

Transcription of an interview of

JACQUES DIXMIER

by

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## **Education**

**Can we start with your early years ? You were born in 1924. Where did you grow up and what was school education like in the years between the two world wars ?**

I was born in Saint-Étienne, not far from Lyon. Since my parents were teachers, both of them, we travelled around France, so I am not from a particular place. I lived in Rouen then Lille and then in Versailles, from when I was nine years old. That was the most important place for me.

**Your school education was mostly in Versailles ?**

Yes, in Versailles from 6th degree to mathématiques spéciales (2 years after the baccalauréat<sup>1</sup>) except, due to the war, one year in St.-Brieuc, which is a small town in Bretagne. Even there I had a very good teacher in mathematics.

**And that was probably important ?**

That was extremely important. I had very good teachers in mathematics for my whole period in school. Only during the 6th grade, the teacher was absolutely zero. But after that, during almost 10 years, each year I had a wonderful professor of mathematics. That is probably one of the main reasons for my vocation.

**Can you remember a particular instance that made you think : mathematics, that is something for me ?**

I always liked mathematics but originally I did not believe that I was able to do research. I realised that I liked teaching and I thought I would be a good professor of mathematics. That was my main reason for continuing to study mathematics - and then to enter the Ecole Normale Supérieure.

**Do you have any family with background in science or mathematics ?**

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1. 1. Graduation from High School.

Not that I know of. My father would have preferred me to become an engineer. When he saw I was good at mathematics, he thought that I should enter École Polytechnique. He himself was a teacher - in French, Latin and Greek. He made no serious opposition when I said that I would prefer the École Normale. My mother always accepted all my plans.

**Before entering the ENS you had to go to École prépara toire ?**

Yes. After the baccalauréat, some people in France go to a so-called Classe préparatoire. In my times, that was called mathématiques supérieures and then mathématiques spéciales.

**Was that in Paris ?**

No. It was in Versailles.

**You entered the ENS at age 18 in 1942 ?**

I was at the ENS from 1942 to 1945 or 46 ; the last year is a little bit dubious. Before 1945, ENS education in mathematics lasted three years. At the end of the war, a 4 th year was added. But I worked at the CNRS<sup>2</sup> - still quite new - at the same time. In fact I wrote my thesis during this year.

**Can you tell me about your reminiscences from your student time at the ENS ? How was it like to study in these difficult times, under German occupation ?**

Many people in France had not enough to eat but that was not the case for me. Food was not good but it was enough ; we had heating as well. You probably want to hear about my attitude towards resistance<sup>3</sup>. I joined the resistance but only at the end. Only very little was known about that because students in the resistance would not tell you about it ; it was too dangerous ! In fact, the resistance became much more important when we were forced to go to Germany to work in factories ; the so-called “service du travail obliga-

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2. 2. Centre national de la recherche scientifique

3. 3. French resistance movement

toire (STO)”<sup>4</sup> was introduced in 1942. I was not concerned with that except in the last month because I was still too young. But many of my friends were old enough. Many of them left the ENS and the country to avoid the STO. In fact, one of my closest friends went first to Spain, then Portugal, then England and he studied in order to become a pilot. He just finished his studies on 8 May 1945, the last day of the war.

**What did it mean for you to be a member of the resistance ?**

I suppose if I had been arrested I would have been imprisoned. I never had an arm. I knew nothing about arms. I participated in some very small meetings.

**And then, liberation came in August 1944 in Paris.**

Yes. In fact the Gestapo came to the École normale supérieure a few days before that. Finally they arrested Georges Bruhat<sup>5</sup> who was the director at the time.

**What happened to him ?**

He was transferred to Germany and he died there in a camp. He was already old ; I do not know much about the details. The general secretary of the ENS, Baillou, who was also taken to Germany, survived and returned to Paris. In fact, they came during the holidays in August and they arrested several pupils, including me. There is something which concerned me personally that I still do not understand. I was at the ENS doing something that was probably illegal. I heard some noise but I did not know what happened. So, I went to the entry of the ENS. There were two officers of the Gestapo. They cried out and I was taken completely by surprise. They fired and I don't understand how I was not wounded. They took me and I was sent to detention in an office. Finally, instead of taking several pupils, they took the director and Mr Baillou, who probably proposed themselves in place of the pupils.

