The Continuing Silence of Bourbaki—

An Interview with Pierre Cartier, June 18, 1997

Marjorie Senechal

This column is a forum for discussion of mathematical communities throughout the world, and through all time. Our definition of "mathematical community" is the broadest. We include "schools" of mathematics, circles of correspondence, mathematical societies, student organizations, and informal communities of cardinality greater than one. What we say about the communities is just as unrestricted. We welcome contributions from mathematicians of all kinds and in all places, and also from scientists, historians, anthropologists, and others.

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Nicolas Bourbaki, 1935–????

If you are a mathematician working today, you have almost certainly been influenced by Bourbaki, at least in style and spirit, and perhaps to a greater extent than you realize. But if you are a student, you may never have heard of it, him, them. What or who is, or was, Bourbaki?

Check as many as apply. Bourbaki is, or was, as the case may be:

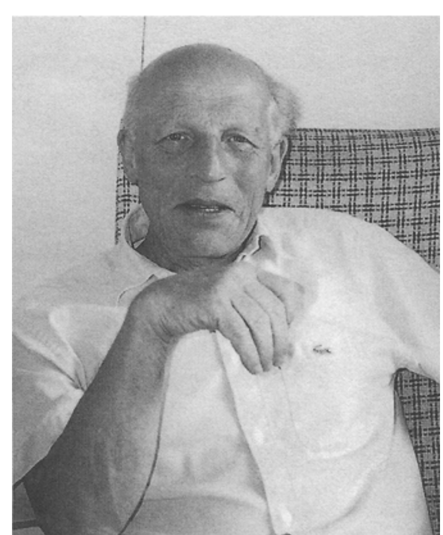
- the discoverer (or inventor, if you prefer) of the notion of a mathematical structure;
- one of the great abstractionist movements of the twentieth century;
- a small but enormously influential community of mathematicians;
- a collective that hasn't published for fifteen years.

The answer is: *all of the above*, and they are four closely woven strands of an important chapter in intellectual history. Is it time to write that chapter? Has the story of Bourbaki come to an end?

Bourbaki was born in Paris in 1935 when a small group of mathematicians at the École Normale Supérieure, dissatisfied with the courses they were teaching, decided to reformulate them. Most mathematicians have had that experience at one time or another, but the scope of Bourbaki's dissatisfaction grew quickly and without bound. By 1939, writing as an anonymous collective under the pseudonym Nicolas Bourbaki, it began to publish a series of books intended to transform the theory and practice of mathematics itself. From its beginning, Bourbaki was a fervent believer in the unity and universality of mathematics, and dedicated itself to demonstrating both by recasting all of mathematics into a unified whole. Its goals were total formalization and perfect rigor. In the post-war years, Bourbaki metamorphosed from rebel to establishment.

Bourbaki's own rules explicitly provided for self-renewal: from time to time, younger mathematicians were invited to join and older members resigned, in accordance with mandatory "retirement" at age fifty. Now Bourbaki itself is nearly twenty years older than any of its members. The long-running Bourbaki seminar is still alive and well and living in Paris, but the voice of Bourbaki itself—as expressed through its books—has been silent for fifteen years. Will it speak again? Can it speak again?

Pierre Cartier was a member of Bourbaki from 1955 to 1983. Born in Sedan, France in 1932, he graduated from the École Normale Supérieure in Paris, where he studied under Henri Cartan. His thesis, defended in 1958, was on algebraic geometry; since then he has contributed to many areas of mathematics, including number theory, group theory, probability, and mathematical physics. Professor Cartier taught at Strasbourg for a decade beginning in 1961, after which he joined CNRS, the Centre National de la Recherche Scientifique. Since 1971 he has been a professor at IHES (Institut des Hautes Études Scientifiques) at Bures-sur Yvette, and has taught at the



Pierre Cartier (photo by Marjorie Senechal).

