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Chapter 4

Mathematics and Society in the 20th Century[1]

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Mathematics education and research are two separate crafts, but—for practical as well as intellectual reasons—it is best if they know each other. In particular, it is very important for mathematics educators to have a broad and balanced view of the way research mathematicians perceive their craft. They must realize that the perception has kept changing throughout history and never as sharply as in the 20th century. This chapter's goal is to recount a few highly significant features of the strife that came in the preceding hundred years. Mathematics ended that century in great spirit and in a state of great vigor, renewed collegiality and marvelous diversity.

But in the 1960s and 1970s, the representatives of the profession described the flow of 20th century mathematics as that of a single majestic river whose irresistible course was not touched by historical accident but had been preordained by inner logic. It necessarily proceeded inevitably and inexorably towards increasingly general, structural, or fundamental notions—which happened to be increasingly abstract. In the spirit of "the end of history," the descriptions never referred to the past or the messiness of Earth.

The majestic flow in question was unflinchingly understood to be leaving aside many people (including myself), and innumerable topics that concern either the foundations (logic) or the applications. We were told that much of what *looks* like mathematics is *not really* mathematics, even though the distinction may not be obvious to the outsider.

The position I am about to describe is starkly different. I believe and I hope to convince you that mathematics is *not* the conservatives' ivory tower. It is a very big house on a rolling terrain, with many doors, windows open to many horizons and bridges to many other houses.

It need not be the Queen of Court Etiquette in Science looking down on most of her subjects from an ivory tower up on a high hill. It deserves to be the beloved Queen of all the Scientists' Hearts, and of the Soul of Science, the only non-contrived link that could prevent various parts from scattering away from one another.

Compared to the conservative view of mathematics, mine is far broader and far more strongly linked to other human activities. It is also a more diverse and lively subject. In particular, it is attractive to persons who are not professional research mathematicians, a category that includes students and most teachers of mathematics. My strong opinions represent a minority view, but one that is increasingly widely shared and I have no doubt will prevail.

In any event, my interpretations and opinions are neither capricious nor based on idle rumor or anecdote, but on widely ranging reading, active and uninterrupted participation in events that occurred in the USA and France over fifty years, and reports by an uncle who was a prominent mathematician in Paris and Houston and participated in the immediately preceding thirty-five year period.

I see mathematical science as a very broad enterprise that shelters many diverse topics, ranging from the very concrete to the very abstract. This view is well represented by a simile I heard used by Hermann Weyl (1885–1955). He compared mathematics to the delta of a great river, one made of many streams: they may vary in their width and the speed of the flow through them; nevertheless, all are always a part of the system, and no individual stream is permanently the most important. This simile represented the mood of mathematics close to the year 1900—and also, for that matter, its mood near the year 1800. More importantly, mathematics has been changing so fast for a decade or so that I feel that Weyl's simile became applicable again in the year 2000.

But the resemblances between these snapshots taken centuries apart certainly do not imply that mathematics is unchanging, something outside ordinary history. In mathemat-

[1] Adapted from an invited address "What will remain of 20th century formal science" at the Europeäisches Forum 1992, held in Alpbach, Austria. This text remains self-contained and preserves some of its original flavor, in part by repeating some points that were already made elsewhere in this book but bear emphasis.

ics, as in every other aspect of human life, the 20th century gave us an example of something starkly different: a rocky history and continuing conflict. Mathematics was not ruled by its own determinism; it did not evolve separately from every other aspect of human knowing and feeling; it has on the contrary been profoundly affected by endless external vicissitudes.

The words *profoundly affected by* must not be misunderstood as meaning *enslaved by*. Of all the triumphs of humanity, the discovery and the development of mathematics is perhaps the greatest kind. A field's importance to the overall human experience is necessarily reflected by the role that internal logic has upon its development; nevertheless, strife has been present in mathematics since the Ancient Greeks. We shall see this when this story ends by mentioning the long-standing conflict between the traditions of Plato, the ideologue, and Archimedes, the experienced scientist. Like every individual human activity, mathematics very much participates in general history, politics, demography, and technology, and it is heavily influenced by the idiosyncrasies of a few key people. Let me give some examples from this century.