**Did school teaching still go on as usual, as before and after the war ?**

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4. 4. Compulsory labour service

5. 5. Gorges Bruhat (1887-1945)

There is a very big difference between now and then but that happened gradually. There was no big rupture in the form of teaching between 1944 and 1946, for instance. During my stay at Ecole Normale, I had courses by Cartan<sup>6</sup> and de Broglie<sup>7</sup>. We understood nothing of de Broglie's courses; we were there, there was a blackboard, de Broglie would only talk and write nothing at all. We had great difficulties in understanding Cartan but we tried. With de Broglie, we did not even try.

**Which of your teachers at the time were important for you and your future career ?**

My main teachers were Cartan and Julia<sup>8</sup>. I had Cartan already as a teacher for two years at École normale supérieure. More importantly, after that I attended Séminaire Cartan and later on, I was a colleague of Cartan at Bourbaki. So, the importance of Cartan for me is immense but this dates mainly from the time after the two years at École normale.

Julia was important for a different reason. I followed his course in 1943 during my first year at École normale. Julia gave a course every year in advanced mathematical analysis. This sort of course, which was also given by Montel<sup>9</sup>, Denjoy<sup>10</sup> and others, was too difficult for us students.

But we were very anxious to have a diploma as soon as possible because of the war; you did not know what could happen in a few months! Julia was an extremely good teacher and we understood him. In fact the level of his course about Hilbert space was not very high but we understood almost everything. As a result, Hilbert space became for me as familiar as ordinary space. That was certainly the reason for my research orientation later on. It is very important to be completely familiar with the basics of your subject.

**Thesis under Gaston Julia**

**I would like to ask you a bit more about your supervisor, your directeur de thèse, Gaston Julia. Nowadays he is famous because**

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6. 6. Henri Cartan (1904-2008)

7. 7. Louis de Broglie (1892-1987)

8. 8. Gaston Julia (1893-1978)

9. 9. Paul Montel (1876-1975)

10. 10. Arnaud Denjoy (1884-1974)

**he had already as a young man studied the iteration of rational functions in the complex plane. Many laymen have seen pictures of Julia sets, etc. Was he well-known at the time? What sort of a person was he?**

First of all, I think he was a very good mathematician. When he was about 40 years old, he became more interested in academic questions than in real mathematics. In particular, I don't think he was very much aware of the recent developments concerning Hilbert space - fortunately for me because that made it easier for me to understand what he was talking about! Another important point about him is that he was very severely wounded during World War I; he lost his nose and wore a mask and he suffered very much. He wrote his thesis, which consists of two big papers, while he was at the hospital. I don't think he had a patron de thèse in the usual sense.

**Oh, I wanted to ask you about this! I remarked that the American Mathematics Genealogy Project does not indicate a directeur de thèse for Julia?**

I tried to solve this problem a few years ago. A young girl who was a student of Mathieu<sup>11</sup> knew that Mathieu was a student of Dufflo<sup>12</sup>, that Dufflo was a student of me and that I was a student of Julia. So she came to me and asked me, could I tell her who was the directeur de thèse of Julia. I did not know and so I looked at his first two big papers. There is no hint of a thesis director, not even anybody who inspired him to write these papers. I asked his grandchild, who is a mathematician as well, and he does not know either. One would have to look through the archives of the Sorbonne. I still don't know.

**Do you know who inspired him to look at these questions?**

Probably Picard<sup>13</sup>. I was recently informed by Michèle Audin (Strasbourg) that the jury de thèse (assessment committee) for Julia consisted of Picard,

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11. 11. Olivier Mathieu

12. 12. Michel Dufflo

13. 13. Charles É. Picard (1856-1941)

Humbert<sup>14</sup> and Lebesgue<sup>15</sup>. Unfortunately, I am not competent concerning Julia's work on functions of a complex variable. In his courses about Hilbert space, he tried to make an analogy between these two topics. I think this analogy was farfetched but maybe I missed some important point. One cannot be sure.

