The evaluations must make use of summation ingenuities, which are probably good for the soul: they should help convince students that the slick ways of evaluating integrals are worth learning.

The section that follows introduces the derivative, with the usual sort of talk about velocities and slopes; almost immediately after that comes the theorem that differentiability implies continuity. The traditional symbols Δx, Δy, dx, and dy make their appearance, accompanied by two brief sermons. Both sermons are right, but I suspect that to some students they would seem to contradict one another. The first sermon says that it is bad to regard dx and dy as "infinitesimals" whose quotient is the derivative; the second sermon says that they are good things anyway: "we can deal with the symbols dy and dx in exactly the same way as if they were ordinary numbers." A missionary's lot is not a happy one.

The weaseling continues in the discussion of differentials. "... we first define the derivative $\varphi'(x)$ by our limiting process, then think of x as fixed and consider the increment $h = \Delta x$ as the independent variable. This quantity h we call the *differential* of x, and write $h = dx$. We now define the expression $dy = y'\,dx = h\varphi'(x)$ as the *differential of the function* y; dy is therefore a number which has nothing to do with infinitely small quantities." I am not happy. What is dy? Is it really a number?

Once both integrals and derivatives are available, the very next section connects them: it contains a discussion of primitives versus integrals, and a statement of "the fundamental theorem." Unlike in most other calculus books, the reader has still not been exposed to the formal juggling with (indefinite) integrals that calculus courses usually consist of. The manipulative aspects of calculus, the standard elementary functions, maxima and minima — all that comes in Chapters III and IV, and a lot more comes after that. The material so far mentioned is what an ordinary calculus course hopes to contain, but rarely finishes; Chapters V–X (more than half the book) treat applications, Taylor's theorem, numerical methods, series (including Fourier series), and even a brief introduction to functions of several variables and differential equations. And all that is just Volume I. (Volume II treats functions of several variables, and even a brief introduction to the calculus of variations and to functions of a complex variable.)

This report was not intended to be an exhaustive review — all I hoped to accomplish was to communicate the intention and the flavor of the book. The truth to tell I didn't like it much. I found it verbose, pedantic, and heavy handed. The less ambitious book of Burington and Torrance probably did more good for the teaching of mathematics in the U.S. than the book of Courant — but, good or bad, they have both ceased being fashionable.

4. G. H. Hardy, *A course of pure mathematics* (1908)

Hardy was a great mathematician, but not a great expositor. This book, however, shows that a conscientious and honest writer who really *really* understands what he is writing about can produce a work of rich exposition that is readable and enjoyable. The first edition came out when Hardy was 31; my copy is the sixth (but definitely not the last) edition, which I bought in 1935 (a couple of years after it appeared) and have treasured ever since. It was the official textbook in the baby real variable course that Pierce Ketchum tried to teach me at Illinois; I found it very hard. (The phrase "baby real variables" is often used to describe the part of analysis that is more intellectual than line integrals but less sophisticated than Lebesgue measure.)

The main preface (the preface to the first edition) reads in part as follows. "This book has been designed primarily for the use of first year students at the Universities whose abilities reach or approach something like what is usually described as 'scholarship standard'.... I regard the book as being really elementary. There are plenty of hard examples [which means problems]... But I have done my best to avoid the inclusion of anything that involves really difficult ideas. For instance, I make no use of the 'principle of convergence' [the Cauchy condition]: uniform convergence, double series, infinite products are never alluded to...."

The first chapter begins with rational numbers (whose existence and properties are assumed known) and proceeds briskly to define real numbers as Dedekind cuts; Dedekind's theorem is reached on p. 29. Weierstrass's theorem (which I was taught to call the Bolzano-Weierstrass theorem, the statement that every infinite set in a closed interval has a point of accumulation) is on the next page, and for a few moments I thought Hardy (or his copy-editor) had made a silly mistake: the theorem is stated for an interval denoted by the symbol (α, β). All is well, however; the current convention about (α, β) versus $[\alpha, \beta]$ had not yet been adopted, and Hardy makes very clear that he is talking about the closed interval $[\alpha, \beta]$. The "examples" at the end of Chapter I include Schwarz's inequality (with a hint). Many of the others are special cases of the theorem that algebraic combinations of algebraic numbers are algebraic (for instance: find rational numbers a, b such that

$$\sqrt[3]{(7+5\sqrt{2})} = a + b\sqrt{2});$$

they culminate in the general theorem that every root of a polynomial equation with algebraic coefficients is algebraic.

Chapter II introduces functions. Hardy offers no official definition of the word, but he proceeds to give a large number of examples, including, for instance, what I was taught to call the Dirichlet function (the characteristic function of the set of rational numbers). A long "example" at the end of the chapter presents the theory of ruler-and-compasses constructions; it culminates in the statement that "Euclidean methods will construct any surd expression involving square roots only, and no others." Chapter III defines complex numbers; here is a sample of the examples at its end. "If $z = 2Z + Z^2$, then the circle $[Z] = 1$ corresponds to [that is, the image of the unit circle under the mapping is] a cardioid in the plane of z."

The real stuff begins in Chapter IV on limits (of sequences). A little more than half way along the long preparation for the ε-δ definition there appears the boldface sentence

There is no number "infinite",

followed a few lines later by the exhortation "the reader will always have to bear in mind... that ∞ *by itself* means nothing, although *phrases containing it* sometimes mean something." Here are a couple of examples of the examples at the end of the chapter." (12) If x_1, x_2 are positive and

$$x_{n+1} = \frac{1}{2}(x_n + x_{n-1}),$$

then the sequences $x_1, x_3, x_5, \ldots$ and $x_2, x_4, x_6, \ldots$ are one a decreasing and the other an increasing sequence, and they have the common limit

$$\frac{1}{3}(x_1 + 2x_2).$$

(14) The function

$$y = \lim_{n\to\infty} \frac{1}{1 + n\sin^2 \pi x}$$

is equal to 0 except when x is an integer, and then equal to 1."

Chapter V continues in the same spirit about "limits of functions of a continuous variable," or, in other words, about the topology of the line. We learn that continuous functions on compact intervals attain their bounds, and we learn (the Heine-Borel theorem) that closed and bounded intervals are compact. Examples: "(15) If $\varphi(x) = 1/q$ when $x = p/q$, and $\varphi(x) = 0$ when x is irrational, then $\varphi(x)$ is continuous for all irrational and discontinuous for all rational values of x. (20) Show that the numerically least value of $\arctan y$ is continuous for all values of y and increases steadily from $-\frac{1}{2}\pi$ to $\frac{1}{2}\pi$ as y varies through all real values."

Many of the words in Chapters VI–IX would look familiar to American undergraduates of the 1980s. Derivatives, integrals, Newton's method, the comparison test for the convergence of infinite series, $\log x$ and e^x and the circular [= trigonometric] functions are treated, as well as maxima and minima, the mean value theorem, implicit functions, the fundamental theorem of the integral calculus, absolute and conditional convergence, and even the hyperbolic functions. Chapter X, the last one, discusses log, exp, and sin and cos for complex arguments. The treatment, however, is rather different from how we usually proceed in Math 103; I'll try to communicate its flavor by, again, quoting a few examples.

"(VI,7) If $y^3 + 3yx + 2x^3 = 0$ then $x^2(1 + x^3)y'' - (3/2)xy' + y = 0$. [Reference: *Math. Trip.* 1903.]

(VI,35) If $f(x) \to a$ as $x \to \infty$, then $f'(x)$ cannot tend to any limit other than zero.

(VII,50) Calculate

$$\int_0^\pi \sqrt{(\sin x)}\, dx$$

to two places of decimals."

The book ends with four appendixes that are partly sermons and partly mathematical subtleties. Appendix I proves the fundamental theorem of algebra by a "topological" argument, not using anything like Liouville's theorem, of course. Appendix II advocates Hardy's favorite symbols, O and o and $\sim$, and uses them to discuss such things as Euler's constant and Stirling's formula. Appendix III is a note on double limit problems, and Appendix IV (much more sermon than subtlety) is titled "The infinite in analysis and geometry."

All the topics treated in this book can be found in more nearly ordinary calculus books (well, almost all), but the way they are treated is not only different from Granville, Smith, and Longley — it is just as far in spirit from Burington and Torrance, and from Courant. Hardy's book is the toughest, most challenging, most rewarding, and most mathematical calculus book that you could possibly imagine.

§2. Algebra and number theory

5. **Maxime Bôcher**, *Introduction to higher algebra* (1907)

The first copyright was in 1907, but the copy I bought in 1935 was printed that year, with, apparently, no change from the first printing — it is certainly not called a new edition.

From the point of view of 80 years after the first appearance of the book, the preface makes curious reading. It begins this way. "An American student approaching the higher parts of mathematics usually finds himself unfamiliar with most of the main facts of algebra, to say nothing of their proofs. [That sentence could have been written in 1987, couldn't it?] Thus he has only a rudimentary knowledge of systems of linear equations, and he knows next to nothing about the subject of quadratic forms. Students in this condition, if they receive any algebraic instruction at all, are usually plunged into the detailed study of some special branch of algebra, such as the theory of equations or the theory of invariants...[but that could surely not have been written in 1987]."

The preface goes on to explain that a part of the purpose of the exercises at the ends of sections is to supply the reader with at least the outlines of important additional theories; as illustrations Bôcher mentions Sylvester's Law of Nullity, orthogonal transformations, and the theory of the invariants of the biquadratic binary form. Surely no modern author of a book on linear algebra would dare to relegate the first two of those to the exercises, and probably many modern authors of books on linear algebra have no idea of what the third one is all about.

I remember now that I found the book difficult then, unclear, and exasperating, but I no longer clearly remember, in detail, just what it was that bothered me.

Determinants are assumed known (a reminder says that a determinant is "a certain homogeneous polynomial of the nth degree in the n^2 elements a_{ij}." The next sentence reads as follows: "By the side of these determinants it is often desirable to consider the system of the n^2 elements arranged in the order in which they stand in the determinant, but not combined into a polynomial. Such a square array of n^2 elements we speak of as a *matrix*." This initial description is followed by a displayed and italicized formal definition of (rectangular) matrices, and that is followed by a warning: "... a matrix is not a quantity at all, but a system of quantities." A few lines below: "Although... square matrices and determinants are wholly different things, every determinant determines a square matrix, the *matrix of the determinant*, and conversely every square matrix determines a determinant, the *determinant of the matrix*." Something about all this I found more bewildering than helpful, and, in fact, I am prepared to argue that a part of it is outright misleading: what could the definition of determinants be that makes it true that every determinant determines a square matrix?