Around 1920, a group of Polish mathematicians collected around a very forceful man named Waclaw Sierpinski (1882–1969). They chose to concentrate on a field that was not practiced much in the reigning intellectual capitals, and founded a very abstract new branch often called *Polish mathematics*. They proudly proclaimed that their goal involved national politics: they did not want the newly reestablished Poland to become a mathematical satellite of Paris or Göttingen. I know that Providence is credited with working in mysterious ways. Yet, would anyone claim that Polish nationalism after more than a century of partition had anything to do with the historical determinism of mathematics? Polish mathematics became an important force pushing towards abstraction at all cost. Yet, by a bitter irony, some of the notions it originated failed to become important in mathematics, but eventually became important to physics—through fractal geometry.

My second example concerns Godfrey Harold Hardy (1877–1947), a strong person as well as a strong and highly inventive mind. The Poles had no strong native physics to contend with, but British mathematics of Hardy's youth was dominated by a form of mathematical physics that was extraordinarily effective (the Heaviside Calculus differentiated discontinuous functions!) but had little concern with continental rigor. During World War I, Hardy was an outspoken pacifist who recoiled from the practical uses of this old British mathematics. During another War, he wrote (Hardy (1940)), an impassioned account of his ideal of pure mathematics. For him, good mathematics could have no bad application—for the simple reason that it could have *no* application of any sort. By another bitter irony, his best example of total inapplicability turned out, in due time, to be essential to a problem he would have loathed: cryptography.

A three-page review of Hardy (1940) in the famous weekly *Nature* by the Nobel-winning chemist Frederick Soddy begins "This is a slight book. From such cloistral clowning the world sickens . . . 'Imaginary' universes are so much more beautiful than this stupidly constructed 'real' one, according to the author . . . Most scientists, however, still believe that . . . the real universe . . . is not stupidly constructed." But nothing can break the appeal of a tract that discriminates between the good and the bad without hesitation. Hardy's book remains in print and continues to this day to enchant some of the young. But would anyone claim that Hardy's militant anti-nationalism had anything to do with the historical determinism of mathematics?

From ideology, let us move on to demography. The 1910s were very cruel to French mathematics. First, Henri Poincaré (1854–1912) died prematurely on the operating table, then millions of young people died in trench warfare, and finally—perhaps worst of all—millions returned broken in health or spirit to a country that did not dare make heavy demands on them. As a result, the young postwar French mathematicians of the 1920s found that the only available teachers were men who had already been ill or old in 1914 and so did not go to war. Some have written movingly about the hardship of training without the usual parental supervision from slightly older advisors, and (as may have been expected) this hardship contributed to the emergence of several very strong personalities. In any event, the France of the late 1920s and the 1930s gave rise to an extremist movement calling itself Bourbaki. But would anyone claim that a demographic unbalance in a country with a long and glorious mathematical tradition has anything to do with the historical determinism of mathematics?

André Weil (1906–1994), now acknowledged as the mind behind Bourbaki, observed late in life that in his prime years, mathematics was little influenced by physics. Was that a natural feature of the preordained development of mathematics? Or could it be that Weil's views were set even before a visit to Göttingen in the 1920s? David Hilbert's dream Mathematics Institute there had three parts: a very pure one that Weil worshipped, one on numerical methods and one on mathematical physics. In the latter part, Max Born and Werner Heisenberg were in the process of creating quantum mechanics—but Weil apparently did not notice.

From demography, let us move to another form of ideology. Soviet anti-semitism treated Jewish mathematicians harshly; Jewish physicists, less so. Hence a number of very gifted mathematicians transferred to physics institutes, where they were welcome. Their move contributed greatly to the formation of the current very rigorous form of mathematical physics. Would anyone claim that Soviet ethnic politics have anything to do with the historical determinism of mathematics?

No one would claim that the specific historical determinism of mathematics only reflected the intellectual moods and fashions that rule society at large. But it happens that a very unusual mood prevailed early in this century, particularly in the 1920s. One especially visible and durable effect was the invention of the International Style in architecture, with its heavy emphasis on structure. In Finland, the very unusual small country where this style was born, modern architecture merged smoothly into what came before it, without discontinuity and without heavy dogmatism. But modern architecture became dogmatic in Germany with the Bauhaus and in France with Le Corbusier (1887–1965). The latter built few houses but made many sketches (for example, his proposed ideal improvement of Paris evokes the worst present suburbs of Moscow). When I was young, Le Corbusier was billed as a great intellect to whom modern architecture owed its intellectual legitimacy. Indeed, he wrote a great deal, but I find little in his writings beyond sophomoric trash. It may be that Bauhaus was useful, even commercially inevitable at a certain stage of the technology and economics of raising large buildings, but no one ever convinced me that they were an inevitable intellectual wave of the future.