**How was he as your director de thèse ? How was the collaboration like ?**

He was not an easy person. As you probably know, he was politically at the extreme right and hoped for a German victory during the war. He and my father were fellow students at École normale for two years : my father in literature and he in mathematics. So they knew each other. Julia had lived in Versailles and my father was a professor at the grammar school in Versailles. That made things easier for me. Julia had six sons. My mother, who was a teacher in elementary school, taught several of them. So there were links between the two families.

**Did he propose a subject for your thesis ?**

No, but I was certainly inspired by some of the things he had told us - generally speaking, on Hilbert space. In my thesis, I studied some particular subspaces, non-closed but not arbitrary - very special subspaces ; my entire thesis is concerned with this class of subspaces. Julia had talked about that but I don't remember any details. As you know, when you are completely embedded in a subject, this subject becomes your own. But I am sure he played a role in this choice.

**Did you consult him during your thesis work ?**

Yes. Finally, I was summoned at the end ; it was probably in April 1947. He was rather critical in his appreciation. But the main point was that he finally said that this would be a thesis.

**Career**

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- 14. 14. Marie Georges Humbert (1859-1921)
  - 15. 15. Henri Lebesgue (1875-1941)

**You had already started to work at the CNRS while you wrote your thesis ?**

As I said, during 1945-46 I was at the same time pupil at the École normale supérieure and employed at the CNRS. I did the main part of my thesis during that year ; after that, during the following year, I was only at the CNRS. In 1947, I was nominated, due to Julia's influence I am sure, as maître de conférences<sup>16</sup> in Toulouse. So, I was a thèsard (PhD student) only for two years.

**How long did you stay in Toulouse ? And how were the conditions ?**

I was in Toulouse for two years. It was tiring for me because I lived in Paris. My wife was a professor of mathematics at grammar schools in several towns in France. The lodging question was extremely difficult. We had a one room apartment in Paris. We travelled a lot and travelling by train from Paris to Toulouse was not easy at all in 1947. The bridge over the Loire at Orléans had not been completely repaired.

After the two years in Toulouse, there was a new position in Dijon in 1949. Things were very easy for a young mathematician at that time because the number of students was exploding. The government created new positions so I had no difficulty at all. Compared with young mathematicians now, I was very privileged. Dijon was much closer to Paris. I was there for six years and after that, in 1955, I was nominated for a position in Paris at the Institut Henri Poincaré.

The number of students was enormous. I held my courses in a place called Conservatoire des Arts et Métiers ; there was a big auditorium. During the first 2 or 3 years, I had 500 students. Jussieu<sup>17</sup>, my later workplace, was only created several years later.

**At that time it was just the University of Paris ?**

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16. 16. Associate professor

17. 17. For many years the home of the Universities Paris-6 and Paris-7.



Yes. You must make a distinction between the creation of Jussieu and the creation of the different universities of Paris. After 1968, instead of one Université de Paris, some 13 or 14 new universities were founded. I stayed at Jussieu.

I took my retirement when I was 60 years old, for personal reasons. A new law had just made that possible.

## Research

**Can we continue with your research work? Can you give me an account of the research areas that you have been most active in? To begin with operator algebras - coming from Hilbert space that you knew so well?**

Being familiar with Hilbert space, I was able to read easily a lot of papers but mainly the work of von Neumann<sup>18</sup> - a lot of papers of von Neumann and in particular his papers, some of them with Murray<sup>19</sup>, about operator algebras.

**Which were motivated by quantum physics at the time?**

He explained that in the introduction of the first paper (there are five or six big papers). But also by the theory of unitary representations! For me that was the main point because I never really understood quantum mechanics. But I understood unitary representations. These papers were prophetic, which was not evident in 1936, the year of their first paper on operator algebras. It became evident ten years later when I wrote my first papers in 1946, immediately after my thesis. I was helped by a friend Godement<sup>20</sup>, who was also a student at École normale. I did not know him at École normale; he was two years older than me. He explained to me a lot of things about unitary representations. I even had the privilege of reading some papers by Godement that he never published; he was reluctant to publish. So I studied what I called von Neumann algebras.

