An interview with Jean-Pierre Serre

Javier Fresán

Jean-Pierre Serre will turn one hundred years old on 15 September 2026. On that occasion, a conference in his honour will be held at the Institut Henri Poincaré. When he suggests that I conduct what he calls "my first centenarian interview," my thoughts turn to Bernard Pivot's conversation with Marguerite Yourcenar at her home in Maine. "I had prepared that interview with the utmost care," Pivot recalls, "because I knew there was a risk of not rising to the occasion. Some writers are indulgent towards journalists and willingly accept questions that are less than compelling, but I knew that Marguerite Yourcenar, although very kind and welcoming, was not one of them. I could not afford to let her down."

The first part of the interview takes place on 19 April 2026, in Saint-Sulpice, where Serre now lives. Lac Léman is a three-minute walk away. On the train from Paris, I write each of my questions on a separate card. We talk without interruption for nearly five hours. He often gestures with his hands like a southerner. Towards the end of the interview, he chooses the next question at random by pointing to one of the cards.

I return to Saint-Sulpice the week of 6 July. I am there when he receives copies of the "fifth and last volume" of his collected works, containing most of his writings from 1998 to 2025. In the spirit of Bourbaki, we go through his answers together, reading them aloud line by line, pruning adverbs and adjectives, and "replacing false theorems with others."

Figure 1. Serre at a restaurant in Feutersoey. (Photo by Eva Bayer-Fluckiger)

Beginnings

Javier Fresán: *When did you realize that you were a mathematician?*

Jean-Pierre Serre: I realized that mathematics was what truly interested me when I was about sixteen. I was also interested in chemistry at the time, but I gave it up. As for becoming a professional mathematician, it took me a while to understand that you could earn a living as one. In those days, you could go straight from the École Normale Supérieure into the CNRS without having proved anything, simply on the strength of your potential. Well, I had ranked first in the *agrégation*, but that was purely pedagogic.

JF: *Ranked first despite your mishap at the oral exam …*

JPS: My first lecture was on lines on quadrics, a topic I knew well. I had even gone over it half-asleep the night before, so everything went perfectly. The second lecture was on prime numbers. I began by saying, "A prime number is …," and noticed that the examiners looked puzzled, but at the time they were not allowed to interrupt. When I was done, I suddenly thought, "Wait a minute, how did I define prime numbers?" I had managed to give the only incorrect definition one can think of: "A number is prime if it is divisible by neither 1 nor itself!"

JF: *That takes some doing!*

JPS: I have another story about the *agrégation*. The written exam was an analysis problem, based on an article by Marcel Riesz. I spent

Figure 2. Serre at his office, in 2026, with the *Éléments* by Bourbaki. (Photo by Javier Fresán)

about an hour trying to get from one equation to the next one. Riesz had probably written "by a change of variables," and given the formula, but the examiner (Jean Favard) thought that would be too easy for the candidates; he gave no hint on how to change variables.

After the exam, I came back to the École Normale. I went up to the first floor and passed a room where members of Bourbaki were discussing integration. I happened to arrive just as they were deciding to replace the language of σ-algebras with that of Radon measures (linear forms on the space of continuous functions). That really interested me, and that's why I later started attending Bourbaki meetings.

Bourbaki

JF: *Was that your first contact with Bourbaki?*

JPS: I don't know how, but I found out that the next meeting was in Nancy in October 1948. At the time, I was living in Auxerre, because my wife was teaching there. Without telling anyone except her, I took the train to Paris, then another train to Nancy. I booked a room at a hotel, and the next day I went to the mathematics department. There was a large lecture hall where the Bourbaki meeting was taking place. I sat down at the back at first, then gradually moved closer and closer, and eventually I started taking part in the discussions. It didn't seem to bother anyone.

JF: *Who was there?*

JPS: Probably Cartan, Chevalley, Delsarte, Dieudonné … but what I remember most is something I said. They were discussing the chapters on field theory (*Algèbre V*), and Chevalley wanted to remove Galois theory because it's too technical. I intervened and declared, "Galois theory is useless." [I did not know that I would in fact use it for most of my life.] The Bourbaki members did not hold this childish sentence against me, and I was invited to the next meeting.

In the end, there was a compromise with Chevalley, who said, "If we include Galois theory in this chapter, then we should also do Lie algebras." Bourbaki agreed, and that's how the first report on Lie algebras by Chevalley came to be written.

Later, when Bourbaki invited me to join, I was asked to write something on ordered fields because I had published a short note on the subject. That's how I got started in Bourbaki.

JF: *Reading that first note on ordered fields, one has the impression that your style was already fully formed.*

JPS: That was the Bourbaki style. Did I learn to write by imitating it, or did that simply come naturally? I don't know.

JF: *Which texts did you write for Bourbaki?*

JPS: At the end of each meeting, Dieudonné assigned tasks to everyone, and for two years mine was to finish my thesis.

One text I spent a lot of time on was the *fascicule de résultats* on differentiable manifolds. That was difficult, because it gives only statements without proofs, so you have to work out the proofs in your head as you write. But at the time I was young enough to be confident, and I don't think I let anything incorrect slip through. I included many things that were not in the literature, for example, on foliations invariant under Frobenius on analytic varieties in characteristic p.

I also wrote a hundred pages of exercises (that were reduced to about forty by being set in small type) for one of the Lie volumes. I enjoyed it. For instance, I made up exercises on hyperbolic Coxeter groups, which I had learnt from Koszul's lectures at the Tata Institute, and also on Dickson invariants.

In commutative algebra, I had some influence on the programme. The old drafts used to start with "specializations," and I said: "No, we'll begin with flat modules." We kept valuation theory. Grothendieck, who was then a member of Bourbaki, thought that valuations were useless, but we kept them anyway.

Grothendieck

JF: *Grothendieck had a complicated relationship with Bourbaki.*

JPS: He wasn't made for collaboration.