The next chapter ("The theory of linear dependence") recalls the concept of proportionality and offers a generalization called linear dependence, which aplies to "sets of constants"; the concept of linear dependence for polynomials is defined separately as something resembling the one first defined, but the two discussions are not presented as special cases of a common generalization. The chapter ends with a section on geometric illustrations, which allows one to speak of the linear dependence of points, circles, and "complex quantities." ("A set of n ordinary quantities is nothing more nor less than a complex quantity with n components.") Linear equations come next, and the chapter *begins* with a statement of Cramer's rule; the proof is assumed known.

As I look at the book now exasperation is my principal reaction: why is this beautiful stuff made to look so awkward and so ugly? "Symmetrical matrices" are defined in the chapter titled "Some theorems concerning the rank of a matrix," and the following statement is proudly displayed as a theorem in that chapter: "If all the $(r + 1)$-rowed principal minors of the symmetrical matrix **a** are zero, and also all the $(r + 2)$-rowed principal minors, then the rank of **a** is r or less." Why, why, why did I need to learn that in order to get a good grade in Mathematics 71?

Chapter VI, the longest in the book, is about linear transformations. Matrix multiplication is introduced here, "suggested by the multiplication theorem for determinants," with a footnote: "Historically this definition was suggested to Cayley by the consideration of the composition of linear transformations...."

By "higher" algebra Bôcher meant a lot more than linear algebra; the lion's share of the book, the central chapters VII–XIX treat mainly invariant theory, bilinear and quadratic forms, and polynomials (including the theory of symmetric polynomials). Linear algebra comes back in a blaze of glory in the last three chapters: they are about λ-matrices and pairs (of bilinear and quadratic forms).

A λ-matrix is a matrix whose entries are polynomials in one variable, which is called λ. The main use of λ-matrices comes from the connection between a numerical matrix A and the matrix $A-\lambda I$ (where I is the identity). The point is that the similarity theory of the one is related to the equivalence theory of the other; phrases such as "elementary divisors" and "invariant factors" are at the center of the stage.

Very few people still remember the book, and their memories of it are not always affectionate. May it rest in peace.

6. **Leonard E. Dickson**, *Modern algebraic theories* (1926)

At the universities that I knew about in the 1930s the two main competitors for the official text of a graduate course in algebra were van der Waerden and Dickson, and Dickson wasn't in German. I knew about van der Waerden, but the course I took used Dickson, and that's what I bought (secondhand) in August 1935.

The preface contains a snide crack at Bôcher; it says, smugly "We...avoid the extraneous topic of matrices whose elements are polynomials in a variable and the 'elementary transformations' of them." Chapters I and II treat invariants, Chapters III–VI linear algebra, Chapters VII–XIII Galois theory, and Chapter XIV group representations.

I feared and respected the book. The exposition is brutal — correct but compressed, unambiguously decipherable but far from easy to read. Thus, for instance, the rational canonical form, probably the deepest and most important result of abstract linear algebra, is stated in a language I couldn't understand then, and I think I understand it now only because I think I know what the theorem says. Here is how it looks.

"By the introduction of new variables which are linearly independent homogeneous linear functions of the initial variables w_i with coefficients in the field F, any linear transformation S with coefficients in F may be reduced to a canonical form defined by

$$(1) \qquad X_1 = x_2, X_2 = x_3, \ldots, X_{a-1} = x_a, X_a = [x_1, \ldots, x_a],$$

[where the bracket denotes a homogeneous linear function of $x_1, \ldots, x_a$ with coefficients in F] and

$$(2) \qquad Y_1 = y_2, Y_2 = y_3, \ldots, Y_{b-1} = y_b, Y_b = [y_1, \ldots, y_b];$$

$$(3) \qquad Z_1 = z_2, Z_2 = z_3, \ldots, Z_{c-1} = z_c, Z_c = [z_1, \ldots, z_c];$$

etc., where a is the maximal length of all possible chains, b is the maximal length of a chain whose leader is linearly independent of $x_1, \ldots, x_a$, and c is the maximal length of a chain whose leader is linearly independent of $x_1, \ldots, x_a$, $y_1, \ldots, y_b$."

Clear? The modern statement must be accompanied by a preliminary definition (companion matrix), as must Dickson's statement (chain, length, leader); it says that every linear transformation has a matrix that is a direct sum of companion matrices.

The general concept of a field is never defined. "A set of complex numbers is called a *number field* if..." and "...all rational functions of one or more variables with coefficients in a number field from a *function field*." The concept of "abstract group" is defined, as a sort of afterthought, in small print, but only permutation groups are ever studied and used (in Galois theory). The first condition that the definition imposes is that "every product of two elements and the square of each element are elements of the set." Dickson is not the only mathematical writer of his generation who was reluctant to use "two" so as to include the possibility of "one" (for two non-distinct objects, if that means anything). Linguistic mores change as time goes on.

With minor exceptions, the contents of the book are still alive and still being taught, but its language and its methods are hopelessly out of date. They could be polished and modernized, but anyone who could do that could write a better book of his own; I think I am safe in predicting that the book is doomed to molder in the archives.

7. **B. L. van der Waerden**, *Modern Algebra*, two volumes (1931)

Which books did *everybody* know about back in the 1930s and 1940s? The answer to that question certainly does not include all the books that I am reporting on, but it includes some of them. Everybody knew about Granville, Smith, and Longley, and probably everybody knew about Landau, and maybe everybody knew about Whittaker and Watson. There is no question in my mind, however, that van der Warden's book is on the list; it was the standard source of quality algebra. Every respectable Ph.D. program included at least one algebra course, and van der Warden's book had very little competition when it came to choosing the text. If it was your turn to teach such a course, and if you didn't think that your students could cope with a good course, then you might have turned to Dickson, or to one of Albert's cloudy expositions — but on the graduate level it was van der Waerden nine times out of ten.

The book was, at that time, available in German only, and that was an obstacle. The German is, to be sure, not as difficult as German can be, perhaps because van der Waerden is Dutch, and the obstacle was frequently ignored. (Did you ever read something by Hermann Weyl?) For many students the book served a double purpose: you learned German from it at the same time that you were learning algebra.

The preface to the first edition confesses that the book is based on Artin's Hamburg course in 1926, but van der Waerden wants to be sure that he gets the credit due him. Artin's material, he says, has undergone so many reworkings and extensions, and the book includes the contents of so many other lectures and so much new research, that its roots in Artin's teaching could be found with great difficulty only. In the second edition, written six years later (and that's the one I am looking at), van der Waerden tells us that he tried to make the first volume usable by beginners; except for determinants, which are used very little, the book is self-contained. (I sympathize; determinants are extraordinarily messy to expound, and the reward is relatively small. It's a pity that they are indispensable.) The second edition of the second volume appeared three years after that of the first, and there too, we are told, hardly a chapter was left untouched.

If you look at the book you'll understand why it was held in such high regard. It has a lot of good material, it is arranged well, it is written clearly — it is just plain good. It has a brief introductory chapter on set theory, whose purpose is, at least in part, to establish the notation. One aspect of the notation I found curious: inclusion and intersection are $\subset$ and $\cap$, as they still are for most of us, but union is $\vee$, which is a surprise; the intersection of a large family of sets is denoted not by $\bigcap$ but by $\mathfrak{D}$ (standing, presumably, for Durchschnitt).

Once all that is finished, algebra proper begins: groups, rings, ands fields, of course, and everything else algebraists can teach us. We learn about polynomials, about when they are irreducible, and what you can do when they are symmetric; we learn about equations and when they can be solved (Galois theory); and at the end of the first volume we learn about infinite field extensions and valuations. The second volume is harder — some of it has the flavor of algebraic geometry. It talks about elimination theory, polynomial ideals, and "hypercomplex systems" (= algebras). Linear algebra is in the second volume also, but it is not the stuff the sophomores in the business school want. It approaches the subject via modules over rings, turns to skew-fields, proves the fundamental theorem for finitely generated abelian groups, and, when it finally condescends to vector spaces, it treats, efficiently, the rational canonical form of matrices, elementary divisors, and at the end, a little of the unitary geometry associated with Hermitian and unitary matrices.

Sprinkled throughout the book there are problems. Their number is not large, but their level is high. One of them asks for the construction of a field with four elements, and one of them asks for a proof that a finite integral domain is a field. Which elements of a cyclic group are generators, asks one, and when do two polynomials have a common factor of degree at least k, asks another. One problem asks for a proof that the equation $x^5 - 4x + 2 = 0$ is not solvable by radicals, and another asks for the pairs of consecutive integers between which the roots of the equation $x^3 - 5x^2 + 8x - 8 = 0$ lie. If you can do all that, can you find all valuations of the Gaussian number field ($= \mathbb{Q}(i)$)?

Why is van der Waerden's book no longer as fashionable as it once was? Is the material in it superseded by now, and is the treatment old-fashioned? Are there many books as good or better?

8. Paul R. Halmos, *Finite-dimensional vector spaces* (1942)

The original edition of this book was Number 7 in the famous Princeton series, Annals of Mathematics Studies; its title was "Finite dimensional vector spaces." Do you see the difference? No hyphen. Al Tucker was one of the editors of the Annals Studies in those days, and he phoned me one day, when he was getting ready to send the manuscript to be printed and bound. Being a carefully and classically educated Canadian, he knew that the hyphen belonged there, but being a conscientious and intelligent editor, he knew that such decisions are up to the author, and he let me have my stubborn way. Later I became converted. Al was right all along; the hyphen belongs there.

The book was inspired by von Neumann's lectures at the Institute in 1939–1940. Feeling the need to share my newly acquired wealth with the mathematical generation coming along behind me, next year I offered a course at Princeton University on what wasn't yet called linear algebra. Ed Barankin and Al Blakers took the course, and prepared a set of notes titled "Elementary theory of matrices"; they were mimeographed and a few copies were sold around Princeton in 1941. The book under review here was based on those notes.

When the manuscript of the book was finished, I submitted it to von Neumann, an important member of the editorial board of the Studies; he received it in good spirits, and assured me that there would be no trouble about its acceptance. His prediction was wrong; a couple of days later he called me in and told me that there was, after all, some trouble. Some of his editorial colleagues didn't think that the Studies was the right place for a textbook, and they wanted to reject it. Johnny disagreed with them, and, before long, he won them over.