École Polytechnique and at the Ecole Normale, where among other activities he runs a seminar on epistemology. In 1979 he was awarded the Ampère Prize of the French Academy of Sciences. Professor Cartier has been involved in various programs to help developing countries, including Chile, Vietnam, and India, build science at home; he is also an editor of a book about art and mathematics. Few people are better qualified to discuss the silence of Bourbaki. We are grateful to him for agreeing to do so with the readers of *The Mathematical Intelligencer.*

The Interview

Senechal: *Please tell us first about your own connection to Bourbaki.*

Cartier: As far as I remember, my first acquaintance with Bourbaki was in June 1951. I was a first-year student at the École Normale, Henri Cartan was my professor of mathematics there, and at his request Bourbaki invited me to join their meeting at Pelvoux, in the Alps. I remember that we discussed many things, especially a text written by Laurent Schwartz on the foundations of Lie groups; it was one of the first drafts in the well-known series of Bourbaki on Lie groups. It was not many years after Schwartz's invention of distributions, which made him famous. You have to understand that the mathematics students at École Normale were all students of both Henri Cartan and Laurent Schwartz (who left Nancy for Paris in 1952). We attended their seminars and courses and tried to use their new tools in all directions. François Bruhat and I were among the first to understand the importance of distributions in the theory of Lie groups and their representations. Bruhat devoted his thesis to these topics and I published my own contributions only much later.

For me, it was very important to be exposed from the inside. I was surprised to see all these great people I had known from a distance. I was accepted very freely. It took three or four more years before I was formally accepted as a member. In the fifties and sixties, there was a continuous spectrum from the inside core Bourbaki to the outside. The work that was printed in the books, what was reported in the seminar, and the work of the students were closely linked, and I think that is one of the reasons for the great success of French mathematics at that time. Of course, those times were very different. The scale was much smaller. Then there were about ten doctorates a year in mathematics in France (compared to three hundred today).

At that first meeting I was what they call a *cobaye*, a guinea pig. I was very enthusiastic about it. First of all, it was the first thing in modern mathematics that I saw. I came from a small city, from a difficult situation because of the war. I had been a student in a very provincial, very outdated high school. Some of my teachers were very good but of course they were very far away from modern science. The mathematics I was taught was classical geometry, in the uncultivated, synthetic way. I did have the luck to have an imaginative teacher in physics, and so at first I wanted to by a physicist. Then I was a student at the Lycée Saint-Louis in Paris before being accepted at the École Normale, and I took private lessons in physics from a very peculiar teacher, Pierre Aigrain. (A graduate of the Naval Academy, he was in 1950 an assistant professor of physics; eventually he became Secretary of State for science under President Giscard.) Usually a bright student completes the program in two years, but I managed to get through it in one. But both the mathematics and the physics I was taught were totally outmoded at that time, totally. I remember that, in a course called General Physics at the Sorbonne, the professor made a solemn declaration: "Gentlemen"—he did not mention ladies but there were very few girl students—"in my class what some people call the 'atomic hypothesis' has no place." That was 1950, five years after Hiroshima! So I went to Aigrain and said, "What do I do?" and he said, "Well, of course, you have to get your degree, but I will teach you physics properly." This shows what the French university was at the time. In order to understand the influence of Bourbaki, you have to understand that. Bourbaki came into a vacuum. Many people have discussed the reasons why this was so; I don't think this is the place to discuss it again. But obviously in the fifties, the early fifties, the teaching of science was very poor. It took Bourbaki about five or six years to subvert the whole system. By 1957 or '58 the subversion had been almost complete, in Paris.

Senechal: *But Bourbaki began in the thirties . . .*

Cartier: The first book was published in 1939, but there was the war, which delayed things, and also André Weil was in the States, Claude Chevalley was in the States, and Laurent Schwartz had to hide during the war because he is a Jew. Bourbaki survived during the war with only Henri Cartan and Jean Dieudonné. But all the work that had been done in the thirties blossomed in the fifties. I remember how we—the young mathematicians—were really eager to go to the bookstore to buy the new books. And at that time Bourbaki published at least one or two volumes every year.

When I formally became a member of Bourbaki in 1955, I had to abide by the rule that everyone should leave at 50, and so I left in 1983, when I was almost 51. I devoted almost 30 years of my life, and at least one third of my work, to Bourbaki. The working habits of Bourbaki involved very many preliminary drafts of a book before it was published. At the time, we had three meetings a year, one week in the fall, one week in the spring, and two weeks in the summer, which is already one month of hard work, ten or twelve hours a day. The published books comprised about 10,000 pages, which means approximately 1000 to 2000 pages of preliminary reports and drafts written every year. I estimate that I contributed about 200 pages a year during all this time with Bourbaki.