JF: *I have the feeling that you don't want to talk too much about Grothendieck, am I right?*

JPS: I have mixed feelings.

Few of the things Grothendieck did really surprised me. His early papers on nuclear spaces are very original. It's a theory I explained to Raoul Bott, and he was able to use it in one of his first papers. I was surprised by his Riemann–Roch theorem, because I had never seen statements of that kind over a general base. Étale topology did not surprise me, because I had found it in dimension one.

To me, the *Éléments de Géométrie Algébrique* were a display of great intellectual power, not of imagination. The work is more original than Bourbaki's, but the basic idea is still inspired by Bourbaki; there is a reason why it's called *Éléments*. Except for that, instead of a collective enterprise, it was the work of a single person. He himself used to say, "Once this is done, we'll be able to really do algebraic geometry." Indeed, these foundations have been widely accepted; they answered a real need. One of their good points is the choice of a suggestive terminology, such as "smooth" (lisse) instead of the ambiguous "regular," and "étale."

By that time, I no longer regarded algebraic geometry as a research topic for me, but simply as a working tool. I was drawn to number theory, algebraic groups, Galois representations, and modular forms. One cannot resist modular forms.

JF: *Grothendieck told you that modular forms were nonsense because of the artificial growth conditions at infinity, didn't he?*

JPS: He couldn't appreciate beautiful things that were not directly connected to what he was doing – the exact opposite of Weil's attitude. I tried several times to interest him in modular forms and, more generally, in the Langlands programme, but it was hopeless.

Figure 3. Serre and his father in the Cévennes, around 1928. (Photo by Adèle Serre)

Figure 4. Serre and Merkurjev at the CIRM in 2015. (Photo by Evgeny Plotkin)

Influences

JF: *What influenced you most when you were young?*

JPS: One of the first serious books I read was the two-volume *Moderne Algebra* by van der Waerden. I had an uncle in America who became a millionaire from oil in Venezuela. His daughter, who was my age, managed to send me from New York books that were impossible to find in France in 1947: Banach's *Théorie des opérations linéaires*, Chevalley's *Theory of Lie Groups*, and van der Waerden's *Moderne Algebra*.

A Polish mathematician writing in French; a French mathematician writing in English; a Dutch mathematician writing in German. It was an excellent lesson: I shouldn't be afraid of foreign languages.

JF: *Are you an heir to the German school?*

JPS: Yes, in a sense. Mainly through my elders. Weil, Chevalley, and Dieudonné had gone to Germany to learn mathematics, because in France the main, indeed almost unique, subject at the time was functions of one complex variable.

I went on to read a great deal of German mathematics. As a student, I learnt more from *Moderne Algebra* than from Bourbaki. At that time, only the first version of *Algèbre: Chapitre I* existed.

The two books by Seifert and Threlfall were also important to me: *Lehrbuch der Topologie*, which covered the basics but also contained material that is still difficult to find today, and their volume on Morse theory, *Variationsrechnung im Grossen*.

Krull's *Idealtheorie* influenced me a little less because I was already working out commutative algebra on my own. It was clear that Krull understood the importance of localization.

JF: *Did you learn class field theory reading Artin?*

JPS: I first learnt it from Weil, sitting at a café near the rue d'Ulm. In less than an hour, he explained the theorems of class field theory to me in cohomological terms. It was a few years later that I read the Artin–Tate seminar. I could even attend part of it when I was visiting Princeton in January 1952.

JF: *What about Frobenius?*

JPS: I edited his *Gesammelte Abhandlungen* (Collected Works). That was a challenge because there was no complete list of his publications. I corresponded with Siegel, who had been a student of Frobenius and lent me many reprints of Frobenius's papers. I had to go through the major journals of his time, volume by volume, to see whether he had published there.

But I already knew Frobenius's results in group theory from standard textbooks. For example, I learnt character theory from *L'Intégration dans les groupes topologiques* by Weil.

I also like Schur's papers very much, but I only read them quite recently, in the last ten or twenty years.

Group theory

JF: *Your most recent book is about finite groups, a subject you have been playing with for most of your mathematical life.*

JPS: I treated myself to an old man's pleasure.

I'm drawn to beautiful, concrete things. For instance, I wrote a whole chapter on "small groups," where I explain *with proofs* facts such as the isomorphism $\mathrm{PSL}_2(\mathbb{F}_9) \cong A_6$. Belief is easier than proof. People usually just say, "Take two elements; they generate the same subgroups …" But my Bourbakist moral is that I use explicit matrices as little as possible. I also enjoyed giving a proof of the well-known fact that, up to isomorphism, there is a unique simple group of order 168.

JF: *Is this your favourite book?*

JPS: No. My favourite remains *Lectures on* $N_X(p)$. Writing it was delightful.[1] Most statements concern schemes of finite type over $\mathbb{Z}$, with no extra assumptions. And then, because I had to explain the notation in the title, it was the only time when I did not have any trouble finding the first line of the introduction: "The title of these lectures requires an explanation …"

[1] AI told me that, among my books, this is the most difficult to read for students. I write for mathematicians, not for students.

Figure 5. Serre with Gross and Ribet during the conference *Applications of Automorphic Forms in Number Theory and Combinatorics* in 2014. (Photo by Bogdan Oporwoski)

JF: *It's rare to prove an interesting theorem about all groups, yet you've managed to prove one.*

JPS: That's what Humphreys wrote in his review of my paper dedicated to Borel. The main result is that, in characteristic p, the tensor product $V_1 \otimes V_2$ of semisimple representations of a group is semisimple, as long as the sum of the dimensions of V_1 and V_2 is at most $p + 1$.

When preparing my course at the Collège de France on torsion points of abelian varieties, I had seen results in this direction in Nori's work, but without a precise bound. Sometimes I ask myself questions because I can't resist finding out. I must have done some calculations and seen that $p + 1$ is a plausible bound. It is optimal for SL_2, and Harish-Chandra had the philosophy that if you understand SL_2, then you understand all semisimple groups. An optimistic point of view.