For 16 years FDVS (as I refer to the book in my records) was a "best seller," or so I was told. Bohnenblust told me once that it virtually supported the whole Studies series for a while. The first six entries (I no longer remember their titles) were serious pieces of mathematics, at or near the research level, and, consequently, they didn't sell as well as a piece of expository writing that seemed to fill a badly felt need. I had to rely on friendly rumors for sales information about FDVS — since the Studies paid no royalties, I had no official way of learning the facts.

In 1958, as co-editor (with J. L. Kelley) of a newly formed series of undergraduate books published by Van Nostrand, I invited myself to submit FDVS to that series. The Princeton Press acted gentlemanly about the deal; they sold me the copyright for the proverbial one dollar (which never actually changed hands). Later, when Van Nostrand was absorbed several times by other publishers and conglomerates, the series ceased existing, and the book was taken over by Springer.

The differences between the body of the current edition and the original one are hard to find: they are minor additions and minor changes in style and order. The main difference is the set of over 300 exercises, a vitally necessary part of any textbook.

Everybody and his uncle has written a linear algebra book in the last twenty years or so, but that wasn't true in 1942 when FDVS came out. The subject is a basic part of mathematics and as such, like calculus, it hasn't changed much in the last 45 years. Mathematics grows, terminology changes, and the way people look at things isn't the same now as it was in 1942. For those reasons, if I were to write the book now, I might do things slightly differently but the emphasis is on *slightly*. FDVS was a good book when it appeared, and then it became perhaps the most influential book on linear algebra for a quarter of a century or more. As the years go by more and more people seek me out and tell me that they (or their father) learned linear algebra from FDVS. It's still a good book, and it's still in print; it sold 628 copies in 1986.

9. C. L. Siegel and R. Bellman, *Transcendental numbers* (1947)
Carl Ludwig Siegel, *Transcendental numbers* (1949)

I am too far from being an expert on the subject to review this book (these books?), but I cannot resist the temptation to pass along the story I heard about it-them. Siegel was professor at the Institute for Advanced Study for a short while, and, in particular he lectured there in 1946 on transcendental numbers. One member of his audience (possibly his official assistant that year) was Dick Bellman, who took notes and prepared them for publication. In due course the notes appeared, and, being in Princeton at the time, I rushed out to buy a copy (full of good intentions about reading it). Very soon thereafter (a few days, weeks, or was it months later?) the book was withdrawn, the unsold copies that the Princeton University Press had were destroyed, and, the way I remember it, the Press even made an attempt to have people who had already bought the book return it.

The reason for all this, as the story reached me, was that the publication process somehow made an end run around Siegel, and he was not aware just what the book contained and how the contents were treated. The "foreword" (distinct from the preface) says, in part, that "Professor Siegel has not been able to look over the final manuscript because of his absence in Europe, but he has agreed very kindly to publication of this Study without further delay." When Siegel finally got to see the book, he was horrified, and he made a scene: it was outrageously bad, he wanted to have nothing to do with it, and he insisted that it be withdrawn from publication. That's the story, as it sticks in my memory. The documentable fact is that I have two identical looking orange books before me, titled as I indicated above, both of them called Annals of Mathematics Studies, Number 16. The second one (the one that does not have Bellman's name on it) came out two years after the first. I leafed through them, and I can tell that there are minor differences between them, but I cannot tell that the first version is wrong or unclear or inaccurate or in any way bad anywhere — to do so would take either very careful reading by an amateur or a knowledgeable look by an expert.

Bellman's version has a page and a half of preface, titled "Historical Sketch of the Theory of Transcendental Numbers"; it reads fine. Section 1 of Chapter I contains the same result in both versions. Bellman makes it

"**THEOREM 1.** e is irrational",

where Siegel just dives into the proof—the very first sentence is

"The usual proof of the irrationality of e runs as follows."

Bellman presents the proof by contradiction (he begins with "...we assume... that $n!e$ will be an integer for n sufficiently large"), and Siegel does not (he ends with "$n!e$ is never an integer...in other words, e is irrational)."

My leafing through revealed several such differences, and, for all I know, in some cases they could have been important ones. Bellman's English is correct, educated, American mathematese; Siegel's is correct but occasionally surprising. Example: "Gelfond's proof was carried over to the case of a real quadratic irrationality b by Kusmin in 1930. However, this method fails if b is an algebraic irrationality of degree $h > 2 \ldots$." The word "irrationality" here seems to be an unusual way of saying "irrational number." Bellman's bibliography is arranged chronologically (which I consider deplorable), contains a paper by Siegel, and gives the title of Lindemann's paper as "Uber die Zahl π"; Siegel's bibliography is arranged alphabetically by author, omits reference to Siegel's paper, and gives Lindemann's paper as "Ueber die Zahl π."

§3. Set theory and foundations

10. Felix Hausdorff, *Grundzüge der Mengenlehre* (1914)

This book is beautiful, exciting, inspiring — that's what I thought in 1936 when I read it, avidly, like a detective story, trying to guess how it would end, and I still think so. It has certainly been an influential book. Cantor was the first to see the heavenly light, but Hausdorff, a leading apostle, spread the word and converted many of the heathen.

The book does not pretend to start from scratch. It could do so, Hausdorff assures us; integers, rational numbers, real numbers, complex numbers could all be defined and their properties derived within set theory, but they are already treated elsewhere, well enough, so let's get on with something new.

Hausdorff begins the new by telling us that nobody can tell us what a set is; the best anyone can do is offer an intuitive indication of where to look for examples. Finite sets are easy to come by, but a theory of finite sets would be nothing more than arithmetic and combinatorics, and that's not what set theory is all about. Cantor's vision, the theory of infinite sets, is the foundation of all mathematics. Paradoxes are worrisome — the foundation of the foundation is not (not yet?) totally trustworthy — but Hausdorff urges us to go cheerfully forward. The paradoxes of infinity are paradoxes only so long as we insist, with no justification, that the laws for finite sets continue to apply, with no change.

This kind of philosophizing doesn't take long; by page 3 we are learning about the set operations, and about sets of sets, and about sets of sets with structure (such as σ-systems and δ-systems). Functions, and in particular one-to-one correspondences, make their appearance (they are certain sets of ordered pairs — what else?), and the ground is prepared for the real action, which is the theory of cardinal and ordinal numbers. That's where I began to be really excited; that's the beginning of the theory of how to count everything.

The heart of the book is Chapters 3, 4, 5, and 6 (there are ten altogether), about cardinal numbers, order types, ordinal numbers, and the connection between ordered sets and well-ordered sets. We quickly learn that no matter what you do to $\aleph_0$ you still get $\aleph_0$ (well, almost no matter what you do); $\aleph_0+1$ is $\aleph_0$, and so is $\aleph_0 + \aleph_0$; $1 + 1 + 1 + \ldots$ is $\aleph_0$, and so is $1 + 2 + 3 + \ldots$; $2\aleph_0$ is $\aleph_0$, and so is $\aleph_0 \times \aleph_0$. But then we learn that $2^\alpha > \alpha$ for every cardinal number α, by an argument that is just like the one that leads to the Russell paradox, except that here the argument is right, it leads to no trouble — and infinities are suddenly seen to stand on the shoulders of infinities, reaching ever higher and higher.

Which of all those infinities enter analysis? What are the cardinal numbers of the various sets, such as the ones formed by irrational numbers, or closed sets, or continuous functions, the objects we work with every day? It doesn't take long to find out — all those have the power (what a curious word!)

$$2^{\aleph_0}.$$

If, however, we consider Lebesgue measurable sets or functions, then the set of them has power

$$2^{2^{\aleph_0}},$$

and that's surely the largest cardinal number any sane analyst ever wants to hear about.

Next we learn about order, and there too a whole new firmament becomes visible. Ordered sets can be isomorphic — the technical word is "similar" — and the equivalence classes for that relation, called order types, can be added and multiplied, and almost every time we look at an old friend from the point of view of order types we get a titillating surprise. To add α and β, just write down α and follow it by β — what could be more natural? — and conclude therefore that $1+\omega$ is not equal to $\omega+1$. Once you learn that, you realize that you have always known it, but it was a surprise just the same. To follow 0 by $\{1, 2, 3, \dots\}$ gives $\{0, 1, 2, 3, \dots\}$, which is, except for notation, the same as $\{1, 2, 3, \dots\}$, but to follow $\{1, 2, 3, \dots\}$ by 0 gives an ordered set that has a last element, which is something new. To multiply α by β, just write down α as many times as β indicates. So, for example, 2ω (where ω is the order type of $\{1, 2, 3, \dots\}$) is represented by

$$0, 1, 0', 1', 0'', 1'', \dots,$$

whereas $\omega 2$ is represented by

$$1, 2, 3, \dots, 1', 2', 3', \dots,$$

a horse of a very different color.

The ordered sets in these examples are as simple as infinite ordered sets can be; the more complicated ones are even more interesting. Consider, for instance, countable ordered sets that are "open" (no first or last element), and "dense" (between any two elements there is a third). Example: the set of rational numbers. Can you think of another example? There aren't any! If we regard similar ordered sets as identical, then the rational numbers yield the only example of countable open dense sets.

Well-ordering (a powerful concept, an ugly word) is next. The order types of the well-ordered sets are the ordinal numbers, and Hausdorff lets us have the startling and nontrivial theorems about them almost faster than we can read. Any two ordinal numbers are comparable (which is not true of more general order types), any set of ordinal numbers is well ordered, and there is an infinitely infinite variety of countably infinite ordinal numbers. Ordinalnumbers can be used in transfinite induction, the most powerful method of classical set theory, and ordinal numbers can be used to index (by magnitude) the cardinal numbers, and thus lead us to frightening and unanswerable questions, such as whether

$$\aleph_1 < 2^{\aleph_0} \quad \text{or} \quad \aleph_1 = 2^{\aleph_0}.$$

The arithmetic of infinite cardinals and ordinals is still a live subject, and it is, if anything, more frightening than it was in Hausdorff's day.

The rest of the book treats subjects that are more familiar nowadays: topology and measure theory. The first and second axioms of countability make their appearance, metric and in particular Euclidean spaces are introduced, the set of points of convergence of a sequence of real-valued continuous functions is discussed (what kind of sets must they be?), the Lebesgue integral is defined, and the last theorem in the book states that every continuous function of bounded variation is differentiable almost everywhere.

Yes, I found the book beautiful, exciting, and inspiring, and I still love it — but by now I couldn't use it as a text. The language and the notation did not survive unchanged (union is called addition, an indexed set is called a complex), and while the statements and proofs are clean and elegant, the emphasis and the point of view are dated. I am glad Hausdorff wrote the book, and I am glad I read it when I did.