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18. 18. John von Neumann (1903-1957)

19. 19. Francis Joseph Murray (1911-1996)

20. 20. Roger Godement (\* 1921)

### **Is it true that you coined the notion von Neumann algebra ?**

No, that was an idea of Dieudonné<sup>21</sup> ; he proposed this name during a discussion at Bourbaki. I thought that this was self-evident ; I should have thought of it myself ! I then used this notion in my book on operator algebras<sup>22</sup> that appeared in 1957. After that, I still studied operator algebras, but more from a C\*-algebra point of view, and at the same time unitary representation theory. Godement had played a big role for me but I was also influenced by all the papers of Harish-Chandra<sup>23</sup>. Then I began to work on enveloping algebras<sup>24</sup> (of course, connected to my previous work). I wrote a book about C\*-algebras in 1964 and then my book about enveloping algebras<sup>25</sup> in 1974. Then I got interested in invariant theory. There is a connection between these two things. It is a little technical, so I will not explain it here. After my retirement, I went to the IHES<sup>26</sup>. I was much freer in my work. I did a lot of work about invariant theory ; moreover, there is also a connection with the theory of partitions. I did a lot of work but I realised that this work was less good than what I had done previously. I am not a great mathematician but I am a good mathematician. My papers from this period are technically difficult but less important than what I had done before. I think this is due to age ; this is the only reason I can see, since I had the best possible facilities to work in.

### **Probably partition theory is also a very difficult topic ?**

Yes, but no more than operator algebras. There are a lot of people who say : ooh, operator algebras !

### **Are there any of your results that you are most proud of - if you could single out a few ?**

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21. 21. Jean Dieudonné (1906-1992)

22. 22. Les algèbres d'opérateurs dans l'espace hilbertien (Algèbres de von Neumann), Gauthier-Villars, Paris, 1957

23. 23. Harish-Chandra (1923-1983)

24. 24. Les C\*-algèbres et leurs représentations, Gauthier-Villars, Paris, 1964

25. 25. Algèbres enveloppantes, Gauthier-Villars, Paris, 1974

26. 26. Institut des Hautes Études Scientifiques, Bures-sur-Yvette

I have to think, I was never asked about that : maybe the paper where I prove that real algebraic groups are of type I; and also some papers about enveloping algebras; and also a paper, which is not well-known because it was completely absorbed by Harish-Chandra later on, about the De Sitter group; and finally, a paper about quasi-unitary algebras.

**When you think about how you achieved your results, was that usually very systematic or would it sometimes occur that you had a sudden jump of inspiration ?**

Both! At least for every important paper, I needed a lot of time and a lot of work - and of course, at some points the ideas come. A particular idea came when I was travelling in a bus.

**Oh, that reminds me of a similar comment by Poincaré<sup>27</sup> !**

Yes, and Serre<sup>28</sup> told me about having an important idea on a train journey<sup>29</sup>. That may be one of the reasons why ordinary people judge us as abstracted people. I don't know how my face looked like in the bus when I suddenly had this idea. I was certainly not interested in what was going on around me.

**Coming back to von Neumann, did you ever meet him personally ?**

Yes, very little, the first time in 1947. He came for the first time after the war from the United States. He stayed at Hotel Lutetia, which is a very big hotel in Paris, and many people came to him. I was extremely flattered that he proposed that I should come and see him.

**He knew about your papers ?**

I had not written anything about operator algebras at the time. I had only my first two papers to give to him. He was, I think, extremely kind because

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27. 27. Henri Poincaré (1854-1912)

28. 28. Jean-Pierre Serre (\* 1926)

29. 29. Also reported in : M. Raussen, Chr. Skau, Interview with Jean-Pierre Serre, Newsletter of the European Mathematical Society<sup>30</sup>, 18-20

I realised later that they were not good papers.

**You were very young at that time ?**

Yes, I was 23 by then. Later, I wrote to him. I think I saw him one more time at the Amsterdam Congress in 1954. I heard a little later that he was very ill. I wrote to him several times. He did not answer immediately but he answered. He was not working on algebras any more but he took pains to answer me.

**Are there other mathematicians that you have a particular admiration for ?**

I have to limit myself to a few whose work I understand : certainly Gelfand<sup>31</sup> and Harish-Chandra whom I already mentioned. I did not collaborate with them but I met them. I also received them at home. I met them at some congresses, Gelfand at the ICM in Moscow in 1966 and later on in Hungary and several other places. I met Harish-Chandra also in Moscow where he was an invited speaker and several times in Paris. But I think I never met him in the United States where he lived up to his death. As you know he died relatively young.