One day, Broué told me he was going to a conference in England, and he didn't know what to talk about. I told him, "You should mention this conjecture." I have no idea why I said that. When he left, I thought, "But why don't I prove it myself?" I did it. A strange experience …

Happy doing mathematics

JF: *You always give the impression of being happy doing mathematics.*

JPS: Oh yes!

JF: *Were there moments of difficulty when you thought you were not going to make it?*

JPS: I was unhappy during my first two years at the CNRS because I wasn't finding anything except that note on ordered fields.

There was a difficult moment when I was a little over thirty. I had taken a road trip around the western United States with my wife and daughter. I spent a month figuring out which roads to take and looking for motels with swimming pools (for my daughter), and when I returned to the IAS, where I was a visitor, I couldn't work for another whole month.

I don't remember ever being unhappy after that. Showing that the Galois representation attached to an elliptic curve has large image took me roughly ten years, but I was doing other things in the meantime, and I wasn't unhappy because I was making progress.

JF: *Another article that was difficult to write was the one on modular representations of* $\mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$.

JPS: It appeared in 1987, but I had already mentioned a less precise form of the conjecture in a letter to Tate from 1973.

The case of level 1 was clear and fit the examples perfectly. Formulating a precise conjecture in the general case, thanks in particular to Mestre's numerical computations, took a long time. It required understanding the ramification of modular forms with "a nasty level and a bad character," a subject about which I learnt a lot from Fontaine.

I later realized that there are some cases in which the "precise form" of the conjecture is incorrect. The problem has to do with the notion of modular forms modulo p. I had chosen to view them as reductions modulo p of the usual modular forms, a setting with which I was familiar and which could be explained easily. Yet, at a deeper level, I knew that they ought to be defined purely in characteristic p terms, à la Katz.

JF: *That did not diminish your love for modular forms.*

JPS: I always loved playing in the "jardin des délices modulaires," as Godement called it. There are so many different points of view on modular forms that one of them is bound to give you something interesting. It's a perfect playground for optimists.

JF: *Have you ever attacked well-known open problems?*

JPS: Borel explained to me that if I am going to work on an open problem, one that other people have already struggled with, I have to feel that I've got something that others may have missed. That's why I have never tried to prove the Riemann hypothesis.

There are people whose minds are strong enough to focus on a single problem for a long stretch of time. Mine is not like that. It likes to play.

Figure 6. Serre and Fresán during the interview in April 2026. (Photo by Eva Bayer-Fluckiger)

Homotopy groups of spheres

JF: *Cartan was your advisor, but he didn't give you a thesis problem. How did you find it?*

JPS: Cartan was my advisor in name only. I worked a great deal for his seminar, both learning things and writing them up. At first, I was interested in representation theory in infinite dimensions, but I couldn't find anything to do there. In the seminar, I learnt about homotopy groups and Eilenberg–MacLane spaces.

There is a striking duality between these two topics. In the case of spheres, homology is known and it is homotopy we want to determine. For Eilenberg–MacLane spaces, all but one of the homotopy groups are trivial, and we want to compute the cohomology. There are two different sides of the same topic. Whenever you make progress on one side, you make progress on the other.

JF: *Was that observation your starting point?*

JPS: No. I read a note a few lines long in which someone computed the cohomology of the Eilenberg–MacLane space $K(\mathbb{Z}, 2)$ by identifying it with infinite projective space $\mathbb{P}^\infty(\mathbb{C})$.

Since $\mathbb{P}^\infty(\mathbb{C})$ is the quotient S^∞/S^1, and S^∞ is contractible, we get a fibre bundle with contractible total space, base $K(\mathbb{Z}, 2)$, and fibre $K(\mathbb{Z}, 1)$. This suggested that, for an arbitrary abelian group A and an integer n, there exists a fibration $E \to B$ with contractible space E and base $B = K(A, n+1)$, which implies that its fibre F is a $K(A, n)$-space.

I convinced myself that I could construct such a fibration by looking at the space E of paths on B. More generally (as I noticed later), one may start with any connected space X, instead of $K(A, n+1)$, and define $E = EX$ as the space of paths, starting with a given base point x of X (i.e., continuous maps $f\colon [0,1] \to X$ with $f(0) = x$, and define a projection map $p\colon EX \to X$ by $p(f) = f(1)$).

The fibre of x is the loop space ΩX of X. Applying this construction to $X = K(A, n+1)$, one obtains what one wanted.

JF: *Was that the sudden discovery in the train compartment?*

JPS: Yes. I was on the night train coming back from vacation. I woke my wife and said, "That's it, I've got it!" I had found the fibration $E \to EX$ I wanted, except that none of the definitions of a fibre space in the literature at the time had been designed for this. But I was certain it would work.

Assuming also that I could use the Leray spectral sequence, I carried out many computations for Eilenberg–MacLane spaces and spheres. For Eilenberg–MacLane spaces with coefficients in $\mathbb{Q}$, I proved that the cohomology ring is a polynomial algebra. Dually, this suggested that the homotopy groups of spheres are finite.[2] At the time, people did not even know that they are finitely generated.

JF: *I guess you had to overcome technical difficulties.*

JPS: The main one was to show that Leray's spectral sequence applies to "my" fibre spaces, and especially to loop space fibrations. I was saved by Cartan and Koszul. At a Bourbaki meeting (around October 1950), they asked me where I stood with my thesis. Koszul suggested that I use cubes instead of simplices to construct a filtration. There were some annoying lemmas. I worked them out mentally for three days, lying on a sofa.

After three days, I could see them clearly enough to write down a proof. I could then write my thesis: at the pace of one page a day, seven days a week, it took me three months, and I defended it in May 1951. It appeared (in French) in the *Annals* at the end of the year.

Eilenberg had given me some good advice, as usual: "Don't try to go as far as possible. What you have is already very new."