11. Garrett Birkhoff, *Lattice theory* (1940)

Some books turn out to have an influence on the mathematical world, but it is rare that one is written with mainly that end in view; this one was. Garrett Birkhoff was 29 when the book appeared, an assistant professor at Harvard, and he thought it "desirable to have available a book on the algebra of logic, written from the standpoint of algebra rather than of logic." He confesses to some timing (priority?) problems. Thus, for instance, he calls attention to the existence of many papers by Ore, "applying lattice theory to groups" [one of which appeared about a year before this book], and then says: "The author regrets being unable to give a more adequate account of these researches, owing to his having finished Chapters I–IV in May, 1938."

It's not a fat book (155 pages), but in subsequent editions it grew (to 283 pages and then to 413 pages). At the end of the first edition there are 17 unsolved problems. The second edition (1948) reports that "eight have been essentially solved" and contains, sprinkled throughout, a total of 111 unsolved problems; the third one (1967) has 166. Here are the first and the last of those. "1. Given n, what is the smallest integer $\psi(n)$ such that every lattice with order $r \geq \psi(n)$ elements contains a sublattice of exactly n elements? 166. Which abstract rings are ring-isomorphic to ℓ-rings [ℓ for lattice]?"

The language of the first edition is different from that of its successors. It speaks, for instance, of the direct union of algebras (which became direct product later). It describes the "somewhat complicated general notion of a 'free' algebra" in terms of "certain identities" and "functions" of the elements; in the last edition the concept is described in the morphism language advocated by categorists.

The word "application" occurs frequently, but in a way that is uncertain to convert the heathen; what it seems to mean is the possibility of expressing some of the results and problems of the target subject in the language of lattice theory. This usage is especially visible in a brief section titled "application to the four color problem" and in the three final chapters (of the first edition), which are "applications" to function theory, logic, and probability. Sample theorem from Chapter VII: "Metric convergence is equivalent to relative uniform star-convergence, in any Banach lattice." Sample theorem from Chapter VIII: "Propositions form a Boolean algebra." Sample theorem from Chapter IX: "Let T be any transition operator on any space (AL). Any element whose transforms under the iterates of T are bounded lattice-theoretically is ergodic."

The book is definitely trying to make converts; it doesn't fail to emphasize the value of the subject. Here is a sample footnote. "The abundance of lattices in mathematics was apparently not realized before Dedekind...Following Dedekind, Emmy Noether stressed their importance in algebra. Their importance in other domains seems to have been discovered independently by Fr. Klein... K. Menger..., and the author."

Did the book make converts? Was the book influential? The number of people who know about lattices is certainly greater now than it was 50 years ago, and so is the number of papers on the subject, but in its effect on, and its genuine applications to, other parts of mathematics, lattice theory has a long long way to go before it becomes comparable with group theory — a comparison that lattice adherents are fond of making.

12. Stephen Cole Kleene, *Introduction to metamathematics* (1952)

There have been many logic books before this one and many after it, but only a few that hit the spot so successfully. Many mathematicians of my generation were confused by (I might go so far as to say suspicious of) the Gödel revolution — the reasoning of logic was something we respected, but we preferred to respect it from a distance. It looked like a strange cousin several times removed from our immediate mathematical family — similar but at the same time ineffably different.

Kleene's book was heartily welcomed because it told the truth and nothing but the truth. It's a no-nonsense book. Its point of view is that of the strange cousin several times removed — strictly the logical, recursive, axiom-watching point of view (hairsplitting if you feel like speaking unkindly, but the hairs are there to be split). It is not the point of view of most working mathematicians, the kind who know that the axiom of choice is true and use it several times every morning before breakfast without even being aware that they are using it. For Kleene, set theory is a suspicious subject — he regards everything other than a finitary (constructive?, intuitionistic?) foundation as unsafe. He writes, for instance: "While our main business is metamathematics, the extra-metamathematical conceptions and results of set-theoretic predicate logic may have heuristic value, i.e. they may suggest to us what we may hope to discover in the metamathematics. ... In terms of the set-theoretic interpretation, the completeness of the predicate calculus should mean that every predicate letter formula which is valid in every non-empty domain should be provable. This interpretation is not finitary, unlike the corresponding interpretation for the propositional calculus..., and so the corresponding completeness problem does not belong to metamathematics."

Part I is a long essay on the problem of foundations; the first chapter is on set theory. The treatment includes a proof that every infinite set has a countably infinite subset, but a couple of chapters later Kleene is careful to point out that the proof uses non-constructive reasoning. The attitude of Part I is that of a doctor who describes the symptoms of a disease — he doesn't "disapprove" of them, but he intends to cure them. In Part II, Kleene begins the therapy. "The formal system will be introduced at once in its full-fledged complexity, and the metamathematical investigations will be pursued with only incidental attention to the interpretation." Step one (and here is where many mathematicians begin to get nervous): the formal symbols to be used are listed. Today, under the influence of computers, to whom we have to speak V-E-R-Y P-L-A-I-N-L-Y, that's considered less odd than it was in the halcyon days of Cantorian purity, when a set was a set and it seemed pointless to think about what letter should be used to denote it. Kleene lists the formal symbols (including parentheses), explains the permissible ways of stringing them together, counts parentheses with care, cautions us about free and bound variables, and then lists the postulates of elementary number theory (each of which is, officially, nothing more than a correctly formed finite string of formal symbols). As an example of how these tools are to be used he gives a formal proof of the formula "$a = a$"; it takes 17 steps.

That's how it goes, and that's how it continues. There is a section on the introduction and elimination of logical symbols (e.g., if A is provable and if B is provable, then $A\&B$ is provable, and conversely), and there is a detailed treatment of the propositional calculus (e.g., if A and B are formulas, then the equivalence of $A\&B$ and $B\&A$ is provable). The next topic is the predicate calculus, which is more complicated but whose discussion is conducted in the same meticulous spirit, followed by formal number theory (if a and b are symbols for natural numbers — that is, non-negative integers — then the implication from "$a + b = 1$" to "$a = 1$ or $b = 1$" is provable). The climax of Part II is Gödel's theorem: if the number-theoretic formal system is consistent, then it contains an undecidable formula — and that's a little less than half of the book.

The second half is recursive function theory and a few additional topics: general and partial recursive functions (Church's thesis), computable functions (Turing machines), and a discussion of consistency of classical and intuitionistic systems.

I am still uncomfortable in the presence of the kind of logic that must study lists of symbols and rules for stringing them together, insecure and *therefore* uncomfortable, but I salute Kleene's book as an early and honest explanation of such things. I bought it when it came out, and in the intervening 35 years I looked at it often, grumbled at it sometimes, and was grateful to it always — it deserves the high regard in which it is held and the influence it has exerted.

13. **Edmund Landau**, *Grundlagen der Analysis* (1930)

Most graduate students of my generation learned of this book, perhaps through the grapevine, or from an instructor's recommendation, or by a serendipitous encounter in the course of a desperate search for some light to show the way through the dark tunnels they were facing, and all the ones I knew loved it and were grateful for its existence. Landau was a great number theorist, and an uncompromising believer in the "Definition, Satz, Beweis" style of exposition, with an occasional, grudging, "Vorbemerkung" thrown in. His books on number theory are not easy to read, but they can be read. Everything is there, and all you have to do to learn the material is to go through the presentation, word by word, line by line, paragraph by paragraph. Landau doesn't bother to give you the motivation; if you don't want to watch this show, don't but a ticket. But he does manage to give you the grand understanding, the picture as a whole, over and above the fussy, line by line details: his organization is masterful, and when you reach the climax you know that you have learned something and you know what you have learned.

This little book was not really up Landau's alley, and in the preface he apologizes for having written it ("I publish in a field where I have nothing new to say"). He felt, however, quite strongly, that it had to be written, and nobody else had done so. The purpose of the book is to build up the foundations of analysis in the mathematical (as distinct from the logical) sense of that phrase, that is, to start from the Peano axioms and develop the necessary properties of real and complex numbers. In many analysis courses (beginning with calculus) this purpose is either not accomplished at all, or is accomplished by a vague wave of the hand. To get the real numbers, the instructor indicates how the definitions of addition and multiplication and order might be based on the Peano axioms, and then assures the audience that it works — the result is a complete ordered field, so there! Landau gives no such assurances; he goes through the details, all the details, with care, so much care that even an intelligent computing machine might enjoy reading the book.

Section 1 of Chapter 1 starts right in, with no waste motion. "We assume given: a set, that is a collection, of things, called natural numbers, with the properties, called axioms, to be enumerated below." The axioms are, in fact, the Peano axioms, and, once their statement is before us, Landau turns to Section 2, and, again with no waste motion, presents Satz 1: "From $x \neq y$ it follows that $x' \neq y'$." The highest number in the book is that of Satz 301.

There is a joke about Landau's style, according to which it is quite possible in a Landau book to be presented with a Definition and a Satz followed by these sentences. "Vorbemerkung: Der Satz ist nicht trivial. Beweis: Klar." Nothing like that occurs in this little book (139 pages). "Beweis: Klar" is indeed a tool that Landau uses (elsewhere), but I looked all through *this* book and found no instance of it. The book has five chapters: Natural numbers, Fractions, Cuts, Real numbers, and Complex numbers. The high point (Satz 205) is called Dedekind's theorem (every Dedekind cut is determined by a real number); this is, of course, the "complete" part of a complete ordered field.

I referred above to the preface, but there are two of them, one "for the student" and the other "for the scholar." (I challenge the translation of "Kenner" as "scholar," or, for that matter, the translation of "Lernende" as student. In German the contrast between the "learner" and the "knower" comes across clearly, and in this context it is a pity that the latter isn't really an English word.) Landau's scholarly apology for having written the book is in the knower's preface; near the end of that same preface he writes as follows. "I hope that, after a preparation stretching over decades, I have written this book in such a way that a normal student can read it in two days. And then... he may even forget the whole content except for the axiom of induction and Dedekind's main theorem."

Do the students (the learners) of today still read Landau? Should they? Shouldn't they?

§4. Real and functional analysis

14. Constantin Carathéodory, *Vorlesungen über reelle Funktionen* (1917)

My copy of this book, acquired in 1948, was a photographically reproduced version of the second edition, copyright 1927, published in 1948 by the Chelsea Publishing Company. The copyright of this version is "vested in the Alien Property Custodian... pursuant to law"; the book was "published and distributed in the public interest by authority of the Attorney General of the United States...." That sort of thing went on quite a bit during and after the second world war.

