MY LIFE WITH MATHEMATICS AND WITH BOURBAKI

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It has been suggested to me that this lecture should be of a personal, rather than a technical character, and should be easily comprehensible to a non-mathematical audience. Therefore, it may be appropriate if I begin with an anecdote pertaining to my childhood.

It is not exceptional, I believe, that a gift for mathematics should reveal itself quite early in life. This seems indeed to have been the case with me. According to Plato's reminiscence theory, a mathematician does not invent theorems: he remembers them form former lives. Whatever truth there may be in this, I have been told that when I was about six or seven years old, a teacher of mine told my mother (who had expressed doubts about my proficiency in mathematics):"Whatever I tell him, he seems to know it already": clearly she was thinking of Plato. Not long after that I managed to borrow a cousin's textbook on elementary algebra (perhaps he was only too glad to get rid of it) and made it my favorite reading for several months or even years.

I have no wish, however, to bore you with such stories. This was only to indicate that the choice of mathematics as the main subject for my endeavors was not accidental, or rather that there was no choice on my part. Nevertheless, this interest was not an exclusive one. It soon turned out that I had some facility for languages, and that poetry was a never-ending delight. I started learning Greek and read the first book of the Iliad. This, perhaps, led to the conclusion that cultures other than mine could well contain as much or more truth than the one I was being exposed to. I conceived the wish to learn Sanscrit and get acquainted with the great epics in that language. I did learn enough to read the Bhagavad-Gita and to attend the lectures of the famous Sylvain Lévi at the Collège de France on the Meghaduta.

I got my basic mathematical education at the École Normale Supérieure, where I was a student from 1922 to 1925. This institution, founded during the French Revolution, recruited itself, both in scientific and humanistic subject, through fairly stiff competitive examination. Traditionally, the best mathematical student in the French secondary schools (the so-called Lycées) go either to the École Normale or to the École Polytechnique, which has a more practical bend. Living together for three or four years, and enjoying the greatest possible freedom, the student at the École Normale become close friends. They tend to learn more from one another than from the courses they are supposed to attend even though some of the best teachers in French science have been professors there.

Luckily for me, I had already been introduced to Hadamard by my teachers at the Lycée, and was at once admitted to his seminar at the Collège de France, the only seminar there was for mathematicians at that time. Its official title, I believe, was merely "current literature." It consisted of reports by members of the seminar on those papers, mostly recent ones, which had attracted Hadamard's attention. Hadamard's purpose was to cover as much as possible of whatever was going on in mathematics, from number theory to probabilities. No better introduction was conceivable into the mathematics of the day.

But Hadamard's seminar made it clear that much in mathematics was going on in foreign countries. As soon as I could, i.e., as soon as I graduated from the École Normale, I started visiting those countries where mathematical activity seemed to be at its peak—first Italy, where I was attracted by the work of Volterra and his school, then Germany and Scandinavia. This was the time when I worked out the ideas for my doctorate thesis, which was largely inspired by Mordell's 1922 paper (then little known, now justly famous as having inaugurated the arithmetical theory of elliptic curves). Soon I accepted the offer of a professorial position in India and was submitted to other influences there, after which much of my life was spent in travels, some voluntary and some not so, as when I went from Finland to jail in France (more or less as a conscientious objector), which gave me the opportunity for one of the best pieces of work in my life (my Comptes Rendus note of 1940). From a brief period of military service in the French army at the time of the defeat of France in 1940, I left for the United States (having again no choice, since I had lost my University position in Strasbourg), then to Brazil, and from there to Chicago and eventually to the Institute for Advanced Study in Princeton, from which I retired in 1976.

Apart from the impressions left on me by my stays (voluntary or not) in many countries, the main events in my life were mathematical discoveries, which pleased me greatly but do not lend themselves to a description in front of the present audience,and the creation of Bourbaki, about which I shall now give some details.

Most of the French mathematicians in the years after the First World War had been students at the École Normale. One of them was Henri Cartan, the son of the great mathematician Elie Cartan, and equally distinguished himself. At the time when I came back from my professorship in India, he was a member of the Strasbourg faculty, and lost no time in getting me appointed there. He and I were jointly in charge of the main calculus course, then known as “calcul différentiel et intégral.” This was supposed be based on a textbook by Goursat, who had been a well-known mathematician of the previous generation, and one of our teachers when we were student at the École Normale. A number of our friends were then in charge of the same course in various French universities. All of us were more or less discontented with Goursat’s textbook, which we felt to be badly out-of-date.