JF: *Did you keep trying to compute more homotopy groups?*

JPS: For a while. One day, just after my thesis, I went to see Cartan and Eilenberg and told them that I had proved that the homotopy groups of spheres are effectively computable. They just laughed at me! So I decided to work on something else, and then a bit later someone published that result in the *Annals*. That is the only time Cartan laughed at me.

I went on computing the cohomology of Eilenberg–MacLane spaces with coefficients in $\mathbb{Z}/2$, and from this I was able to prove that for every $n \geq 2$, there exist infinity many integers i such that the homotopy group $\pi_i(S^n)$, when finite, has even order.

JF: *So, in particular, it is non-zero!*

JPS: My interest in this kind of statement was sparked by a conversation with Chevalley. He told me the value of $\pi_i(S^3)$ for small values of i, which he had learnt from a lecture of J. C. Moore. I told him that I already knew them; I wanted to know more. He replied, "The other ones are zero."[3]

My little grey cells were outraged. "What? It's simply not possible!" ["C'est pas Dieu possible!"] So I proved the non-vanishing theorem quoted above out of indignation, an efficient technique. The proof made me use for the first time analytic number theory style arguments.

Cartan

JF: *How were your relations with Cartan?*

JPS: There is a video in which Broué's daughter asks me, "What is Cartan to you?" I look at Cartan and say, "That's a little difficult to explain." He agrees: "Yes, it's a little difficult to explain." After that, we both laugh and I say: "At the École Normale, and in Sorbonne, I had professors who were not very good, and then Cartan came, and that was something else!" In the second volume of my collected works, I included a photograph of the two of us laughing like old accomplices.

We were like family: it was clear that I was a kind of son. After his death, I said in front of Cartan's family that "he was severe with mistakes, but not with the people who made them," and they all jumped because, in their experience, that was not true. I have no recollection of Cartan asking me to revise one of the texts I wrote for his seminar, whereas others had to produce two or three successive versions. He always accepted mine immediately. It seems I had the style he wanted.

Once he told me: "You see, Serre, in my day, you did your military service, wrote your thesis, got married, and had children. And you've done everything in the opposite order!" It was not quite true, because my daughter was conceived after I got married, but the rest was indeed in the opposite order.[4]

Questions and conjectures

JF: *How do you tell a question from a conjecture?*

JPS: In principle, it's easy. A conjecture means that you think the answer is yes. A question means that you are not ready to commit yourself. For example, it used to annoy me that people called it

[2] With the exceptions of $\pi_n(S^n) = \mathbb{Z}$ and $\pi_{2n-1}(S^n)$ for even n, which is a direct sum of $\mathbb{Z}$ and a finite group.

[3] Of course, Moore had not said that; Chevalley had misunderstood.

[4] And, because I had a child, I avoided doing my military service.

Figure 7. Serre in Neuchâtel during an interview in April 2021 about his book *Rational Points on Curves over Finite Fields*.

a conjecture – what later became a theorem – that projective modules of finite type over $k[x_1, \ldots, x_n]$ are free, because in FAC it was written explicitly: "It is not known whether …"

Once somebody published a paper in the *Annals* entitled *A counterexample to a conjecture of Serre,* a conjecture that I had never made. There again, I had written "It is not known whether …"

Later, Ekedahl and I published a note in *Comptes Rendus* about curves whose Jacobian is isogenous to a product of elliptic curves. We worked out a whole series of examples, some of pretty large genus, but we did not want to make conjectures. So instead we posed two questions: whether there are such examples for every genus g, or whether, on the contrary, only finitely many values of g are possible.

JF: *You told me that you made your Conjectures I and II on Galois cohomology of algebraic groups "with a beginner's ignorance, but a taste for daring claims."*

JPS: I was very ignorant of algebraic groups at the time, but it all seemed so beautiful to me. As a former topologist I knew that $\pi_{n+1}(BG)$ is isomorphic to $\pi_n(G)$, so I was pleased with the idea of associating cohomological dimension 1 with connected groups ($\pi_0(G) = 0$) and cohomological dimension 2 with simply connected groups ($\pi_1(G) = 0$). I remember that Martin Kneser was skeptical about Conjecture II because he knew it was already difficult for classical groups over totally imaginary number fields. I didn't really know what had already been done and I didn't have any obvious counterexamples. I think I would not have made such a conjecture later on. I just got lucky.

JF: *Do you still think Conjecture II is true?*

JPS: Hard to say. I probably just don't think anything. In general, when I put forward a conjecture, I stop thinking about it. I consider that I've done my job, and then people can have fun with it. I'm not going to start solving my own conjectures …

JF: *Is there any of your conjectures where you regret not having been more daring?*

JPS: I always try to make the most daring conjectures, and sometimes they were too daring. Here's an example. In GAGA I had noticed that something was true for SL_n and symplectic groups. What do they have in common? They are simply connected! So I made a conjecture, and Grothendieck soon after proved that, on the contrary, it was true essentially only in those two cases. The conjecture was as false as possible.

JF: *Do you mind when one of your conjectures is false?*

JPS: No. There are people who take such things to heart, but I don't. I am delighted when a question is solved; be it with a yes or a no. I am surprised by people who think that it would be a catastrophe if the Hodge conjecture were false. It would be great progress! The important thing is to know.

It also irritates me when mathematicians write "unfortunately." Or that they "hope." It is absurd. When they are told that their hope is false, do they become hopeless?

It's not that things leave me indifferent, but counterexamples bring a lot of new information (think, for instance, of the Milnor–Kervaire constructions of spheres with non-standard differentiable structures – or with no such structure at all).

Counterexamples

JF: *What is your favourite counterexample?*

JPS: I like my example of a smooth projective variety over a field k of characteristic p that cannot be lifted to characteristic zero. It was a counterexample to Weil's idea that one could always lift. He and Zariski thought they had a proof using the fact that Chow coordinates are defined by polynomial equations with coefficients in $\mathbb{Z}$ – which is correct but irrelevant.