I thought the book beautiful when I read it as a student (long before 1948); looking at it now, I still respect its organization, its clarity, and its honesty, but I am surprised how dated it has become.

The book is dedicated to "my friends, Erhard Schmidt and Ernst Zermelo." The preface says that by now (which means 1917) the changes that Lebesgue's work has produced in the theory of real functions can be considered to have come to an end, and it seems therefore necessary that the subject be rebuilt systematically, from the ground up; the purpose of this book is to accomplish just that.

The care and attention with which the contents are presented are admirable and lovable. Since real numbers are at the basis of it all, the introduction explains what the book will mean by them. The answer is that they form a complete ordered field (that language is not used, but the concept is defined in detail), and that's where the action begins. Chapter I is set theory, but all sets, here and elsewhere in the book, are subsets of $\mathbb{R}^n$. The union of two disjoint sets A and B is denoted by $A + B$ (if they are not disjoint the $+$ sign is decorated with a dot on top), and the intersection by AB. The chapter discusses countable sets, proves that $\mathbb{R}$ is not countable, and proves (explicitly referring to the axiom of choice) that every infinite set has a countably infinite subset. The chapter contains the requisite topology too: closed sets, open sets, perfect sets, closures, cluster points, condensation points, Borel's covering theorem (= Heine-Borel in the class that I took), and Lindelöf's covering theorem.

Chapters II and III are analysis in the usual sense of the word. The concept of sequence is recognized as a special case of the concept of function (that was not a cliché in those days), and the study of limits includes the notions of lim sup and lim inf for sequences of *sets* (lim sup and lim inf even for sequences of numbers are causes of insecurity among the graduate students that I have been seeing in the last ten years or so). For functions, words such as semicontinuity, variation, and uniform continuity play an important role.

Chapter IV generalizes to $\mathbb{R}^n$ the preceding material about the metric and the topology of $\mathbb{R}^1$, with emphasis on the concept of connectedness. Chapters V–X are mainly about measure theory: the concept that is now known as Carathéodory outer measure begins it all, measurability is defined, enough linear algebra is quickly built up to study the invariance properties needed to understand the usual (Vitali) construction of non-measurable sets, measurable functions are defined (with the discussion including the Baire classes, but for the finite ordinals only), and the Lebesgue integral is defined in terms of the measure of the ordinate set. Chapter XI, the last, discusses functions of several variables, and in particular the Fubini theorem. Like some of the other books in this report, this book is not a slim one to be slipped into your pocket: it's a biblical looking hefty 718 pages.

Why do I think the book is dated? The reasons are partly the terminology, partly the old-fashioned notation, partly the inclusion of subjects that have become almost maximally unfashionable (such as condensation points, Baire classes, and ordinate sets), and partly the gentle, thoughtful, very professional, didactic attitude that shines through every page. When bad books die, they go to the archives; isn't there a more nearly heavenly repository for the good ones?

15. Stanisław Saks, *Theory of the integral* (1937)

This is not really a book on measure theory, but for me and many students of my generation it was the main source of measure theoretic wisdom in the 1930s and 1940s. The first three chapters were the ones of greatest interest to me ("The integral in an abstract space," "Carathéodory measure," and "Functions of bounded variation and the Lebesgue-Stieltjes integral"); the focus of the others was on the modern-classical theory of the differentiation of functions of real variables and on the strange integrals associated with the names of Perron and Denjoy. At the end of the book there are a couple of notes by Banach, occupying a total of only 18 pages; they are titled "On Haar's measure" and "The Lebesgue integral in abstract spaces." The version that I was first exposed to was in French (Théorie de l'intégrale, 1933), but as soon as L. C. Young's translation appeared, I ordered my copy; I received it in October 1973. The editorial board of the series in which the book appeared is noteworthy; it consists of S. Banach, B. Knaster, K. Kuratowski, S. Mazurkiewicz, W. Sierpiński, H. Steinhaus, and A. Zygmund — an impressive list.

Chapter I defines the concept of measure ("additive functions of a set") and bases integration on it. Its 38 pages are practically a complete book — it's hard to think of an important theorem about the subject that is not there. Chapter II shows that sometimes (e.g., in metric spaces) measure can be obtained from other, less restrictive, set functions (such as outer measures). As dividends, the same chapter proves that analytic sets are measurable and discusses the Baire category theorem. Chapter III treats the theorems associated with the names of Lusin and Fubini, and discusses bounded variation and absolute continuity. Some of the terminology is old-fashioned by now, but the treatment is clean, simple, rigorous — it was a pleasure to study.

Many sections of the chapters I am not discussing in detail are classical and permanent parts of analysis, and continue to appear in modern texts.

Thus, for example, Chapter IV (on the differentiation of interval functions) contains Vitali's theorem, the Lebesgue decomposition, and the facts about points of density. Chapter V is about area; the chapters that follow treat what in the 1930s I classified as the recondite parts of the subject (and still do).

Banach's discussion of Haar measure is not the best that can be given by now, but it was helpful to a lot of us then; it is based on what has come to be called the Banach limit. The last piece of mathematics in the book, Banach's treatment of abstract integration, is in counterpoint to Saks's set-theoretic one; it is the non-measure-theoretic linear functional approach.

A personal note: I learned measure theory from Saks, and I found the book inspiring. Years later it inspired me, for instance, to write a book about measure theory, but not because I found anything wrong with Saks. I wanted to add things that Saks didn't have, I wanted to organize the material differently, and I wanted to reach a less sophisticated audience. I had Saks before me as I was writing, and I am grateful for another giant shoulder that I had a chance to stand on.

16. Stefan Banach, *Théorie des opérations linéaires* (1932)

I bought my copy in June 1937, a year before I got my Ph.D, and rushed right over to Wascher's bindery to convert it from a paperback to something more permanent. The hard buckram binding stood up find through the years. The book is still on my shelf, and I still have occasion to consult it from time to time. It can be regarded as a watershed. Before Banach (the book) functional analysis (a name that didn't come into vogue till much later) was a curiosity; after Banach it became a fad to be eagerly adopted by many and to be sneered at by just as many.

A small group of us (five or six) had a seminar whose purpose was to study just that book. The French was easy even for those of us whose French was weak — it was Polish French.

Banach had some not totally innocent fun in writing the book. A metric space is called an "espace (D)" (for distance?), sets belonging to the σ-algebra generated by closed sets are "ensembles mesurables (B)" (for Borel?), complete metric groups are "espaces du type (G)" (for group?), complete metric vector spaces whose metric topology makes them topological vector spaces are "espaces du type (F)" (for Fréchet?), and the objects that have come to be called Banach spaces are "espaces du type (B)."

The influence of the book is difficult to overestimate; it started an avalanche. Most of it is about Banach spaces, and in 254 pages it touches on all the basic concepts and many of their applications. The Hahn-Banach theorem is there (but Hahn's name is not in the index), the Baire category theorem is applied to prove the existence of nowhere differentiable continuous functions, the conjugate spaces of many of the standard function spaces are explicitly determined, bases, weak convergence, compact operators — the works. Many of the problems that Banach spacers have been working on during the last 50 years are raised in the book, and not all the ones raised are solved yet. There is no question but that the book is a classic. It might have been born with faults, and it might be out of date by now, but it's a classic, and I think that students living at the end of the twentieth century could still profit from looking at it.

17. A. Kolmogoroff, *Grundbegriffe der Wahrscheinlichkeitsrechnung* (1933)

This is (was?) without doubt one of the most important mathematics books of the century. I don't know whether any teacher ever was courageous enough to use it as a text, but I am inclined to doubt it. It's a short book, it contains no exercises, its expository style is mercilessly concise, and the only edition available was, for quite some time, the German one. (An English translation appeared in 1950.) Text or no, however, many students read the book and profited from it; it was the rock that served as the foundation for many lookout towers later. I bought it early, and the margins of my copy are filled with questions and comments in the purple ink I favored in 1937.

Probability theory is a deep part of mathematics and, at the same time, it is a powerful tool for understanding nature. Its origins (questions about gambling) led to some pleasant pastimes and puzzling paradoxes; "deep" was the last word that anyone would have thought to apply to the subject.

When the noncombinatoric, infinite, aspects of the theory began to come to the fore, and, as time went on, the pressure from applications increased, the subject became harder and messier. There didn't seem to be any organizational principle that held it together. That a relatively new part of analysis, Lebesgue measure, had in the technical sense the same structure as probability was slow in being recognized, but recognized it was — it's hard to keep secrets from people like Borel and Fréchet. Kolmogoroff's book is a systematic presentation of that recognition.

The very first section of the book presents a list of "axioms." That's a misnomer I think. The axioms constitute in fact the definition of a probability space: a set, with a specified Boolean algebra of subsets, and on that algebra a probability measure. (The condition of countable, as opposed to finite, additivity comes a few pages later, but once it comes it never goes away.)

The misnomer rubbed some people the wrong way. Uspensky in his good book on the techniques of probabilistic calculation (as opposed to ideas, which is what Kolmogoroff focuses on) writes as follows. "Modern attempts to build up the theory of probability as an axiomatic science may be interesting in themselves as mental exercises; but from the standpoint of applications the purely axiomatic science of probability would have no more value than, for example, would the axiomatic theory of elasticity."

The dictionary connecting probability and measure theory is easy to learn. Events are sets, the probability of an event is the measure of a set, a random variable is a measurable function, an expectation is an integral, independence has to do with Cartesian products, and the laws of large numbers are ergodic theorems. The hardest and most mysterious concept in probability is that of conditional probability (and its functional generalization, conditional expectation). The moment measure theory is recognized as the right way to do things, the difficulties and the mysteries disappear. All you do is apply the Radon-Nikodým theorem and add an entry to the dictionary: a conditional probability is a Radon-Nikodým derivative.

Those are the secrets that Kolmogoroff's book reveals. By now they are well-known secrets, and Kolmogoroff's exposition can be improved, but the book served an almost unsurpassably useful purpose, and we should all be grateful for its existence.