**Did you travel a lot in general ?**

I did not travel a lot - of course, more than an ordinary Frenchman but less than an ordinary mathematician. The reason was that I did not want to leave my family. Anyway, since I was a member of Bourbaki, I was absent from my family for one month every year, and I found that was already very much. I refused most of the invitations with one exception : I lived with my family for one academic year in New Orleans. In fact, I often went with my family to the summer congress of Bourbaki. So I was not always separated.

**Bourbaki**

**You became involved in the work with the Bourbaki group already as a young man ?**

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31. 30. I. Gelfand (\* 1913)

Yes, but not younger than most of the others from that generation : Godement, Koszul<sup>32</sup>, Samuel<sup>33</sup>, Serre and then a little bit later Borel<sup>34</sup>, Cartier<sup>35</sup>, Bruhat<sup>36</sup>, Douady<sup>37</sup>. We were very young all of us when we became members.

**More or less at the same age ?**

Yes, more or less around 25 years old.

**Were you asked to join ?**

That was due to Serre and Samuel. Serre has probably explained to you that he almost forced himself into Bourbaki. There was a Bourbaki congress at École normale supérieure. During the first years after the war, it was difficult to meet at a hotel, so Bourbaki held meetings at the ENS. Serre was a pupil there and he just asked to listen. From his questions they quickly understood that he would be a very valuable member.

Pierre Samuel is a Jew. He was a pupil at École normale supérieure but he had to remain hidden during the war. He and Cartan knew each other, probably dating back to the entrance competition to the ENS. Just after the war he went to America, where he was a pupil of Chevalley<sup>38</sup>. Anyway, he became a very young member of Bourbaki, one or two years after the war. One day in 1949, Serre and Samuel, who both knew me, approached me and asked whether I would accept to become a member of Bourbaki. At the time, this was extremely flattering ; I would have jumped ! It turned out to be very important for me because otherwise I would probably have worked on Hilbert space all my life. Bourbaki forced me to learn a lot of other subjects.

**How was collaboration in Bourbaki ? How did you progress from an exchange of ideas to drafts and to the final volumes of *Eléments***

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32. 31. Jean-Louis Koszul (\* 1921)  
33. 32. Pierre Samuel (\* 1921)  
34. 33. Armand Borel (1923-2003)  
35. 34. Pierre Cartier (\* 1932)  
36. 35. François Bruhat (1929-2007)  
37. 36. Adrien Douady (1935-2006)  
38. 37. Claude Chevalley (1909-1984)

## **de Mathématiques ?**

Well, most of this is well-known by now<sup>39</sup>. Usually, there were five or six drafts on the same subject written consecutively by different people.

The first draft for the volume Algèbres de Lie was written by Koszul when I was already a member of Bourbaki. I wrote the second or the third draft on the subject and, I think, also the final one. In theory, a version is not accepted by Bourbaki unless everybody agrees. In reality, after having discussed five or six consecutive drafts, some people might still not be completely satisfied but they have then become too tired to disagree. Well, for Algèbres de Lie, there were no serious disagreements.

## **I think this book was the standard source for a long time to come !**

If you take into account not only the first chapter, which is about the general theory of Lie algebras, but also the chapter about root systems as part of the subject and then the chapter about semi-simple Lie algebras which were written a little later. I think the idea of an independent theory of root systems is due to Cartier, who explained that to Bourbaki, as I remember, at the blackboard. We were convinced and decided to make a separate study of root systems, in general. Cartier found the axiomatics and also wrote the first of several drafts on the subject. I was involved in the second or third draft ; at the time, I had to use Coxeter's book about polytopes<sup>40</sup> ; at certain points, it was not very satisfying. The book is very beautiful but it uses sometimes ad-hoc methods building on geometric intuition - not really suitable for Bourbaki. It was only later that Borel found algebraic proofs of everything and that was beautiful, too. I wrote one of the last drafts. I was good at writing ; I had practically no original ideas for Bourbaki but I think I was instrumental for the writingup process.