Think also of physics. Having confirmed existence of the atom in the 1900s, it went on to focus increasingly on the search for the most fundamental structural components of matter, increasingly tiny ones. Biology took this path later.

How was mathematics affected by the above-mentioned politics, demographics, and general intellectual moods? I view them all as responsible for the fact that near the middle of our century mathematics behaved in ways totally at variance with its mood today and its mood in 1900 or 1800.

This atypical mathematics is conveniently denoted by the name it took in France, but the current that gave rise to Bourbaki also affected many countries other than Britain, France and Poland. It strongly affected the USA, with a little-known wrinkle. One might have expected a brash new industrial giant to favor applications, but in terms of mathematical research the precise contrary was true. In Europe, the 19th century had created wide-ranging establishments against which Bourbaki could revolt. In the USA, before the arrival of refugees from Stalin and Hitler, research mathematics was dominated by aristocrats and anarchists, hence was very pure (as well as outstanding on its terms). Bourbaki did not reach the outlying countries Sweden and Finland, and there were strong counteracting forces in Germany and Russia. In the 1960s, when Bourbaki was its strongest, it benefited from another extraneous event: Sputnik created a period of unprecedented economic growth in Academia, with minimal social pressure on the sciences, and greatly increased the number of math PhDs, including many Bourbaki products. The math departments' balance was overwhelmed by them.

To sum up, Bourbaki found roots by selecting one of the many components of the mathematics of 1875–1925, gathered strength during the second quarter of our century (the period to which the above examples refer), and took power around 1950. During the third quarter of the century it exerted an extraordinary degree of control. There was no disorder in mathematics, but the field was narrowed down to a truly extraordinary extent. At one time it seemed to reduce to little more than algebraic topology; at a later time, to number theory and algebraic geometry. These are extremely important fields, to be sure, but concentration on a single field was quite contrary to the historical tradition that I have already mentioned and that had led Hermann Weyl to the image of the delta of the Nile. Mathematics seemed to have reduced itself to basically a single stream at any given time. This happened to be the cliché description that Herman Weyl (in a contrasting image) applied to physics.

The Bourbaki, as has already been implied, never paid attention to the historical accidents that contributed to their birth; they felt themselves to be the necessary and inevitable response to the call of history. Today, however, this call seems forgotten, and there is wide consensus that, like new math, "Bourbaki is dead."

Who killed Bourbaki? Throughout its heyday, my friend Mark Kac (1914–84) and many other open-minded mathematicians argued, in vehement speeches and articles, that Bourbaki had misread mere accidents for the arrow of history. But such negative criticism invariably lacked bite, and it had no effect. My own partisan opinion is that Bourbaki's fate was typical of many ideologies outside science. The founders could only insure their immediate succession; gradually, the ideological fervor weakened and the movement continued largely by force of habit. The resulting weakening was gradual and not obvious. But everyone noticed when the movement was knocked down by yet another event that had nothing to do with the historical determinism in mathematics. This event was something I view as a return to sanity, namely the rebirth of experimental mathematics that followed (slowly, as we shall see) the advent of the modern computer.

From where did the computer come? From the mathematical sciences understood in a broad sense. What relation is there between the advent of the computer and mathematics as narrowly reinterpreted by Bourbaki? None whatsoever. The computer arose from the convergence of two fields that surely belong to mathematics but were spurned by Bourbaki, namely, logic and differential equations. We all know that one must never rewrite history as it might have proceeded if two crucial events had chanced to occur in the reverse order. But in this instance the temptation is strong to air the following conviction. Had an earlier arrival of the computer saved experimental mathematics from falling into a century of decline, Bourbaki might have never seemed to anyone to be an unavoidable development. Let me elaborate on the computer's roots.

Surprisingly, while **Foundations of Analysis** was (for a while) the overall title of their treatises, the Bourbaki had only contempt for the logical foundations of mathematics, as in the work of Kurt Gödel (1906–78) and Alan Turing (1912–54). In the 1930s, Turing had phrased his model of a logical system in terms of an idealized computer. His *Turing machine* had a very great influence on the thinking of those who developed the actual hardware.

However, the man who made the computer into a reality was John von Neumann (1903–57). He was not only a mathematician, but also a physicist and an economist, and his great breadth of interests came to include a passion to find ways to predict the weather.