18. Marshall Harvey Stone, *Linear transformations in Hilbert space and their applications to analysis* (1932)

My friend Ambrose got to this book before I did, and he "sold" it to me: he explained that it's not really analysis (which I was scared of) but a close cousin of matrix theory (which, partly because of and partly despite Bôcher, I was beginning to like and understand). Stone began to write it, the preface says, in or near 1928; I bought my copy in 1937. The book was an influential source of operator-theoretic wisdom for a while, but not so influential, and perhaps for not so long a time, as its author might have wished. Wintner's book (*Spektraltheorie der unendlichen Matrizen*) came out three years earlier, but it never really got off the ground; Wintner's matrix point of view was awkward and the von Neumann axiomatic approach was preferred by almost every student of the subject. Von Neumann's first operator papers started appearing in 1929, and they put Wintner and to a large extent also Stone out of business.

Stone knew about the von Neumann work, and referred to it, sometimes in a manner that I thought was wistful. "The initial impetus of my interest came from reading some of v. Neumann's early and still incomplete work,...to which I had access, but which was never published. Thereafter, I worked independently, the results...being obtained without further knowledge of his progress along the same or similar lines." Elsewhere: "[a] recent paper of J. v. Neumann...came to my attention too late to be cited."

This is a large, heavy book (622 thick pages). The applications are in the last chapter (Chapter X), which is more than 40% of the whole; I doubt that anybody ever read it all. The style is formal and ponderous. The definitions are long, and the theorems are longer, consisting of many sentences and frequently filling a half page or more. The theorem that bilinear functionals come from operators via inner products takes almost a whole page to state, and the same is true of the theorem that describes the functional calculus; the various parts of the concept of homomorphism (which is what the functional calculus describes) are listed as part (1),... part (7) of the theorem. Here, to illustrate the style, is a shortie (a medium general version of polar decomposition): "Every maximal normal transformation T for which $\ell = 0$ is not a characteristic value is expressible as the product of a not-negative definite self-adjoint transformation H and a unitary transformation U which is permutable with H." Authors do not usually see themselves as others see them; here is how Stone describes his own writing. "In order to compress the material into the compass of six hundred odd pages, it has been necessary to employ as concise a style as is consistent with completeness and clarity of statement, and to omit numerous comments, however illuminating, which will doubtless suggest themselves to the reader as simple corollaries or special cases of the general theory."

Stone was 29 years old when the book appeared; the title page describes him as Associate Professor in Yale University.

19. H. F. Bohnenblust, *Theory of functions of real variables* (1937)

The dissemination of mathematical information in the 1930s and 1940s was not as efficient as it has since become. Xerox copies and preprints had not been invented yet, the number of new books that appeared each year was finite, and the everywhere dense set of meetings and conferences that we live with today was still far in the future. One ingenious system that did exist was that of "notes." If a good mathematician gave a good course at a good university, the notes for the course were in great demand, and, before long, the system of producing and distributing such notes became a matter of routine. A student or two would take notes during the lecture, the result might or might not be examined, changed, and approved by the lecturer (usually it was), and then it would be mimeographed (how many students in the 1980s have that word in their vocabulary?), and sold. There was no advertising except word of mouth; the usual price was $1.00 or $2.00 or $3.00. The package that reached your mailbox consisted of, say, 200 pages of what looked like typewritten material. One of the earliest, most famous, and best examples is the work here under review, the Bohnenblust notes.

The copy on my desk is paper bound, similar in its orange color to the still extant Annals of Mathematics Studies (which, in fact, replaced Princeton's contributions to the notes market). The date on it is 1937, but almost all the material in it is still very much alive and still very much a part of graduate courses throughout the world. It's not a long work (130 pages), but it is amazingly comprehensive. It is, in fact, a combination course: set theory, general topology, real function theory, measure theory, and even a cautious toe dipped into functional analysis.

The 130 pages are divided into sixteen chapters. The first chapter begins with the Peano axioms and ends with two proofs of the existence of transcendental numbers; cardinal numbers are introduced in the second chapter. Chapter 3 prepares the ground for the metric space theory that comes soon (the inequalities of Schwarz, Hölder, and Minkowski); topological spaces and, in particular, metric spaces come in Chapters 4 and 5. Chapters 6, 7, and 8 are elementary real function theory: convergence, lim sup and lim inf, and infinite series (from the serious classical point of view — Chapter 8 defines the Euler constant and has a section on slowly convergent series). Chapter 9 is probably the only one that would not appear in most courses nowadays: it is on summability theory, a la Cesàro, Hölder, etc. All but one of the remaining chapters are on measure theory; the exception is the functional analysis chapter, where Hilbert space is defined and measure theory is applied to construct a functional model for it. Once again the chapter is serious classical analysis; to give examples of orthonormal sets, Bohnenblust discusses the Legendre, Laguerre, and Hermite polynomials. When measure theory is resumed, it turns to Fubini's theorem, the indefinite integral, functions of bounded variation, and the Riemann-Stieltjes integral. Whew!

I learned a lot of real function theory (and set theory and topology and...) from these notes; the material is there, and it is cleanly, crisply, rigorously, and honestly presented. It is not a "motivated" presentation; it makes no effort to sell you the contents. You have to want to learn the stuff to learn it here, but if you do want to, you sure can.

Boni was a popular teacher, a charming guy, with a pronounced French accent (he is Swiss). He stayed on at Princeton for quite a few years, but eventually he moved to CalTech, and that's where he ultimately retired from.

20. Lawrence M. Graves, *The theory of functions of real variables* (1946)

Lawrence Graves was about ten years old when Lebesgue measure first saw the light of day. He was fifty when this book appeared; he had already been teaching the material at the University of Chicago for twenty years. He could have written most of the book twenty years before he did — by the time it appeared, its spirit had gone out of fashion. It is, for that very reason, an excellent item to discuss among the books of yesteryear — it is more yester than its age indicates.

There are two reasons the book seems (to me) to belong to a much earlier era: content and style.

About a half of what Graves chose to include could still be a part of courses on real function theory today, but at least a half of it is not likely to be. Even back in the 1950s at Chicago, when Graves and some of us young turks taught the course in alternate years, the subjects we covered were different enough to give rise to student complaints. If a student took the course from X one year, it was quite likely that he would want to take the comprehensive MS exam during the next year, when Y was in charge. Since by then X was teaching something else (or, quite possibly, off on a leave of absence far away), it fell to Y to administer both the written and the oral exams, and the questions would be quite different from the ones X would have asked.

Real function theory to Graves meant, reasonably enough, derivatives and integrals; for some of the rest of us the emphasis was more likely to be on operators and L^p spaces. Both sides could speak both languages, of course, but, naturally enough, each of us enjoyed and emphasized one language more than the other, and a newcomer who was taught in the one had difficulty expressing himself in the other.

The book begins with a brief description of the logical notions that Graves regarded as basic to the study of mathematics. I wonder how a modern logician would regard that description. Graves distinguishes between a statement and a statement about a statement — that's fair enough — and he feels free to use the universal quantifier in the latter case as well as in the former (a second order predicate calculus?). In his list of "important logical laws" he writes

$$-(p:\ -p)$$

for "the law of contradiction" (omitting "for convenience" the universal quantifier symbol), and he writes

$$p \vee -p$$

for "the law of excluded middle." He says that he is "asserting these statements to be true." (Are they statements or statements about statements?) One mathematically highly sophisticated but logically innocent friend told me recently about the great bewilderment that Graves's discussion caused him. Taking the set-theoretic, Boolean algebraic, point of view, my friend regards $-(p:\ -p)$ and $p \vee -p$ as equal, and, what's more, equal to the unit element (truth?) in the pertinent Boolean algebra. What then, he demands, is the difference between the law of contradiction and the law of excluded middle?

If A and B are sets, then, for Graves, the union and the intersection are $A+B$ and AB. (Most people frown on that nowadays). Graves uses $\supset$ for "implies" and $\sim$ for "if and only if." (Why not?; but $\Rightarrow$ and $\Leftrightarrow$ are more common today.) Graves uses the out-of-fashion Frege-Peano symbol $\ni$ for "such that." (Why not? I'm fond of it too, but most mathematicians today have no idea what it means. If they have to guess, they are likely to interpret it as the inverse of membership; if $x \in A$, then $A \ni x$.) As a result of his notational conventions, Graves's book has many odd-looking sentences that need to be deciphered before they can be understood. If, for instance, a real-valued function f on a domain S (a subset of $\mathbb{R}$) has a finite derivative at c, then, says Graves,

$$\exists M < \infty . \exists \varepsilon > 0 \ni : x \text{ in } SN(c;\varepsilon) \cdot \supset \cdot |f(x) - f(c)| \leq M|x - c|.$$

These comments are meant to indicate what I mean when I say that the mathematical style of the exposition is not in the spirit of the 1980s. The literary style is almost as formal as the mathematical one, and, in particular, it is always syntactically and lexically correct. It is not difficult to read, but it frequently seems a little stiff. Speaking of set theory, for instance, Graves says: "When any given class of entities is presented for consideration, it is thereupon possible to conceive of a new entity not present in the given class."

As for the content, the first of the twelve chapters is mainly logic, one chapter is on the Riemann integral, one on implicit function theorems, one on ordinary differential equations, and the last one on Stieltjes integrals. It's all good mathematics, but I wonder if it's in the right place; if I were to teach the course now, I'd probably omit all of these chapters.

Uniform convergence gets a chapter all its own, and the Lebesgue integral gets two. The first of those begins with functions of intervals, quickly defines sets of measure zero, and then follows F. Riesz by introducing the integral (for limits of step functions) as a linear functional with appropriate convergence properties. The climax of the chapter (following a treatment of inner and outer measures and Borel measurable sets) is the fundamental theorem of the integral calculus. The second chapter on the Lebesgue integral talks about Fubini, Nagumo (a complicated criterion for uniform absolute continuity), Egoroff, and Riesz-Fischer.

Everyone who writes a teaching book is trying not only to influence but to some extent also to predict the future. Prediction is always hard and risky. People who learn of a theory soon after its birth, where "soon" means within a decade or two, cannot always correctly predict which parts of it will live and which will be sloughed off three or four decades later.

§5. Complex analysis

21. E. J. Townsend, *Functions of a complex variable* (1915)

I didn't know Townsend, but he was important at the University of Illinois (math chairman for a while) and his books were frequently used there even after he retired (and continued to live in Urbana when I was a student there). Used as texts, yes, but not well spoken of: I heard the rumors about how dreadful they were. I bought his complex variable book in 1936, when I was a second year graduate student, but I never understood much of it. Looking at it now, I do not judge it to be dreadful, but it is not very good, especially not at explaining the concepts at the basis of the subject.