Senechal: *How many people belonged, at that time?*

Cartier: About 12. It was always a small, well-delimited group. The seminar was different, much more open. But still, in the 1950s, if you look at the table of contents of the seminar volumes, about half the papers were written by members of Bourbaki; in those days the interaction between the seminar and the group was very strong.

Now that's no longer true: it's still a distinguished series but it's usually written by people who have no direct connection with the institution Bourbaki. But at that time people published in the seminar series part of their discoveries, or preliminary accounts of Bourbaki's ideas that later appeared in the books.

I was typically a member of the third generation. You can say that there have been four. The first generation were the fathers: André Weil, Henri Cartan, Claude Chevalley, Jean Delsarte, and Jean Dieudonné, people who founded the group in the thirties. (Others joined in the beginning, but left soon.) Then there was a second generation, people invited to join during or just after the war: Laurent Schwartz, Jean-Pierre Serre, Pierre Samuel, Jean-Louis Koszul, Jacques Dixmier, Roger Godement, and Sammy Eilenberg. The third generation was Armand Borel, Alexandre Grothendieck, François Bruhat, myself, Serge Lang, and John Tate.

Senechal: *Did these generations differ in their attitudes or outlook?*

Cartier: They were very different. I think they became more and more pragmatic, and less and less dogmatic.

Senechal: *And how did that show up in Bourbaki's work?*

Cartier: From the beginning, the Bourbaki treatise was conceived as comprising two parts. The first part is on foundations and consists of six books, on set theory, algebra, general topology, elementary calculus, topological vector spaces, and (Lebesgue's) integration theory. The last four of these books give the foundations of analysis, as perceived by Bourbaki, with a strong bias toward functional analysis. The second part, falling short of more ambitious projects, consists of two very successful series, on Lie groups and on commutative algebra. Looking back at the list of the Bourbaki members of the second and third generations, you realize that some of the world's leading experts of the time were there, and that accounts for the breadth and depth of the second part of Bourbaki's work.

The older generation had learned mathematics in the old-fashioned way. They were the ones to reshuffle mathematics. The second generation had already been exposed to the new teaching. My generation, the third generation, did not have to prove that the new method was better than the old one because we were taught with the new method basically. I think I was just on the borderline, because in high school I was still taught in the old method, but when I went to Paris I was exposed to the new thinking. And so we were less and less dogmatic, because we didn't have to prove anything. The core of French mathematics had surrendered to Bourbaki. Bourbaki had already seized power, not only in intellectual terms but also in academic terms. It was clear that from an institutional point of view, Bourbaki had won.

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If you look at the volumes on Lie groups, you will see that the later ones have chapters that you don't expect in Bourbaki. It became more and more explicit; there are tables and drawings. I think this was basically the influence of one person, Armand Borel. He was fond of quoting Shaw, "It's the Swiss national character, my dear lady," and very often during a discussion he would say, "I'm the Swiss peasant."

Of course at that time differential geometry was blossoming, and it had always been a great challenge to Bourbaki. You have to remember that the father of Henri Cartan was Élie Cartan, the geometer, and the Bourbaki recognized only one godfather, Élie Cartan, and had much dislike for all the other French mathematicians of the thirties. Bourbaki came to terms with Poincaré only after a long struggle. When I joined the group in the fifties it was not the fashion to value Poincaré at all. He was old-fashioned. Of course, the opinion about Poincaré has completely changed. But it's clear that his style and Bourbaki's style were totally different.

The fourth generation was more or less a group of students of Grothendieck. But at that time Grothendieck had already left Bourbaki. He belonged to Bourbaki for about ten years but he left in anger. The personalities were very strong at the time. I remember there were clashes very often. There was also, as usual, a fight of generations, like in any family. I think a small group like that repeated more or less the psychological features of a family. So we had clashes between generations, clashes between brothers, and so on. But they did not distract Bourbaki from his main goal, even though they were quite brutal occasionally. At least the goal was clear. There were a few people who could not take the burden of this psychological style, for instance Grothendieck left and also Lang dropped out.