When I told Weil, he wrote to me, "You have proved the Riemann hypothesis." That was his favourite way of saying that my argument was wrong, because from a false statement, anything follows. Later, he agreed that I had not "proved the Riemann hypothesis."

I was not completely satisfied with this counterexample. I wanted an example that is not liftable at all, meaning that if the variety lifts to a local ring A, then $pA = 0$. I was happy to construct such an example a few years ago, in a letter to Illusie that appears in the fifth volume of my collected works. One curious thing is that I need to assume that $k \neq \mathbb{F}_p$.

JF: *This "fifth and final volume," as you call it in the preface, has just appeared. What did you put in it?*

JPS: My favourite papers in this volume are:

(1) the paper on finite subgroups of $G(k)$, where I generalize Minkowski's bounds on the order of finite subgroups of $\mathrm{GL}_n(\mathbb{Q})$ to arbitrary reductive groups G over arbitrary fields k;

(2) the paper on self-dual normal bases of Galois extensions in characteristic 2, with the surprising feature that the existence of such a basis depends only on the Galois group;

(3) the Bourbaki seminar on complete reducibility, a subject I greatly enjoyed because of the simplicity of its statements once they are translated into the language of Tits buildings;

(4) the notes from a series of lectures on cohomological invariants (UCLA, 2001), written with the help of Skip Garibaldi.

There are other topics I enjoyed writing about. For instance, *How to use finite fields for problems concerning infinite fields*. The rest may be less interesting, but Weil once told me that Collected Papers should include everything the author wrote.[5] One never knows what may interest a future reader. I regret that I did not always follow his advice. In the first four volumes of my *Collected Works*, I left out most of my joint papers with Borel, and now it looks as though I never worked on semisimple groups!

Collaboration

JF: *Did you enjoy collaborating?*

JPS: I prefer writing alone, but I learnt a lot from collaborating, especially with Armand Borel. For our first paper together, on the subgroups of compact Lie groups, we worked Bourbaki style: one of us wrote a draft, and the other rewrote it entirely. But later on, we became lazy and used the dangerous[6] method of writing one section each and sticking them together.

With Hochschild, I used another Bourbaki method: reading our paper aloud one line by line. Yet we still missed an error in one of the theorems.

JF: *And besides writing?*

JPS: I like discussing mathematics, but I don't have many memories of proving something with other people. Here's an exception. I remember working with Borel on Sunday, 25 June 1950. We were in the hotel room in the Latin Quarter where my wife and I were living at the time. She came back at the moment we were finishing writing a note with the proof that you cannot fibre $\mathbb{R}^n$ by compact fibres not reduced to a point, and told us that the Korean War had started. We replied, "Oh really?," and simply went on with our work. That was my training in spectral sequences. We found the theorem on a Sunday, Cartan's father brought it to the Académie des sciences on Monday, and by Wednesday we had the galley proofs.

JF: *A few years later you also proved a theorem with Cartan during a Bourbaki meeting.*

JPS: It was about the finiteness of the cohomology groups of a compact analytic variety with coefficients in a coherent sheaf. I needed this for what I wanted to do with projective space. Cartan and I reduced the proof to a statement about perturbations of compact operators on a Fréchet space. It was clearly in Schwartz's expertise. We told him, "You prove that." Schwartz proved it. We told him, "You publish it." Schwartz said, "No, but I'm happy if you publish it." He gave us a draft of the proof; we wrote it as a note in *Comptes Rendus*, which was published under his name.

JF: *Among your contributions, do you have the feeling that some of them were less inevitable than others, that people might not have found them without you?*

JPS: In mathematics, one feels that everything would eventually have been done. It's rather the way things happen that is sometimes surprising. With the homotopy groups of spheres, the surprising ingredient was the loop space fibration. No such surprise in FAC, nor in GAGA.[7]

FAC and GAGA

JF: *That's precisely a situation where the articles were not written in the order one imagines, right?*

JPS: I proved first that a coherent sheaf on complex projective space is given by a module over a polynomial algebra, and found the arguments of GAGA. But then I thought, "In this form, this should work over an arbitrary field, provided that one has defined coherent sheaves." So I had to write FAC before GAGA.

Everything was pretty obvious, except for a difficulty concerning cohomology. Standard definitions did not apply, and I had to define cohomology using coverings. But then Igusa wrote me that he was having a seminar on my work, and he couldn't prove the exact sequence of cohomology associated with an exact sequence of sheaves in degree > 1. I found a way out, using the part up to

[5] I kept his advice in mind when I edited the works of Frobenius, Cartan, Steinberg, and Tate.

[6] As the saying goes, "In collaboration, theorems add up and errors multiply."

[7] I coined those acronyms myself. They go so well together!

degree 1 to handle the case of affine varieties, and then affine coverings for the projective case.

A year later, Godement found the method of embedding a sheaf into an injective one, which allows for a definition where the cohomology exact sequence is obvious. That's the way Grothendieck did it later.

JF: *Where did the motivation come from?*

JPS: I wanted to know the analogue of Cartan's Theorems A and B on complex projective space. There are not enough holomorphic functions, but homogeneous polynomials take their place.

JF: *You were on the front line when Cartan proved Theorems A and B.*

JPS: Back from Princeton, I arrived to Cartan's seminar at the moment when he was explaining the two Cousin problems. Surprise. Cartan tells us that we should think of these problems in terms of sheaves (the subject of the previous year), that these sheaves are sheaves of modules over the sheaf of local rings, and that these modules are coherent (a new notion, inspired by Oka's work). Then he tells us that in the next lecture he will prove that the H^1 of such a sheaf on a Stein manifold vanishes. He also tells us that perhaps this can be generalized to higher degree. I jumped up to the blackboard and said to Cartan: "Yes, you absolutely must do the case of higher degree." Bruhat was by my side and supported me. I don't remember what Cartan replied to me, but the following week he had duly proved what became Theorems A and B.