There is an unimportant silly mistake early on that indicates nothing more than haste or carelessness, but it is not untypical. In the discussion of Dedekind cuts ("partitions") Townsend uses $\sqrt{2}$ as an illustration (of course!) and he says: "Put into set A_1 all of those rational numbers whose squares are greater than 2 and into A_2 all rational numbers whose squares are less than 2." I think he meant to put -1.6 into A_2, but he was absent-minded about signs.

That's a small thing. What bothers me more is that irrational numbers are never really *defined*; the place where the words occur in bold face, indicating that now the definition has been achieved, reads as follows." ...the partition may be said to define a new number; we shall call such a number an **irrational number.**" Complex numbers are just as bad: "By assuming the additional fundamental unit $\sqrt{-1}$, which we shall represent by i, a very important extension of the number-system thus far discussed can be made."

Townsend is good at the formalism, and seems to have been careful both in choosing what to put in the book and how to treat it, but he is not good at definitions and explanations — not by the standards that mathematicians are used to in the twentieth century. "If a complex number assumes but a single value in any discussion, it is called a **constant.**" Referring to the definition of "function" (of a real variable) offered a little later, he says: "It is to be observed that it is not necessary that x should have every value in an interval; it may take, for example, only a set of values."

I could quote a lot more that sounds strange nowadays, and sounded either strange or difficult or mysterious or wrong when I was a student. Here is one more sample: "If we may assign at pleasure to a number values which are numerically as small as we may choose, then the number is said to be **arbitrarily small.**"

On page 3 (back in Dedekind cuts): "suppose the totality of rational numbers to be divided in any manner whatever into two groups $A_1, A_2 \ldots$," and on page 114: "... the independent variable is replaced by any one of a definite set of its linear substitutions such that the set forms a group." Presumably the early use of the word "group" was just an instance of absent-mindedness. Talking about "linear automorphic functions" Townsend says: "Any portion of the Z-plane which maps into the entire W-plane is called a **fundamental region** of the given function." Surely that can't be right?

There is a discussion of conditional convergence and of the set of possible sums of a conditionally convergent series that I found extraordinarily foggy, and the definition of "branch-point" is difficult to make honest sense out of. The word "connected" is used on p. 20 (in the definition of a region), but is never defined; simply connected is "defined" later, as follows. "A region is said to be **simply connected** if every closed curve in it forms by itself a complete boundary of a portion of the given region."

That's how it goes: Green's theorem, conjugate functions, point at infinity, linear fractional transformations, uniform convergence of series, singular points, the fundamental theorem of algebra, stereographic projection... a generous serving of the standard material of an introductory course, with many formulas and calculations, and a reasonable supply of pictures, but almost no clear explanations or conceptual clarifications. Not a dreadful book, but not one that could be used today.

22. E. T. Whittaker and G. N. Watson, *A course of modern analysis* (1902)

The first edition of this gigantic book (608 large pages) appeared in 1902, but the preface to my copy (which is the fourth edition) is dated 1927. To me the book looks like a quintessential representative of the British mathematics of a hundred years ago. It has two parts: "The processes of analysis" and "The transcendental functions." The second part is longer (345 pages) than the first, and I find it frightening. Its twelve chapters have titles such as "The zeta function of Riemann" (I am not yet trembling), "The confluent hypergeometric function" (now I am), and "Ellipsoidal harmonics and Lamé's equation" (I am ready to flee to cohomology and ask for asylum). In the complex variable courses at Illinois in the 1930s Whittaker and Watson was frequently used, but, to the best of my knowledge, the second part was never entered.

The first part, which is all I shall report on now, is an odd sort of book (of about 235 pages) on the theory of functions of a complex variable, and I think most students of today would be uncomfortable with its style and its approach. In the first paragraph of the first chapter the words Subtraction and Division and Arithmetic are spelled that way (with capital initials). There is nothing at all wrong with that, obviously, but it indicates the customs of another age (and another country?). It takes two pages to describe Dedekind cuts — no proofs, just definitions — and complex numbers appear immediately after that. One of the three problems ("examples") at the end of Chapter I is this: "Shew that a parabola can be drawn to pass through the representative points of the complex numbers

$$2+i, 4+4i, 6+9i, 8+16i, 10+25i."$$

Analysis begins with "the theory of convergence"; the first sentence defines limit, the next section explains the O notation, soon after that we are told about "the greatest of the limits" (which means lim sup, but it is not called that here), Cauchy's "principle of convergence" is shown to be necessary and sufficient for convergence, and then series can begin. As an illustration, worked out in a page full of O's, a condition for the absolute convergence of the hypergeometric series is derived. I went through that, fifty years ago, I studied it hard, carefully following each step and expanding the calculations when I found it necessary to do so. The chapter ends with a section on infinite determinants (with a credit line to "the researches of G. W. Hill in the Lunar Theory"), and at the end of the chapter there is a large batch of problems most of which are to me as frightful now as they were then. Here is one of them. "Shew that the series

$$\sum_{n=0}^{\infty} \frac{e^{2\pi i n x}}{(w+n)^s},$$

where w is real, and where $(w+n)^s$ is understood to mean $e^{s \log(w+n)}$, the logarithm being taken in its arithme.tic sense, is convergent for all values s, when $I(x)$ [= the imaginary part of x] is positive, and is convergent for values of s whose real part is positive, when x is real and not an integer."

Chapter III is about continuous functions and uniform convergence. A "two-dimensional continuum" is defined as "a set of points in the plane possessing the following two properties:

(i) If (x, y) be the Cartesian coordinates of any point of it, a *positive* number δ (depending on x and y) can be found such that every point whose distance from (x, y) is less than δ belongs to the set.

(ii) Any two points of the set can be joined by a simple curve consisting entirely of points of the set." [That's an open connected set, right?]

Chapter IV is about Riemann integration — first over intervals in the line, and then over paths in the complex plane. Problem: "Shew that

$$\int_a^{\infty} x^{-n} e^{\sin x} \sin 2x \, dx$$

converges if $a > 0$, $n > 0$."

Analytic functions and their properties (through Liouville's theorem) appear in Chapter V, residues and their applications in Chapter VI, and after that the material becomes more and more rapidly more and more British. One section in Chapter VII is about the expansion of functions in series of inverse factorials. Chapter VII is about asymptotic expansions and summability theory. ("Shew that the series

$$1 - 2! + 4! - \cdots$$

cannot be summed by Borel's method, but the series

$$1 + 0 - 2! + 0 + 4! + 0 - \cdots$$

can be so summed." The last two chapters of Part I are on differential and integral equations.

I have been trying to make a point by quotations and examples, and I'm not sure that I have succeeded. Many of the mathematicians I know have told me that they were surprised when they learned how different the mathematics of graduate school is from calculus. They remembered being good at formal integration and even enjoying it, but they found the concepts, the insights, the theorems, and the proofs of "real math" startlingly different from that kind of undergraduate sport. It seemed almost as if the courses they had been getting easy A's in belonged to a subject different from the one they now had to struggle to understand but were learning to consider beautiful. Although I am a total dilettante when it comes to complex function theory, I have long regarded it as one of the most beautiful parts of mathematics. The reason, however, is not the exposition of Whittaker and Watson. That book precipitates no surprise and shows no beauty; the impression it makes is that of calculus made messy. I didn't like it when I studied it, and have never learned to like it since. It isn't and it never was a good place to learn complex function theory from.

23. Konrad Knopp, *Funktionentheorie*, 2 volumes (1930)

I like everything about this book. I liked everything about it a little more than fifty years ago when I learned complex function theory from it, and I still think it is one of the best teaching books that I have ever seen. There is nothing fancy about it; the table of contents wouldn't cause anyone's heart to skip a single beat. What it contains is the standard material in every first course about complex variable theory, plus a few luxuries that not everybody must know, minus a few other luxuries that other books do put in.

Since the book appeared before Cantor replaced Euclid in the high school curriculum, Knopp proceeds cautiously about sets. In the first half dozen pages he gives several examples of subsets of the complex plane, the kind that are quite likely to arise later, and then, in the next dozen pages, he tells about Dedekind, Bolzano-Weierstrass, lim sups and lim infs, and Heine-Borel. After that, down to work: definition or differentiability, definition of line integrals, the Cauchy theorem and the Cauchy formula, series of functions, and, in particular, power series, analytic continuation, entire functions, and the classification of singularities — and that's the end of Volume 1. Volume 2 has meromorphic functions, periodic functions, including elliptic functions (a lovely luxury), roots and the logarithm, algebraic functions, and a cautious toe dipped into Riemann surfaces — and that's it.

There are a few exercises (is $|z|$ differentiable?) and many examples (the Laurent series of $1/(z-1)(z-2)$ in $1 < |z| < 2$ and in $2 < |z| < \infty$). The exposition is always simple, correct, clean (one of my terms of greatest praise), and clear. Learning the contents was a do-it-yourself project for me; I forced myself to read every word, by writing out an English translation of the book, both volumes, every word.

The book even looks good. It belongs to the once famous and widely sold Sammlung Göschen, consisting of tiny, green, pocket-sized volumes. The pages are somewhat smaller than those of the smallest paperback detective stories on drugstore racks (slightly over 4 inches by 6), and each volume is just 1/4 inch thick. They almost (but not quite) fit into a shirt pocket; the Knopp volumes fit easily, both of them together, into the side pocket of any jacket — they're ideal to read while hanging on to a strap on the subway, or squeezed in one of the middle seats of a crowded airplane. I so much liked the convenience of booklets of that size, that, years later, when I had an editorial connection with the Van Nostrand publishing company, I worked very hard to persuade them to continue the Sammlung Göschen tradition in this country — but my persuasion miscarried. (The compromise that did come out, the Van Nostrand Mathematical Studies, edited by Fred Gehring and myself, contained some books of high quality, but they were awkward, larger, uglier.)

Ahlfors's famous complex analysis book (I am looking at the 1966 edition) has more material in it than Knopp (the Riemann mapping theorem, the Dirichlet problem, and Picard's theorem) and some fancier topological language (chains and cycles), and the same is true of Conway's book (1978 edition), which discusses luxuries take extra space, of course; both those books are about 1.5 times as long as Knopp. They are good books, but my heart remains with Knopp.

Knopp is one of the few "dead" books that shouldn't be dead. Unlike Dickson's algebra book, it would be easy to modernize, and, by the addition of a third tiny volume, it could make available many of the same luxuries that Ahlfors and Conway offer. Would anybody care to try?