## **How could all the different personalities work together at Bourbaki ? I was often quite puzzled when reading about the Bourbaki meetings and congresses. They seemed to have an almost anar-**

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39. 38. More about this topic in A. Borel, Twenty-five years with Nikolas Bourbaki (1949-1973), Notices Amer. Math. Soc. 45 (3) : 373-380 ; M. Mashaal, Bourbaki : une société secrète de mathématiciens, Pour la Science, 2002 ; English translation : AMS. 2006

40. 39. H.S.M. Coxeter, Regular Polyopes, Macmillian, 1963

**chistic character ; but at the end, they resulted in these very rigid volumes !**

That is an interesting remark ; you are probably right. Bourbaki was also a big machine. And the final versions which were sent to publisher were almost always typewritten by Dieudonné so they were very much influenced by Dieudonné's personal style - but everyone accepted that.

**Was André Weil<sup>41</sup> still an important figure in your times ?**

Certainly, but mainly by correspondence since he was in America when I started to work with Bourbaki in 1949. I certainly did not see Weil during my first year at Bourbaki. Of course, his influence was enormous, in particular during the first years of the Bourbaki enterprise before the war. After that, his influence was mainly felt through what he wrote. He participated in some but not all of the meetings ; I would say he was there about half of the time - this can be checked in the archives. He could even have been influent for a longer time but he had decided himself that the age limit was 50.

**Was this age limit generally accepted ?**

Yes. Weil was invited to the congress shortly after he had become 50 years old. It was held as usual but when the last day ended with a resuming meeting for the congress and decisions for the next one, Serre and Borel asked Weil to stay away from this meeting. They thought that if you are no longer a member of Bourbaki, you should no longer have any influence. Weil was certainly not happy but he did not object. Many of the people present said nothing and would have preferred him to stay. But if somebody objected, we all did. Those were the rules of the game.

**How about Dieudonné ; did he stay after becoming 50 years old ?**

No, but he was still often invited to the congresses. I do not remember whether he still wrote some final versions of the Bourbaki books.

**Just a few days ago, we had the celebration of the 60 years of Sé-**

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41. 40. André Weil (1906-1998)

**minaire Bourbaki<sup>42</sup> at Institut Henri Poincaré; Jean-Pierre Serre gave the lecture number 1000. You have given several lectures for this famous seminar.**

Not that many, five or six perhaps; far fewer than Grothendieck<sup>43</sup> or Serre. I am not angry but I think Bourbaki is no longer Bourbaki. The seminar is all very well but the reason for Bourbaki was to publish books.

**It no longer has the influence it had back in the 50s and 60s?**

I suppose there are still meetings at Bourbaki but what do they do? They are perfectly able to write books! For some of the subjects, we had written drafts that have never been published and that is a pity, I think. These drafts were of course just the beginning; they should have worked more on them. For a long time, nothing appeared and about 10 years ago, a single ninth chapter appeared in the volume on commutative algebra, I think. It is a pity that nothing has happened ever since.

I remember that Demazure<sup>44</sup>, who worked very actively with Bourbaki, expressed that you often may have the feeling that if you sit down, think about a topic for enough time and then write down your findings, then after some time you have found everything of interest; he said that this feeling is wrong. He was right because mathematics changes all the time. Maybe it is true that the story had to stop. But not so soon, I think! In the drawers, there are drafts for chapters on, for instance, complex semi-simple Lie groups, on class-field theory. Also, several more chapters on spectral theory had been written.

**Research Education, PhD students**

**My next questions are concerned with your PhD students. You have had several PhD students?**

Yes. I was a directeur de thèse (PhD supervisor) for twenty students. Moreover, there was a number of students who wrote their thèse de 3ième cycle

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42. 41. Three times a year, with five lectures nowadays

43. 42. Alexandre Grothendieck (\*1928)

44. 43. Michel Demazure



with me. The best of my PhD students was certainly Alain Connes<sup>45</sup>. He appeared in my seminar. Usually, I knew the participants of my seminar beforehand because they had followed one of my courses. I was stuck - who is this man? A few months after that, he offered me a four page paper. I understood immediately that his result was very important and that he had given a very simple proof - absolutely amazing!

Connes said several times that Choquet<sup>46</sup> had been very important for him; he also mentioned me very generously.