Senechal: *Did the goals stay clear in people's minds all the time, or were they changing?*

Cartier: They changed. The first generation had first to create a project from nothing. They had to invent a method. Then in the forties you can say that the method had emerged and Bourbaki knew where to go: his goal was to provide the foundation for mathematics. They had to submit all mathematics to the scheme of Hilbert; what van der Waerden had done for algebra would have to be done for the rest of mathematics. What should be included was more or less clear. The first six books of Bourbaki comprise the basic background knowledge of a modern graduate student.

The misunderstanding was that many people thought that it should be *taught* the way it was written in the books. You can think of the first books of Bourbaki as an encyclopedia of mathematics, containing all the necessary information. That is a good description. If you consider it as a textbook, it's a disaster.

Senechal: *Were you aware of that when you were a member of Bourbaki? Did people in Bourbaki realize that this was not a textbook?*

Cartier: More or less, but not so clearly as now. There was some misunderstanding about that, I suppose because we didn't have textbooks. I remember very well how I learned algebra and topology. When I was a student, every time that Bourbaki published a new book, I would just buy it or borrow it from the library, and learn it. For me, for people in my generation, it was a textbook. But the misunderstanding was that it should be a textbook for everybody. That was the big disaster.

Anyway, by then the scope of the project was more or less clear. But what should Bourbaki do after that? The second generation had an existing method, and had just to develop a project with clearly delineated boundaries. The third generation had to go beyond that, to go into the open world, which meant, at that time, geometry in a general way: algebraic geometry, differential geometry, several complex variables, Lie groups, moduli spaces, and so on.

I think I'm responsible for the idea that Bourbaki should devote a special chapter to the geometry of crystallographic groups. The reasons for that are clearly stated in the introduction to the series on Lie groups. Coxeter was the first to understand the relation of Lie groups to the crystallographic groups and their classification. Certainly the people who were working on Lie groups were, by spirit, more geometrical and more pragmatic than the others. But I remember that I had to fight quite hard to convince my colleagues within Bourbaki that crystallographic groups should be given preeminence.

Senechal: *What was Bourbaki's opinion of Coxeter?*

Cartier: I think that by the sixties people realized the importance of his work. Borel had many of the same ideas and Jacques Tits also played a role. Tits was much closer in spirit, in his way of doing mathematics, to Coxeter than to Bourbaki. He wasn't formally a member of Bourbaki but he had a long collaboration with us. So we could thank him, in the books, for his collaboration without breaking the rule of anonymity. Tits was very generous: he supplied us with many of the exercises, and many of his results were published for the first time in Bourbaki volumes. But of course he had a very different way of thinking about mathematics.

In the second generation and third generation, the two main series were commutative algebra (with algebraic geometry in the background) on the one hand, and Lie groups on the other hand. And there is an obvious difference of style and of emphasis, despite the fact that at that time Bourbaki was really a collective and everyone contributed to every book, more or less. Serre was a master of both sides; he was not an expert in Lie groups at first but he became one. Serre was the natural leader in the second generation because, like Weil in the first generation, he was the only one with a really universal approach to mathematics. But neither of them was an analyst. Certainly the contents of Bourbaki were much more about algebra, algebraic geometry, than about analysis.

By the fourth generation the goal was less visible. Grothendieck had developed his own program, outside of Bourbaki, so the need for a Bourbaki was less obvious. And there was also some lack of a global understanding of mathematics. The members had become more specialized in their interests.

There were various attempts within the group to focus on new projects. For instance, for awhile the idea was that you should develop the theory of several complex variables, and many drafts were written. But it never matured, I think partly because it was too late. There were already many good textbooks on several complex variables in the seventies, by Grauert and other people. By the end of the seventies, the method of Bourbaki had been so well understood that everyone knew how to write in this spirit. There was a whole generation of textbooks, and books, which were under his influence. Bourbaki was left without a task, and so he decided to devote part of his energy to revising his own books, the so-called "New Edition." The revision was mostly completed; these were really thorough revisions.