§6. TOPOLOGY

24. John W. Tukey, *Convergence and uniformity in topology* (1940)

This is a slim volume (90 pages), but it packed a punch in its day, and I am proud to own a copy inscribed by the author. (John chose to write my name in what he believed to be the Hungarian fashion, and he didn't get it quite right, but, as with a misprinted stamp, that probably makes my copy even more valuable.)

The work is Tukey's thesis ("drawn from" it, he says), and it is, in effect, a competitor to Bourbaki's approach to topology. It begins with set theory. Early on Tukey explains that the symbol $\varnothing$ for the empty set should be "pronounced as the phonetic symbol or the Scandinavian vowel" (most students think it's the Greek phi). He is also firm about the functional notation: "$f(x)$ is the value of f at x; we *never* use $f(x)$ to refer to a function." The climax of Chapter I is the discussion of Zorn's lemma. Tukey was one of the first (the first after Zorn?) to adopt it as an axiom and to insist that it is a much more convenient transfinite tool than the axiom of choice or, horrors, transfinite induction. He presents four different forms of it and gives hints about how to prove their equivalence.

Chapters II and III are about directed sets (directed systems for Tukey) and the convergence of functions on them. The favored concept is that of a phalanx, which is a function defined on a stack, which, in turn, is the set of all finite subsets of some set. The convergence of phalanxes plays the fundamental role, and the assertion is that topology can in fact be based on that concept of convergence (as it *cannot* be based on the convergence of sequences).

Chapter IV is about compactness, and here, it turns out that Tukey missed the boat — the definition he chose was not the one that the world has since adopted. For Tukey a space X is compact if every phalanx in X has a cluster point in X (or satisfies any one of nine other equivalent conditions). Of course he knows about and he refers to the Alexandroff-Urysohn notion of "bicompactness" (which is the modern version of compactness), but for him it is not the definition.

What I have mentioned so far is the first half of the book. The next four chapters are about normality, structs (a concept of uniformity), function spaces, and examples; one of the most fascinating chapters is Chapter IX, the final one, which is five pages of pontification. The titles of the sections is that chapter, and a quote from each section, will perhaps best convey some of its flavor.

What is topology? "Topology should be an analog of modern algebra. Modern algebra is concerned with suitably restricted finite operations and relations. Modern topology should be concerned with suitably restricted infinite operations and relations."

The role of denumerability. "The countable is important because it is so nearly finite."

Which separation axioms are important? "I do not think that normality is important."

No transfinite numbers wanted. "I believe that transfinite numbers, particularly ordinals, have a proper place only in descriptive theories, such as: the successive derivatives of a set, the Borel classes of sets and the Baire classes of functions, and some of the less pleasing parts of the theory of directed systems."

The subsequence and its generalizations. "We may look on the statement 'every sequence contains a convergent subsequence' as being in the form we wish to generalize. If we do, we find that it will not generalize satisfactorily."

Phalanxes vs. filters. "Phalanxes...form a part of a theory of convergence...which includes sequences. We obtain generality without discarding the intuitive treatment of special cases."

Tukey was 25 in 1940. He was then, and he has continued to be, a colorful character whose personality and whose work have had a lot of influence. While he was a graduate student in Princeton some of his friends even formed a society called the Gegen Tukey Sport und Turnverein. The US entered World War II in 1941, and everybody's life changed. Tukey left topology (but he continued to be based in Princeton), and became one of the leaders of the statistical world.

25. Solomon Lefschetz, *Algebraic topology* (1942)

It used to be called combinatorial topology, and Lefschetz claims credit for re-baptizing the subject. The new name caught on; by now only oldsters know that the old one ever existed. Lefschetz seems also to be claiming credit for re-baptizing bicompact to be compact, but I don't agree with that. As I remember Princeton in those days, in the late 1930s and early 1940s, the different notions of compactness, and their various names, all had their advocates, and while bicompact was the winning concept, it had the losing name.

The book had a strong influence on the subject; it was regarded as the gospel. It was, moreover, not only the new testament, but the old one at the same time. There weren't many sources where an outsider could learn the other kind of topology, the kind that used to be called point set topology, and then set-theoretic topology, and ultimately general topology, and as soon as it appeared Lefschetz's book started serving as the standard reference for both kinds. It taught us the facts, the words, and even the right attitudes that we should adopt.

Chapter I is a breathless, compressed, introduction to general topology. It tells us all about connectedness, compactness, and even the bare bones of dimension theory, and it does that in 18 pages (namely, pp. 6–23; the first five pages are about set theory, through Zorn's lemma). Up to that point the separation axioms (Hausdorff, normal, and all that) are not even mentioned; they, as well as metric spaces and a smidgeon of homotopy, are disposed of in pp. 24–40. The first forty pages of this book were, for a while, the only printed and easily availabale source of modern information about general topology in this country; they contain a lot more than many graduate courses have time to cover in the three or four months the subject is usually allotted.

Chapter II (pp. 41–87) is about the same length, and it does about the same thing for a part of algebra as Chapter I did for a part of geometry. The title of the chapter is "Additive groups." (The preface says that "Claude Chevalley practically acted as a collaborator" for it.) The concept of group is assumed known; the discussion begins with the definition of topological group. (The preface warns that all groups throughout the book are topological, but it allows their topology to be discrete.) The fundamental theorem of finitely generated abelian groups is proved (the representation as products of cyclic groups), inverse and direct limits are discussed, characters are defined and examined, the Pontrjagin-van Kampen duality theorem is stated and the compact-discrete case of it is proved. In other words, Chapter II is, like Chapter I, more like a textbook than a chapter in a textbook.

I don't propose to go into any detail about the remaining chapters (there are eight altogether), but I should at least say what they are. Chapters III–VI are about complexes (which used to be called "abstract complexes"), Chapter VII is about the homology theory of topological spaces, and Chapter VIII is about polyhedra (and includes what is known as the Lefschetz fixed point theorem). Just for good measure, the book ends with a couple of appendixes. One is by Eilenberg and MacLane about homology groups of infinite complexes and compacta; it contains what seems to be one of the first appearances of Ext. The other appendix, by Paul Smith, describes his important work on fixed points of periodic transformations.

There are not very many books like this, and, for sure, there are not very many authors like Lefschetz who can write them.

26. John L. Kelley, *General topology* (1955)

This, the last book of auld lang syne that I am reporting on, is the youngest of the lot, but more than thirty years of exerting a strong influence on the terminological and topological behavior of hordes of students make it auld enough. It's a good book, and it was a best seller and a trend setter in its day. Its contents are not surprising now, but by no means all of them were automatically included in all topology books before 1955. Connectedness, compactness, and metrization — yes — that's what general topology is all about. Nets ("generalized sequences"), however, were hard to find in the literature, and their appearance in Kelley's book helped make them an almost universally used tool. Function spaces had been studied by topologists because they were curious spaces that others could be embedded in — but Kelley discusses them from the point of view of analysis, a subject that is interested in individual functions as well as in the spaces in which they consort together. In the preface he confesses that he thought of the book as the material that "every young analyst should know."

The book contains other nonorthodox topics (besides nets and the analytical study of function spaces); among them are uniform spaces and, in an appendix of thirty pages, the A. P. Morse approach to set theory. Admirers of the book who are reluctant to admit that it contains anything less than perfect regard such things equally highly with the rest of the contents. That's one reason why the Morse set theory has managed to stay more or less alive, but by now its intrinsic merits are causing it to slip more and more into a deserved oblivion. As for uniform spaces — some of their special instances (such as metric spaces, topological groups, and topological vector spaces) are vital parts of modern mathematics, just as groups, rings, and fields are vital special instances of "universal algebra" — but the valuable concrete is not always a good reason for spending time and energy on the unnecessarily abstract. Only a few people nowadays are ready to defend the thesis that uniform spaces belong to the list of subjects that every mathematician (or even every young analyst) must study.

Students find the book difficult sometimes, but they almost always find it rewarding. There is an introductory Chapter 0 that lists the important prerequisites, defines some of their terms, and proves a few facts about them. The list contains cartesian products (large ones), group homomorphisms, cardinal and ordinal numbers, and Zorn's lemma.

One of the most striking features of the book (a novelty?) is its use of problems. There is a big batch of problems at the end of each chapter, and they are not just "finger exercises" (in the sense of pianists), but full-fledged recital pieces, integral parts of the discussion. Example: T_0 and T_1 spaces are defined in the problems following Chapter 1, and the Kuratowski closure and complementation problem (the one to which the answer is 14) is in the same place. The chapter on nets (titled "Moore-Smith convergence") is short, but it manages to present a careful (and rare) discussion of subnets (a naive imitation of subsequences is likely to lead to chaos). The biggest step in the proof of Stone's theorem on the representation of Boolean algebras is a problem (broken down into ten bite-sized subproblems). Semicontinuity is defined in the problems of Chapter 3, and so are topological groups, and w^* topologies for spaces of linear functionals.

Chapter 4 characterizes the subspaces of cubes (products of intervals), and pressents the Urysohn metrization theorem. In the problems we learn about the Hausdorff metric for subsets and about the Tychonoff plank (a normal space with a non-normal subspace). The biggest theorem in Chapter 5 is that a product of compact spaces is compact, but that's not all: the one-point compactification is here, and so are the Stone-Čech compactification, and a bit of the theory of paracompact spaces. Chapter 6 is on uniformity; the Banach-Steinhaus theorem is among the problems. Chapter 7 on function spaces discusses the point-open and the compact-open topologies (of course — that's what topologists have always done), but the Tietze extension theorem is among the problems, and so are the Stone-Weierstrass theorem and a bit of the theory of almost periodic functions.

It's a good book indeed — a useful book — a teaching book, a learning book, and a reference book — long may it wave.

§∞. Epilogue

Do the changes in the textbook literature indicate profound changes in the teaching of mathematics in the last fifty years? So far as I can see, the answer at the undergraduate and beginning graduate level is a firm no. You can probably think of a few subjects taught nowadays that were not taught in the 1930s. Computer science is one of them, and so, perhaps, is finite mathematics (which is nothing but a new mixture of some easy and presumably useful old subjects). Such things take some of our instruction time, yes, but have they really radically altered our curricula? I don't think so.

Algebra, analysis, and topology are pretty much the same now as they were then. We have replaced many old words by new ones and we have learned a few (very few) new facts, but a good graduate student at a respectable university would have very little trouble as a time traveller in either direction — fifty years forward or back, the questions (and even the answers) on the qualifying exams have stayed pretty much the same.

There were some bad books written in the old days and some good ones, and exactly the same is true today. *Plus ça change, plus c'est la même chose.*