**I assume you had more collaboration with him and your other students than you had yourself with your supervisor Julia?**

Yes, but after all it is difficult to compare. For my 20 students, supervision was very different from one to another. In fact, Connes worked mainly on his own. Roughly speaking, after a few years, he brought me his thesis. I read it and asked him to change a few commas. So with him, in a sense, it worked out the same way as between me and Julia.

Let me also mention Michel Duflo and Michèle Vergne. With Duflo, it was essentially very much as with Connes; he worked independently. Michèle Vergne wrote several papers. We also wrote a paper in collaboration, after her thesis. During her thesis work, I read everything she wrote and gave some advice but that was not very important. With some other students, I had to contribute in a more substantial way. I have collaborated with some of my students in joint papers : with Bernat, Duflo, Vergne, Maréchal, Berline and Brion.

**Are you still in contact with your former PhD students?**

With several of them; my first PhD student Alain Guichardet and his wife are very good friends. I also see some of the others from time to time - and Connes is a friend; I meet him and his wife occasionally.

**Books. Teaching.**

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45. 44. Alain Connes (\*1947)

46. 45. Gustave Choquet (1915-2006)

**You are the author of a series of textbooks.**

Not really many. I wrote two books for the first two years<sup>47</sup>, called first cycle at the time. They were in fact quite important for me, since they sold very well and thus they were a source of income - not big money, of course! Otherwise, I wrote a textbook on general topology<sup>48</sup>, which was not a very big success. Then there was a book about Lebesgue integration<sup>49</sup>. And then, I have my research books<sup>50</sup> but that is another story.

**Did you have a particular philosophy of how to teach and how to write? Was it close to the Bourbaki style?**

That would probably be true for the written text. For oral presentations, I have certainly a philosophy that differs from most of my colleagues. In order to make good talks, I prepared them word for word. Most of my colleagues think that this is a far too formal attitude, that you have to rely on inspiration. I do not believe that at all. I have often heard the opinion that it is good to get stuck; otherwise students do not understand that there is a real difficulty. I must say, I do not agree at all. I was usually very careful to write in big letters, to avoid talking into the blackboard and so on.

I really wanted to be different from Denjoy; it is said that, during a lecture, he thought a, he said b, he wrote c and d would have been correct! In fact, when I was a student at the ENS, during my second year, I tried to follow a course given by Denjoy. After the second lecture, I left; it was hopeless for me. Well, not for everyone; Choquet was a pupil of Denjoy. I know that the written work of Denjoy is very good. But as a teacher, he was terrible. For written text, I must admit that I like the Bourbaki style!

**Probably it depends on how you use it?**

Yes, of course. You can be extremely Bourbaki or not at all; I would place myself somewhere in the middle. But I like the theorem-proof structure. This

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47. 46. J. Dixmier, Cours de mathématiques, 1ère année, 2ème année, Dunod

48. 47. J. Dixmier, Topologie générale, PUF, 1981

49. 48. J. Dixmier, L' intégrale de Lebesgue, Les Cours de Sorbonne, 1962

50. 49

is probably an effect of what I suffered from during my education at the ENS. During that, my fellow students and I read the books of Goursat<sup>51</sup>, Picard<sup>52</sup> and Darboux<sup>53</sup>. To understand what was going on, then you would sometimes have to guess and jump 40 pages back and forth. I did not like that and, probably as a reaction, I loved a very formal presentation, which of course, may be very effective at the same time. I like brevity, although I can see disadvantages as well. For presentations, it is probably best to mix a formal part with motivations, applications and so on. But I do not know how to do that in the best possible way.

**And of course it depends on who listens ; students are different !**

Well, I think one cannot talk about an “ordinary student”.

**Some people like geometric intuition ; others despise it...**

For instance, Grothendieck told us at Bourbaki about the time when he was a student in Montpellier. He said that he did not understand the theory of functions of a complex variable at all. The professor would just draw loops and so on ; for Grothendieck, that was not mathematics. I had such a course with Valiron<sup>54</sup>, who was a very boring teacher but his course was very good. He made drawings and I accepted that. I did not insist on perfect rigour at the time.