Thus the computer was born in the 1940s from a strange combination of abstract logic and the desire to control Nature. Eventually, the computer changed mathematics in a very profound fashion. But for a very long time, core mathematicians felt totally unconcerned, and viewed it with revulsion. Because of his work on the computer, von Neumann ceased to be accepted as a mathematician and in 1955 he decided to resign from the Princeton Institute for Advanced Study. (He died before his planned move to California.)

The year 1955 was also the date of publication of Fermi, Pasta & Ulam (1955), a text that appeared only in a Los Alamos report but was widely read and viewed as an early masterpiece of experimental mathematics before it was actually printed in Fermi (1965) (pp 977–988) and then in Ulam (1974) (pp 490–501). A comment by Stanislaw Ulam (1909–1984) informs us that the initiative for using the computer to assist mathematical research had come from Enrico Fermi (1901–1954), who was of course a physicist, not a mathematician. And Ulam asserts that "Mathematics is not really an observational science and not even an experimental one. Nevertheless, computations were useful in establishing some rather curious facts about simple mathematical objects." Surprised, I reached for a more positive statement in the autobiography, Ulam (1976), but found nothing worth quoting.

How did experimental mathematics fare during the 25 years after 1955? That period happens to end in the year of publication of Mandelbrot (1980), and coincided with the heyday of Bourbaki. In a near-perfect first approximation, it saw *no experimental mathematics at all*. Not only was the lead of von Neumann and Fermi not followed by mathematicians, but their disinterest for the computer was carefully considered, not caused by ignorance. For example, when I was new at IBM, which I joined in 1958, opportunities to use computers were knowingly and systematically offered to every mathematician with a good name who could be coaxed into the building. Not one of them paid attention to the offer. Interest in experiment did not spread to at least some mathematicians until my work started attracting wide attention.

In understanding the process of discovery, the slowness of the acceptance of the computer brings up forcibly a very old issue: the respective contributions of the tool and of its user. Galileo wrote a whole book complaining bitterly about those who belittled him by claiming that his discovery of sunspots was only due to his having lived during the telescope revolution. In fact, telescopes were widespread but useless before one reached Galileo's steady hand and good eye. For contrast, consider the chapter of mathematics called the global theory of iteration of rational functions, to which the Mandelbrot set belongs. Pierre Fatou (1878–1929) and Gaston Julia (1893–1978) are—quite rightly—praised for developing this chapter, and no one would dream of belittling their contributions as being due to their having lived during the age of Paul Montel (1876–1975). Montel was the mathematician who, in 1912, discovered Fatou's and Julia's key tool, called the normal families of functions. Soon afterwards he was called into the Army, leaving behind Fatou (who was a cripple) and Julia (who had come back from the trenches as a wounded war hero). After World War I, Montel looked after the theory of iteration as *his* baby. Today, in the noise that accompanies changes in mathematics, those who use the computer are treated like a Galileo and not like a Montel. That is, critics are found to belittle their work as solely due to their living in the computer age. If it were so, experimental mathematics would have thrived after von Neumann and Fermi; the preceding remarks show that it did not.

Let me summarize, make a general comment, and conclude. One cannot disregard the lessons of history, contrary to the belief of those who argued that the pure mathematics of the mid-20th century was preordained by destiny. Its birth in the 1920s was influenced by Polish and English ideology, a demographic catastrophe in France, and the general intellectual mood of the day; its success was hastened by a long spurt of economic growth, and its demise was hastened by a mere technological development. None of these events was influenced by mathematics, none was preordained, and none acted immediately. In any event, von Neumann's and Fermi's lead was not followed by other mathematicians.

To conclude, "What will remain of 20th century mathematics?" There can be no short and truthful answer, because at this point of its history, mathematics is in healthy and constructive turmoil. Once again, the Bourbaki utopia flourished when every science was experiencing unprecedented growth and minimal social pressure. It seemed that any would-be peer group could organize itself and prosper with no hindrance from other, equally self-interested peer groups. But today the sciences face scarcity and strong pressure to justify both their size and their goals, and everyone bemoans the absence of generalists capable of representing more than a few groups. How the effects of the resulting intractabilities and pressures will combine with the internal logic of mathematics, of the computer and also of today's mathematical physics— that thriving no man's land between theorem proving and observation of nature—is simply beyond prediction. Fortunately for the teachers of mathematics, they are not asked to predict, but it is best for them to know the past, if only to avoid being drawn to repeating its deep errors.