Senechal: *Do the revisions include a change of style?*

Cartier: No, no. But for instance, the section on the topology of metric spaces was much more developed and deepened, the proofs were improved, and there is a small volume that tried to bridge the gap between probability theory and the way that Bourbaki presented Lebesgue integration theory. That was an attempt to correct one obviously mistaken point of view of Bourbaki.

Senechal: *What other areas of mathematics do you see now as having been left outside?*

Cartier: First of all analysis, although there is an elementary calculus text, a very good book, that was the influence of Jean Delsarte. There is essentially no analysis beyond the foundations: nothing about partial differential equations, nothing about probability. There is also nothing about combinatorics, nothing about algebraic topology, nothing about concrete geometry. And Bourbaki never seriously considered logic. Dieudonné himself was very vocal against logic.

Anything connected with mathematical physics is totally absent from Bourbaki's text. In the Bourbaki seminar, I contributed a long series of papers with emphasis on questions of mathematical physics. But I was the only one, and my contributions were not always accepted without a fight.

But even in the areas of mathematics that were not considered by Bourbaki, looking backwards over the last thirty years, it is obvious that their development has been very much influenced by the Bourbaki spirit.

Senechal: *Was there a bias against physics, or did Bourbaki just not think about it?*

Cartier: Well, of course there was a strong bias against it, for most people. At the beginning I suppose I was slightly heterodox within the Bourbaki group. I had a longstanding interest in mathematical physics. A few years ago, in a discussion with André Weil, just after he published his own memoirs, I said, "You mentioned that in 1926 you were at Göttingen . . . in 1926 something happened in Göttingen." And Weil asked, "What did happen in Göttingen?" and I said "Oh! Quantum mechanics!" And Weil said, "I don't know what that is." He was a student of Hilbert in 1926 and Hilbert himself was interested in quantum mechanics, Max Born was there, Heisenberg was there, and others, but apparently

For him it was important to see questions as a whole, to see the necessity of a proof, its global implications. As to rigor, all the members of Bourbaki cared about it: the Bourbaki movement was started essentially because rigor was lacking among French mathematicians, by comparison with the Germans, that is the Hilbertians. Rigor consisted in getting rid of an accretion of superfluous details. Conversely, lack of rigor gave my father an impression of a proof where one was walking in mud, where one had to pick up some sort of filth in order to get ahead. Once that filth was taken away, one could get at the mathematical object, a sort of crystallized body whose essence is its structure. When that structure had been constructed, he would say it was an object which interested him, something to look at, to admire, perhaps to turn around, but certainly not to transform. For him, rigor in mathematics consisted in making a new object which could thereafter remain unchanged.

The way my father worked, it seems that this was what counted most, this production of an object which then became inert—dead, really. It was no longer to be altered or transformed. Not that there was any negative connotation to this. But I must add that my father was probably the only member of Bourbaki who thought of mathematics as a way to put objects to death for esthetic reasons.

From "Claude Chevalley described by his daughter" (1988)
In *Nicolas Bourbaki: Faits et légendes*

André Weil didn't pay any attention to it. I recently had an occasion to give a public lecture about the philosophy of space of Hermann Weyl, so I read the literature about him carefully. There is an obituary of Hermann Weyl written by Chevalley and Weil. They praise him, for good reasons, but there is no mention of his work in physics, not even his work in general relativity. By all accounts, the two best books of Weyl are his book on general relativity and his book on quantum mechanics!

Senechal: *Bourbaki's last publication was in 1983. Why doesn't it publish anything now?*

Cartier: There are several reasons for that. First, there was a clash between Bourbaki and his publisher, about royalties and translation rights, ending in a long and unpleasant legal process. When the matter was settled in 1980, Bourbaki was allowed to make a deal with a new publisher. Using the extensive work done in the seventies towards the revision of the old books, we were able to republish them in a new edition. We completed the existing series by two or three more volumes, but then . . . silence.

Beyond the easily stated goal of a "final edition," Bourbaki struggled in the seventies and the eighties to formulate new directions. I mentioned already a failed project about several complex variables. There were attempts at homotopy theory, at spectral theory of operators, at the index theorem, at symplectic geometry. But none of these projects went beyond a preliminary stage.