JF: *Leaving topology was a decision?*

JPS: No. I never really changed subjects; there was always continuity. I kept topology as a tool and often used some of its ideas. For instance, once I was trying to prove a theorem about the cohomology of profinite groups. One starts with a profinite group G and an open subgroup H with finite p-cohomological dimension, and the question is whether the larger group G has the same p-cohomological dimension. A necessary condition is that G contains no element of order p. Is that enough? I was trying to construct a counterexample, but each time I ran into something that did not work. After a while, I realized that I was running into a question similar to the non-existence of certain maps between the cohomology of topological spaces. That non-existence followed from the properties of the Steenrod operations. What I had to do was clear: to use these operations in the cohomology of profinite groups. When I did, I had only to prove a few lemmas, and the matter was settled: no counterexample could exist. I was exhausted. That evening my wife offered me half a bottle of champagne. That's the only theorem which got me such a reward.

Figure 8. How to use an elbow crutch (Tel Aviv, 2000).
(Photo courtesy of Jean-Pierre Serre)

JF: *Do you oscillate between trying to prove something and trying to construct counterexamples until one of them wins?*

JPS: Yes, I often do that. That's how Nagata proceeded when tackling problems in commutative algebra, where it is hard to have intuition: you try to construct counterexamples and see what prevents them from working. Also, Carleson spent two years looking for a function whose Fourier series does not converge almost everywhere to it, before proving that they do converge. After all, to prove a theorem is to prove that there do not exist counterexamples.

Weil

JF: *I feel Weil is underappreciated (in French: "il n'est pas reconnu à sa juste valeur").*

JPS: I agree. Note that the French sentence "pas reconnu à sa juste valeur" is ambiguous. It only says that two things are different: no inequality sign.

JF: *What are your memories of Weil's 1948 course on his proof of the Riemann hypothesis for curves over finite fields?*

JPS: Except for conics, which I knew well, I had never heard of algebraic geometry. I attended the course and took notes, but did not understand much.

JF: *What was his lecturing style?*

JPS: He didn't write notes the way mathematicians usually do, but rather cards, in literary style. On one card there could be just a formula, without any proof. Then at the blackboard he would reconstruct the proof, with varying degrees of success.

I remember he once gave a talk at a conference in Japan where he went to the blackboard, wrote $L(s,\chi)$, and nothing else, for an hour. He went on and on about the beautiful properties of L-functions. That was the opposite of his writings, where there often is an entire page without a single line break.

Weil's natural place in Paris would have been at the Collège de France. Sometimes I dream of what it would have been like.

JF: *Why didn't they choose him?*

JPS: It was after the war, and they chose Leray. Leray had been a prisoner of war, whereas Weil had refused to serve.

L'avenir des mathématiques

JF: *There was a time when you read and reread Weil's text* L'avenir des mathématiques*, from 1947.*

JPS: Oh yes! But I was shocked that my friends from Harvard didn't appreciate it. They did not realize at which date it was written and how prophetic it was. For instance, the sentence about studying the tower of extensions of a field obtained by extracting roots of unity of "infinitely increasing" order. That's exactly what Iwasawa did ten years later! This text is a marvel. No other mathematician wrote something at that level.

I think what Tate didn't like was the sentence, "For until now, however broad our generalizations of Gauss's results may have been, we cannot say that we have really gone beyond them." Are L-functions of representations of non-abelian groups to be regarded as a broad generalization of Gauss's results? Somehow I feel that Weil's claim from 1947 stopped being true when Langlands announced his programme, in a long letter to Weil in 1967 which circulated very quickly.

I can't believe that's a coincidence. Langlands had perhaps not read *L'avenir des mathématiques*, but he knew that Weil was the best person to understand him. If Langlands had never existed and Weil had been twenty years younger – there are a lot of "ifs" here – he might have been the one to discover the Langlands programme. Building bridges between different topics was his forte.

Figure 9. Serre and Fresán on 19 April 2026.
(Photo by Eva Bayer-Fluckiger)

JF: *In that respect, he was the opposite of Chevalley, right?*

JPS: Chevalley was in favour of working on one, and only one, topic at a time. Once he told me: "If one tries to put two different things into the same photograph, the result is blurred." A beautiful comparison, but "comparaison n'est pas raison."

For instance, he worked both on algebraic linear groups and number theory, but never on rational points of algebraic groups. He also succeeded in eliminating analytic methods from class field theory. As I said once,[8] "elles se sont bien vengées!"

Langlands programme

JF: *How did you react to the Langlands programme?*

JPS: I already knew the implications for elliptic curves over the rational numbers (the modularity conjecture). What surprised me was the generality of Langlands's ideas. For example, the principle of functoriality, that you could transfer from one group to another. I was impressed that this could be done.

It's a programme, not a collection of conjectures that can be refuted with counterexamples. And because it's a programme, with many possible variants, it keeps moving forward. One of its charms is that it can be broken down into small pieces, and from time to time people manage to prove one of them. For example, Wiles

[8] In my lecture for the Galois bicentennial, thinking of the Langlands programme. The video is available at https://www.youtube.com/watch?v=zd0iIVPB8Qo.

proved one "small piece," about elliptic curves over $\mathbb{Q}$, and that small piece was enough to solve Fermat.

The development of the Langlands programme is the most important thing I have witnessed.

JF: *Langlands wrote his letter to Weil in 1967, the same year in which Weil published his paper "…* Funktionalgleichungen*" and Grothendieck gave a series of lectures about motives.*

JPS: 1967 was a great time for number theory, Grothendieck's motives, Langlands programme, Baker's effective bounds … I once gave a lecture[9] with that title.

Motives appeared in a letter that Grothendieck sent me in 1964, but they were not made public until 1967, in a seminar he gave at the IHÉS and that was never written up.