Henri Cartan and I had frequent discussions about the best way of presenting this or that topic of the prescribed curriculum to our students. One topic which engaged us more particularly was the so-called Stokes formula, which had been the subject of important work by Henri Poincaré and Elie Cartan. One day, as I remember it, one of us had a bright idea: why not assemble five or six of our friends, concerned with the same problems, and, as we thought, “sett1e such matters once and for all?” Although we did not know it, it may be said that Bourbaki was born in that instant.

The idea seemed good. Soon we got together in a moderately priced restaurant in Paris and we agreed to renew those meetings at regular intervals. At first our purpose was merely to bring up to date our teaching of calculus. Then we thought that this could best be accomplished by writing a textbook to replace “the Goursat.” Eventually we realized that this would not be enough.

As a model before us, we had Euclid’s *Elements*: this, for more than twenty centuries, had made obsolete all earlier treatises and had served as the indispensable introduction to all mathematical teaching. Had Euclid’s *Elements*, too, been the product of a collective enterprise? This does not seem impossible; at any rate, Euclid, for us, is no more than a name, since Greek tradition has left us nothing about him. In contrast with other Greek mathematicians, we have no biography of Euclid, while we know that various part of his work are the product of a long tradition. We decided to adopt *Elements of Mathematics* as the title for our collective work, and that it should serve as the foundation for the whole of the mathematical science of the Twentieth century, just as Euclid had been for Greek geometry for many years.

As we planned that no part of work should be the product of an individual contribution, it became clear that our names should not be aligned on the title page, or rather title pages, since obviously many volumes would be needed. What name should we put there? For a long time hardly any mathematical work had appeared pseudonymously. The need for a mathematical career seemed to make it necessary that individual authorship should be acknowledged but little did we care for such considerations. while a tradition for what was known in the École Normale as "*canular*" (a certain style of practical jokes) was alive among us. One such "canular" had been a farcical lecture, read to first year students by a senior student under an assumed name, ending up with a nonsensical "Bourbaki's theorem." Bourbaki had been the name for a general in the 1870 war between France and Germany. Bourbaki was the name selected to appear as the fictitious author our work. Thus, for many years, anonymity was preserved. We were fortunate to find a publisher with a keen sense of humor who, in spite of dire warning by distinguished professors, agreed to handle our project. Indeed it turned out to be a financial bonanza for his publishing house, but this was still in a somewhat distant future.

Quite soon we discovered that our bimonthly meetings would not be adequate to our purposes. We decided that summer meetings, of one to two weeks, should be held in some pleasant place in the country. Our first such "congress," (as we used to call them), was held in a laboratory belonging to the University of Clermont (where one of us was a professor), and otherwise empty during the summer. We planned to hold the next one in Spain, in a place famous for its monastery (The Escorial). The Spanish Civil War interfered and our 1936 congress was held in a country house belonging to the mother of one of us; but in our archives it still bears the name of "the Escorial congress." There the main outlines of our method were adopted.

Topics were to be classified according to what we agreed to call the underlying "structures": general set-theory, to begin with, then general topology, algebra, etc. Each topic was to be reported on by one of us, then discussed by the full congress, then entrusted to another member of our group, and so on, until full agreement was reached. Nothing was to be given to print without the unanimous consent of the whole group; thus it was expected that each topic would go through the hands of at least three or four of us: the group would be free to reject *in toto* any manuscript that was submitted to it. This happened on at least one occasion with a proposed treatment of integration theory on whose principles no agreement could be reached. Such disputes were sometimes violent, or at least might have seemed so to an outsider. At one of meeting in Le Poët, in the French Alps, where we had reserved for our meeting a small hotel in an agreeable site, we learnt eventually that the hostess had spent the whole of our first evening in the corridor, wondering whether she should call in the police (the "gendarmes"). She had heard one of us threaten another one to throw him out of the window! She soon got used to such incidents. Would such a laborious process ever converge? Would ever anything get printed? It required a good deal of faith to believe it. But our faith in Bourbaki was boundless.