Bourbaki could not find a new outlet, because they had a dogmatic view of mathematics: everything should be set inside a secure framework. That was quite reasonable for general topology and general algebra, which were already solidified around 1950. Most people agree now that you do need general foundations for mathematics, at least if you believe in the unity of mathematics. But I believe now that this unity should be organic, while Bourbaki advocated a structural point of view.

In accordance with Hilbert's views, set theory was thought by Bourbaki to provide that badly needed general framework. If you need some logical foundations, categories are a more flexible tool than set theory. The point is that categories offer both a general philosophical foundation—that is the encyclopedic, or taxonomic part—and a very efficient *mathematical tool,* to be used in *mathematical situations.* That set theory and structures are, by contrast, more rigid can be seen by reading the final chapter in Bourbaki set theory, with a monstrous endeavor to formulate categories without categories.

It is amazing that category theory was more or less the brainchild of Bourbaki. The two founders were Eilenberg and MacLane. MacLane was never a member of Bourbaki, but Eilenberg was, and MacLane was close in spirit. The first textbook on homological algebra was Cartan-Eilenberg, which was published when both were very active in Bourbaki. Let us also mention Grothendieck, who developed categories to a very large extent. I have been using categories in a conscious or unconscious way in much of my work, and so had most of the Bourbaki members. But because the way of thinking was too dogmatic, or at least the presentation in the books was too dogmatic, Bourbaki could not accommodate a change of emphasis, once the publication process was started.

I think the eighties were a natural limit. Under the pressure of André Weil, Bourbaki insisted that every member should retire at fifty, and I remember that, in my eighties, I said, as a joke, that Bourbaki should retire when he reaches fifty.

Senechal: *It seems that this more or less happened.*

Cartier: Yes, I think one of the main reasons is that its stated goal, to provide foundations for all existing mathematics, was achieved. But also, if you have such a rigid format it is very difficult to incorporate new developments. If the emphasis doesn't change, it's still possible. But of course, after fifty years, the emphasis had changed.

Senechal: *Would you say a little more about that?*

Cartier: André Weil was fond of speaking of the *Zeitgeist*, the spirit of the times. It is no accident that Bourbaki lasted from the beginning of the thirties to the eighties, while the Soviet system lasted from 1917 to 1989. André Weil does not like this comparison. He says repeatedly, "I've never been a communist!" There is a joke that the 20th century lasted from Sarajevo 1914 to Sarajevo 1989. The 20th century, from 1917 to 1989, has been a century of ideology, the ideological age.

Senechal: *By ideology, do you mean the idea of a blueprint that can serve for all purposes and for all time?*

Cartier: *A final solution.* There are good reasons to hate that expression, but it was in the people's minds that we could reach a final solution. There is a book by H.G. Wells called *A Modern Utopia,* which ought to be reprinted. By chance I was reading it just at the time of the collapse of the Soviet system. As you know, H.G. Wells was certainly very friendly towards the October 1917 revolution, he was a friend of the Soviets, admittedly. But he had a very sharp mind and he had such a sharp historical view that he could envision developments. Even though he was excited by this new era, he understood that the final solution doesn't exist and that it was a mistake to consider that you can reach such a state of social historical equilibrium that from then on society will stay as it is forever. Wells argued very well against this idea. If you read his books, you will see that as one of his obsessions.

Hilbert, I think, reflected this *Zeitgeist.* There is one recording of his voice; in Constance Reid's book about Hilbert there is a floppy disk of it, a record of some speech that Hilbert gave in Germany in the thirties. It's very ideological. At the time Hilbert was aging and so his views were solidifying.

If you put the manifesto of the surrealists and the introduction of Bourbaki side by side, as well as other manifestos of the time, they look very similar. My daughter is currently translating a book about the birth of cinematography, and in a chapter about the Italian futurists there is a very similar statement. In science, in art, in literature, in politics, economics, social affairs, there was the same spirit. The stated goal of Bourbaki was to create a *new mathematics.* He didn't cite any other mathematical texts. Bourbaki is self-sufficient. Of course at the time the communists in the Soviet Union were claiming the same. We know now it was a lie, and that the leaders knew at the time they were lying. Certainly Bourbaki was not lying, but still, the spirit was the same. It was the time of ideology: Bourbaki was to be the New Euclid, he would write a textbook for the next 2000 years.