Motives didn't surprise me because I had often explained to him that the ℓ-adic invariants associated with a morphism of algebraic varieties are related by the actions of Frobenius elements. Grothendieck took the idea much further. With his natural optimism, he decided that all morphisms between motives come from algebraic cycles, which is only reasonable assuming the Hodge conjecture.[10]

Étale cohomology

JF: *A few years after Weil published his conjectures, you made an attempt to construct a cohomology theory for algebraic varieties in positive characteristic.*

JPS: It did not work. We already had cohomologies modulo p, but we didn't want to count fixed points modulo p. As an heir to the German school, I defined modules over the Witt vectors of the base field starting from the structure sheaf. In my optimism, I thought they would be free of finite type. For curves this is true, and they give the same answer as the ℓ-adic representations, but in higher dimension there are counterexamples, already for a product of two supersingular elliptic curves.

Grothendieck talks about "Serre's tentative paper" in his lecture at the ICM. People asked me, "Didn't that bother you?" Not at all: he was right.

JF: *How was étale cohomology discovered?*

JPS: I was familiar with Weil's paper on fibre spaces, which I had presented at the Bourbaki seminar before writing FAC. Weil's fibre spaces are, by definition, locally trivial for the Zariski topology, and sometimes that works well. But if H is a connected subgroup of SL_n, the H-principal bundle $\mathrm{SL}_n \to \mathrm{SL}_n/H$ is in general not Zariski locally trivial. When H is SO_n, Lang had explained to me that the existence of a rational section would contradict well-known properties of quadratic forms over function fields. My motivation to introduce a slight variant of what became étale topology was to make these bundles locally *isotrivial*, a weaker notion than locally trivial. To view this as incentive for a new "topology" was a brilliant idea of Grothendieck.

I can still see the Darboux lecture hall where I gave my talk at the Chevalley seminar in April 1958. I had defined what I viewed as the "true" H^1, giving it that name, and Grothendieck immediately told me that this was going to give a Weil cohomology in all degrees with which one could perhaps prove the Weil conjectures.

JF: *What prevented you from going further?*

JPS: I hadn't even made the effort because I had a wrong instinct. I was a topologist, so to define H^2 I would have had to look at π_2, and that was enough to block me. But Grothendieck did not care about homotopy groups. In his mind, everything could be expressed in terms of what he called "topology."

Proof of the Weil conjectures

JF: *How did you learn about Deligne's 1974 proof of the Weil conjecture?*

JPS: I think I have already told that story in a previous interview. I was in a hospital room about to undergo surgery. I phoned Deligne and told him about the little things I had been working on. He patiently listened to me, and then he said, "You know, I think I have proved the Weil conjectures." He explained the idea, and I saw it was correct. To do something so important starting from such a simple idea, that doesn't happen very often! The next day, the Raynauds came to visit me in the hospital, and I told them the basic principle of the proof.

JF: *Later, you would even explain it to the King of Sweden …*

JPS: When Deligne received the Crafoord Prize, I was invited to speak at the award ceremony, in the presence of the King and Queen of Sweden. I was given the instructions that my remarks should last no more than five minutes, applause included (?).

I had prepared just one sentence. Before saying it, I explained that Deligne deserved the prize because he had proved a very important result and I added, tongue-in-cheek, "So I suppose

[9] At the Harvard mathematics department, on 28 April 2017, with the title *50 years ago: a great time for number theory*. The video is available at https://www.youtube.com/watch?v=hs0lRR99vPI.

[10] Hence people who do not want to assume that conjecture have to use a different definition of motives, that of Yves André.

Figure 10. Serre in Saint-Sulpice in 2020. (Photo by Eva Bayer-Fluckiger)

you expect me to explain the proof." The mathematicians in the audience smiled knowingly: of course, that was impossible. Then I uttered the sentence I had prepared: "I am going to try." I heard the audience gasp.

I went to the blackboard, drew a potato, and explained that lots of people had tried to cut it into slices, but it never worked, until Deligne realized that there could not be a nasty slice, because that would lead to a contradiction.

JF: *You also delivered the laudatio when he received an honorary doctorate in Paris in 1995.*

JPS: There I chose the style of a fairy tale and spoke of the magic garden, which everyone could admire from afar, but whose door was locked. And it was Deligne who had found the key and opened the door so that we could all enter.

Cleaning up?

JF: *In French toilets there is a sign, "Please leave this place as clean as you found it when you entered." Once you wrote to Tate that you felt the same way about mathematics. Are there parts of your work that you didn't leave clean?*

JPS: There are at least two topics that I left "dirty."

The first is the sign problem in my duality theorem for coherent cohomology on a smooth projective variety X of dimension n. The proof was carried out in two stages, separated by at least one year. I first proved, by induction on the dimension, that $H^n(X,\omega)$ is one-dimensional and that it is target of a non-degenerate pairing. This is sufficient for many applications, but not for all. For instance, it doesn't imply that automorphisms of X act trivially on $H^n(X,\omega)$. It took me about a year to prove that $H^n(X,\omega)$ actually has a canonical element. Of course, that element had to be independent of the chosen projective embedding. The main point concerns signs.[11] Unfortunately, I did not publish my proof, because I thought it would follow from Grothendieck's more general results, and now I no longer remember the method.

The second example is the properties of the Galois groups associated with abelian varieties over a number field. I gave two courses at the Collège de France in 1984–1985 and 1985–1986. There are handwritten notes of the lectures available at Numdam, and letters to Ribet, Bertrand, and Vignéras explaining with proofs the main results, but a detailed account is lacking, probably because these results are themselves incomplete.

Publishing

JF: *Have you ever had a paper rejected?*

JPS: Only once. My paper on the Euler–Poincaré distribution of a profinite group was rejected by *Inventiones*. Looking back, I agree it probably did not deserve to be published in such a good journal.