In fact, when the War broke out in 1939, the first fascic1e on general settheory was ready and the first one on general topology was nearly so. In 1940, Dieudonné circulated among us the first number of an internal periodical called "La Tribe," "The Tribe (This had been the technical name proposed for a concept in the abortive project on integration theory.). "La. Tribu," which I believe, continues to this day, purported to inform us of the progress of our collective work; it has remained as a feature of the activities of Bourbaki ever since. Even during the war, communication were never completely interrupted between members of Bourbaki. A particularly useful meeting was held in 1943 between only three participants in a hotel in Liffré near Rennes, where food was still abundant. The proceedings of that congress were sent out to the members then in the United States in two copies; one of them was lost, but the other one reached its destination through the c1andestine Gaullist mail.

The end of the war reestablished communications between members of Bourbaki on both sides of the Atlantic and work continued along established guidelines. More than 20 volumes were published, including such topics as Lie groups and topological vector spaces. Eventually, after some new members had been co-opted into the group, it was agreed that members should retire at the age of 50. Thus I have been out of touch with Bourbaki for quite a while now, even though as an honorary member, I still receive occasional reports on current work. It must be acknowledged that, while Bourbaki still meets regularly every year, it has not published anything for a long time, while perhaps, thanks to its method, it could still do some useful work. Nevertheless, there is no doubt that if it did not exist, no one would now think of creating it. lndividual work, or else collaboration between small groups of two or three, seems more conductive now to mathematical progress. On the other hand, few mathematicians would deny that Bourbaki's coming into existence was timely. Without Bourbaki, mathematics might not be now quite what it has become.

What philosophy, if any, was behind Bourbaki's enterprise? And what philosophy, if any, was behind my own relationship to mathematics? As to the first question, the answer is a simple one. At the time our group started work there was a growing conviction, on the part of more and more mathematicians, that all of the existing mathematics can be derived from some suitable set of axioms (for instance, the famous Zermelo-Fraenkel axioms) for set-theory, and that intuition, however indispensable for discovery, need not and should not play any part in the logical structure of our science. This, in essence, had been preached by Hilbert, but it remained to actually perform this task: after Bourbaki no one could dispute its feasibility. Little does it matter that it led no an exaggerated part given by some to axiomatics in elementary teaching. This was the so-called "new maths," against which, fortunately, reaction had set in. It was eventually realized that young minds should be introduced slowly and gradually to abstract thinking, but it had never been Bourbaki's purpose how mathematics should be taught to young students. It seems that by now general agreement has been reached on this subject.

As to my own personal views, (since the Inamori Foundation's letter requests me"to address my own philosophy and my own outlook on life and the world which will serve to edify and enlighten the audience"), it can best be described by recalling the well-known exchange of views between Kronecker and Eisenstein on the occasion of the former's "defense" of his doctoral dissertation. One of the "theses" which he had offered to "defend" was that "Mathesis et ars et scientia dicenda" (mathematics is both art and science), to which Eisenstein is said to have retorted that it is all art, not science. In the same sense, I once compared mathematics to sculpture in a very hard medium, since the answer to most questions in mathematics is what the mathematician cannot change, but he can and will change their arrangement according partly to logical but mostly to aesthetic reasons. This is where history comes in.

Like not a few of my contemporaries, I had been early intrigued by what has been (misleadingly) known as Fermat's last theorem (actually not his last, and not a theorem!), viz., that the equation $x^n = y^n + z^n$ has no solution in integers. I was still in the École Normale when this led me to reading Fermat himself. On the other hand, my growing interest in art (both pictorial and musical) had led me to the idea that it is the greatest art which requires our fullest attention: applied to mathematics, this led me to read Riemann, first his doctoral dissertation, then his great memoir on Abelian functions, after which I started reading other classics. I soon perceived that three is more beauty to be found in Archimedes than in some volumes of the Mathematische Annalen. From there the transition was easy to a deep interest in the history of mathematics, and I was greatly pleased when Bourbaki agreed to supplement most sections with a brief but thorough history of the subject. For some years I played no small part in the writing of such "historical notes."

Having thus become acquainted with beauty in mathematics, to what else could I have dedicated my life? Firstly, in my own work, as long as 1 found myself capable of adding to the existing corpus of mathematical knowledge; then, when it was too late for that, with historical work about the great mathematicians of the past. Eventually such men as Fermat and Euler when their writings, and more particularly, their correspondence is scrutinized, tend to become personal friends. This has made me happy. Will such a statement edify and enlighten the present audience? I am inclined to doubt it, but at my age, I fear it is the best I can do.