**Mathematics after retirement**

**Let me ask you about mathematics after your retirement. You explained that you stayed at the IHES for five years ?**

Yes, in an informal manner. I was not invited but I came to live in Bures-sur-Yvette. Some friends from the institute kindly offered me an office. I came to the IHES on a daily basis ; it was an extraordinary environment. I had never had that much stimulation except at Bourbaki.

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51. 50. E. Goursat, Cours d'analyse mathématique

52. 51. É . Picard, Traité d'Analyse

53. 52. G. Darboux, Leçons sur la théorie générale des surfaces et applications géométriques du calcul infinitésimal

54. 53. Georges Valiron (1884-1955)

**And you participated in lots of discussions and collaboration at the time ?**

Not a lot but much more than in Jussieu. That, in the end, was dominated by administration, meetings, boards, etc. I had in fact succeeded in avoiding much of the administration. At the IHES, there was nothing of that kind. And I could ask Connes, Jones<sup>55</sup> and others.

**Did you follow the development that came from Jones' work, the interplay with knot theory for example ?**

A little, certainly! The few things I know about knot theory come from the acquaintance with Vaughan Jones at the IHES but I never used them in my own work. On the other hand, Jones asked me questions about enveloping algebras and I was once able to provide a counterexample.

**Do you still write research papers ?**

I stopped working regularly when I was 68. I was at the IHES from age 60 to 65. Then I wrote to the director, Marcel Berger, that it was not correct to use an office because I was too old and I came too little. After I had stopped, at two occasions I could not resist pursuing certain ideas. This work resulted in two papers. I wrote the last one at age 82; it is about triangle geometry<sup>56</sup> Well, not ordinary triangle geometry. We associate to a triangle a certain point and define in this way a nowhere differentiable function of two variables; we only use methods of classical analysis. I think I would be unable to work on a really modern subject. For instance, I had a very hard time trying to read Alain Connes' new book<sup>57</sup> which he presented to me.

**Do you still come to seminars ?**

A few times! For instance, I did not come to the last Séminaire Bourbaki be-

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55. 54. Vaughan Jones (\*1952)

56. 55. J. Dixmier, J.-P. Kahane, J.-L. Nicolas, Un exemple de nondérivabilité en géométrie du triangle, Enseign. Math. (2) 53, no. 3-4, 369-428

57. 56. A. Connes, M. Marcolli, Noncommutative Geometry, Quantum Fields and Motives, Colloquium Publications 55, AMS, 2008

cause I feared not understanding the lectures. I was a little in doubt because Serre is such a good lecturer.

**Do you use a computer ? Do you read and write emails ?**

No, I have no computer and do not use email. If I were ten years younger, I would have been forced to use them. Computers began to become useful for a mathematician when I was around 65 years old ; too late for me ! And everywhere people of my age told and tell me that they have difficulties with their computers. I did not want to bother !

**A secret life has advantages ? !**

In essence, yes.

**Other interests**

**Can you tell me about your interests apart from mathematics ?**

I wrote two science fiction books - well, short stories, not big novels.

**Was that a long time ago ?**

Not so much. I was between 60 and 65 for the first and a little older for the second. These are two small groups of short stories.

**They were published ?**

The first one sold 600 copies. For the second one, I do not even know. After that I wrote a detective novel and sent it to the same publisher but he said that my books were too difficult to sell.

**But you enjoyed writing them ?**

Yes. But I do not do it anymore. I lack new ideas !

**You are a big reader, it seems !**

You see only a little part of my library. But you are right, one of my main occupations now is reading.

**I would like to thank you very much for this interview !**

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The 15 th ICMI Study on The Professional Education and Development of Teachers of Mathematics Mariolina Bartolini Bussi. At the beginning of 2009, the volume The Professional Education and Development of Teachers of Mathematics was published, edited by Ruhama Even and Deborah Loewenberg Ball, as the outcome of the equivalently named ICMI Study. As the concern for teacher education was one of the leading forces for the birth of the ICMI in 1908, it might be considered that this study is quite late in coming, being from the fifteenth in the ICMI series.

The premise of the 15 th ICMI Study is that teachers are key to the opportunities that students have to learn mathematics. What teachers of mathematics know, care about and do is a product of their experiences and socialisation, together with the impact of their professional education. The Professional Education and Development of Teachers of Mathematics assembles important new international work - development, research, theory