JF: *And how many times did you receive a referee report?*

JPS: Two or three times. There were no referee reports for FAC, GAGA, etc. In 1952, my paper with Hochschild on the cohomology of extensions of Lie algebras was refereed by Chevalley. He suggested that we use exterior algebra, and I refused on the grounds that everything works for infinite-dimensional Lie algebras as well. I think the next time was in 1984, when I submitted my paper on the Witt invariant of the form $\mathrm{Tr}(x^2)$ to *Commentarii*, and Kervaire found that the arguments at the end were too brief and should be expanded.

When I dedicated my paper "Galois is large" to Weil, he thanked me for "this mass celebrated in my honour" and made substantial suggestions. I considered each of them, but they would have required altering the carefully chosen order in which the material was presented. So I didn't change anything.

[11] A typical example: is the residue of the 2-form $dx\,dy/y$ at $y=0$ equal to dx or to $-dx$?

Collège de France

JF: *Where did you most enjoy doing mathematics?*

JPS: I do mathematics at home, not in an office.

As for lecturing, the places I like most are Harvard and the Collège de France. I also enjoyed the atmosphere at Princeton's Institute for Advanced Study and at the IHÉS.

JF: *You often gave the same course at Harvard and the Collège.*

JPS: Tate would write to me, "Do you want to come this year?" and it was understood that I would give a course. I usually chose something that I was going to talk about later at the Collège in more detail; it served as training. I chose the subject completely freely, of course. They were both wonderful audiences.

Only the first year at the Collège was difficult because I had a small audience, and that audience was Grothendieck, Cartan, and Cartier, and they asked me tons of questions during the lecture. When I got home, I was exhausted.

JF: *You stayed at the Collège de France for almost forty years. Were there times when it felt like a burden to have to give a course on new research every year?*

JPS: It was never a burden because I could take sabbatical years from time to time, and I did when necessary. In 1993, on the road back from climbing rocks in Fontainebleau's forest, I realized that I didn't have a topic for the following year, so I just decided: "I'm going to retire." I told my wife when I got home, then informed the Collège. I finished my course by saying, "This is my last course: not just of the year, but the very last one." When I tell this story, I like to add: "and they all threw themselves on the floor in despair." Too bad: no one did that.

JF: *Did you ever interact with other professors?*

JPS: Yes, with Jacques Tits. Not with the others. I went to listen to one of Foucault's lectures because he was an old friend from the École Normale Supérieure. The room was packed; there were lots of marginal characters sitting on the floor – it suited him very well – and he was reading from a text. So I asked Foucault, "Why do you do that?" And he replied something that shocked me: "The way I say things is more important than what I say." He did not want people to make a fuss about him using one term rather than another. I thought that was a bad sign.

Old age

JF: *In* Souvenirs d'apprentissage*, Weil recalls that old people in India, when eating chili peppers, can no longer experience the intensity of the taste they remember from their youth. Are there any areas of mathematics for which you have lost the taste you had when you were young?*

JPS: I cannot think of any. It rather goes in the other direction. Getting older made me appreciate topics of which I was ignorant (or even did not like) when I was a beginner. A good example is analytic number theory, which I now like very much – especially because of its growing intersection with other topics such as Lie groups. I also like combinatorics.

Outside of mathematics

JF: *Let's finish by talking about something else. Outside of mathematics, what interests you the most?*

JPS: What do I like most? Books, movies, sports come to my mind:

(1) Books. So many! Here are some names of books and authors I like most:

Jean Giono's books of the post-war period (*Les récits de la demi-brigade*, etc.).

That irresistible Jewish writer, Isaac Bashevis Singer.

Nabokov's *Ada* is a marvel. It even played a small role in the International Mathematical Union. I was vice-president, and the other vice-president was Faddeev. When the question arose of choosing the next president, I didn't want the job, so I campaigned for Faddeev. It was the first time a Russian had been chosen, and this was during the communist period. And why did Faddeev and I get along so well? Because the two of us loved *Ada*, and that created a bond.

I also like the Japanese: Ishiguro, Murakami …

I read Proust during my first visit to Japan for the Tokyo–Nikko conference in 1955. I stayed there for almost a month, in an environment where the only languages spoken were Japanese and English. I had brought along a volume from the Pléiade edition, and in the evening, to relax, I would read passages from it. I love the third volume, *Le Temps retrouvé*. I like the first two less. "Longtemps, je me suis couché de bonne heure." It is splendid to begin a thousand-page book with such a short sentence. Bourbaki sometimes made fun of me because I had a tendency to begin a write-up with the short sentence, "Let n be a positive integer."

From Thomas Mann, I like *Joseph and His Brothers* and *Der Erwählte*. There is also *Lotte in Weimar*, which amuses me because there is a chapter in which Thomas Mann puts himself inside Goethe's mind, half-asleep. It's a bold thing to do for a German

Figure 11. Serre with his nephew Denis and his grandniece Margaux at Fontainebleau on 16 September 2024. (Photo by Jean-Paul Crosefinte)

writer, as scandalous as if a mathematician wrote "I am going to put myself into Gauss's mind."

Among more recent books, *Le bâtard de Nazareth* by Arditi.

(2) Movies ("le septième art"). I especially like those of Studio Ghibli (*Howl's Moving Castle*, *Kiki's Delivery Service …*), the Coen brothers (*O Brother*, *Where Art Thou?*, *Fargo*, *No Country for Old Men*), Tarantino (*Pulp Fiction*, *Django Unchained*), Woody Allen (*Mighty Aphrodite*), Altman (*McCabe & Mrs. Miller*), Pabst (*Die Dreigroschenoper*), Hitchcock (*The Trouble with Harry*) …

(3) Sports. My favourite were table tennis and rock climbing. I stopped the first one when I was 70 or so; I kept rock climbing (Fontainebleau boulders, and also Chamonix training cliff "Les Gaillands") until I was about 95; I had to stop because the muscles were just gone. But I still enjoy going to Fontainebleau with friends or family, and revisiting the rocks I used to climb.

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