Senechal: *Why is there a lack of any kind of visual illustration in most of Bourbaki?*

Cartier: I think the best answer would be the description of Chevalley given by his daughter [see insert]. The Bourbaki were Puritans, and Puritans are strongly opposed to pictorial representations of truths of their faith. The number of Protestants and Jews in the Bourbaki group was overwhelming. And you know that the French Protestants especially are very close to Jews in spirit. I have some Jewish background and I was raised as a Huguenot. We are people of the Bible, of the Old Testament, and many Huguenots in France are more enamored of the Old Testament than of the New Testament. We worship Jaweh more than Jesus sometimes.

So, what were the reasons? The general philosophy as developed by Kant, certainly. Bourbaki is the brainchild of German philosophy. Bourbaki was founded to develop and propagate German philosophical views in science. André Weil has always been fond of German science and he was always quoting Gauss. All these people, with their own tastes and their own personal views, were proponents of German philosophy.

And then there was the idea that there is an opposition between art and science. Art is fragile and mortal, because it appeals to emotions, to visual meaning, and to unstated analogies.

But I think it's also part of the Euclidean tradition. In Euclid, you find some drawings but it is known that most of them were added after Euclid, in later editions. Most of the drawings in the original are abstract drawings. You make some reasoning about some proportions and you draw some segments, but they are not intended to be geometrical segments, just representations of some abstract notions. Also Lagrange proudly stated, in his textbook on mechanics, "You will not find any drawing in my book!" The analytical spirit was part of the French tradition and part of the German tradition. And I suppose it was also due to the influence of people like Russell, who claimed that they could prove everything formally—that so-called geometrical intuition was not reliable in proof.

Again Bourbaki's abstractions and disdain for visualization were part of a global fashion, as illustrated by the abstract tendencies in the music and the paintings of that period.

Senechal: *Did the members of Bourbaki appreciate abstract music and abstract art?*

Cartier: I don't think there was much taste for abstract music or art. You could say that on the whole they had standard bourgeois tastes. Educated bourgeois—not philistine. For instance, both Cartan and Dieudonné were lovers and practitioners of music, but they were very classical. Cartan certainly, in his Protestant education, was very fond of Bach, and Dieudonné was quite a good piano player, at an amateur level, but quite good, and he had a fantastic memory. He knew hundreds and hundreds of pages of score by heart and could follow every single note. I remember I had a few occasions to go to the concert hall with him. It was fascinating, he would look at the score in his hand and exclaim "*OH!*" if a note was missing from the orchestra! He devoted the last six months of his life—when he decided that his mathematical life was finished, he had written his last book, and he retreated to his home—to listening to recordings and following the scores and the notes.

It's interesting to know that revolutionaries in mathematics were not revolutionaries in other things. I suppose that the only person in the Bourbaki group who was really aware of the connections of the Bourbaki ideology with other ideologies was Chevalley. He was a member of various avant-garde groups, both in politics and in the arts. As the editor of Chevalley's work, I have decided, at the urging of his daughter, to include a special volume about his work outside mathematics. He had written various pamphlets, and various notes; Catherine Chevalley will have to work hard to collect these things and we will publish them as part of his collected works.

Chevalley was the only one who perceived the connection between Bourbaki and the rest, and that may be why, in the seventies, he was more critical than other people. In the seventies a sensible person could already see the end of a long historical trend, and I think he was very sensitive to this. Mathematics was the most important part of his life, but he did not draw any boundary between his mathematics and the rest of his life. Maybe this was because his father was an ambassador, so he had more contact with various people.

Senechal: *Could you state the main reasons for the decline of Bourbaki?*

Cartier: As I said, in the eighties there was no longer a stated goal, except for the long legal battle. I think it was one of the cases of the century! We hired a famous lawyer who had fought for the heirs of Picasso and Fujita. We survived artificially: we *had* to win this battle. But it was a pyrrhic victory. As usual in legal battles, both parties lost and the lawyer got rich. In fame and in pocket.

In a sense Bourbaki is like a dinosaur, the head too far away from the tail. When Dieudonné was the scribe of Bourbaki, for many many years, every printed word came from his pen. Of course there had been many drafts and preliminary versions, but the printed version was always from the pen of Dieudonné. And with his fantastic memory, he knew every single word. I remember, it was a joke, you could say, "Dieudonné, what is this result about so and so?" and he would go to the shelf and take down the book and open it to the right page. After Dieudonné (and an interlude by Samuel and Dixmier) I was the secretary of Bourbaki, and it was my duty to do most of the proofreading, I think I proofread five to ten thousand pages. I have a good visual memory. I wouldn't compare myself with Dieudonné, but there was a time when I knew most of the printed material in Bourbaki. But no one after me was able to do this. So Bourbaki lost the awareness of his own body, the 40 published volumes.

And as I said before, Bourbaki was more or less like a family. The second or third or fourth generation in any family or any social group follows definite sociological patterns. My own family was typical. My grandfather was a self-made man, a very successful businessman. My father and my uncle went into the business, but they were not so devoted to the fight. And people in my generation—well, I suppose I made the right decision not to engage in it. Indeed, people in my generation who did go into our family business did not do so well, because they didn't have anything to fight for.

But these are the inner workings. Of course the outside world also has an influence. That the outside mathematical world has changed is obvious. We all know that what Stalin could never achieve with his army, to conquer the world, the collapse of the Soviet Union has achieved for mathematics. The Russian mathematicians have brought a different style to the West, a different way of looking at the problems, a new blood.

It's a different time, with different values. I have no regrets: I think it was worthwhile to live in the twentieth century. But now it's finished.

Senechal: *How would you describe your journey with Bourbaki?*

Cartier: I have been personally very happy, because when I reached the time of normal retirement from Bourbaki, I had the very fortunate opportunity to be asked to deliver the lecture on behalf of Vladimir Drinfel'd at the International Congress of Mathematicians at Berkeley in 1986 (Drinfel'd was prevented from coming for political reasons). It was a great challenge and a great honor for me; his paper is one of the most important papers in the proceedings. Overnight that changed my mathematical life. I said, "This is what I have to do now." Of course I knew the basic material but the perspective was new. I cannot claim that within the few hours I had to prepare the lecture I could really master it, but I understood enough to explain to the people, "This is new, it is important."

When I began in mathematics the main task of a mathematician was to bring order and make a synthesis of existing material, to create what Thomas Kuhn called *normal science.* Mathematics, in the forties and fifties, was undergoing what Kuhn calls a solidification period. In a given science there are times when you have to take all the existing material and create a unified terminology, unified standards, and train people in a unified style. The purpose of mathematics, in the fifties and sixties, was that, to create a new era of normal science. Now we are again at the beginning of a new revolution. Mathematics is undergoing major changes. We don't know exactly where it will go. It is not yet time to make a synthesis of all these things—maybe in twenty or thirty years it will be time for a new Bourbaki. I consider myself very fortunate to have had two lives, a life of normal science and a life of scientific revolution.

BIBLIOGRAPHY

Nicolas Bourbaki, Faits et légendes, by Michèle Chouchan, Éditions du Choix, Argenteuil, 1995.

"Nicholas Bourbaki, Collective Mathematician: an interview with Claude Chevalley," by Denis Guedj, translated by Jeremy Gray, *The Mathematical Intelligencer*, vol. 7, no. 2, 18–22, 1985.

Les Mathématiques et l'Art, by Pierre Cartier, Institut des Hautes Études Scientifiques preprint IHES/M/93/33.

"The Continuing Silence of a Poet," by A. B. Yehoshua, in *The Continuing Silence of a Poet: collected stories*, Penguin Books, 1991..

Erratum

Due to an error in the computerized typesetting files for *Math Intelligencer* vol. 19, no. 4, some of the characters that were supposed to be superscript or subscript instead appeared as normal type. Instances of this problem occurred in "The Miraculous Universal Distribution," by Walter Kirchherr, Ming Li, and Paul Vitanyi, p. 10, and "Numerical Distances Among the Spheres in a Loxodromic Sequence," by H.S.M. Coxeter, pp. 41–47.