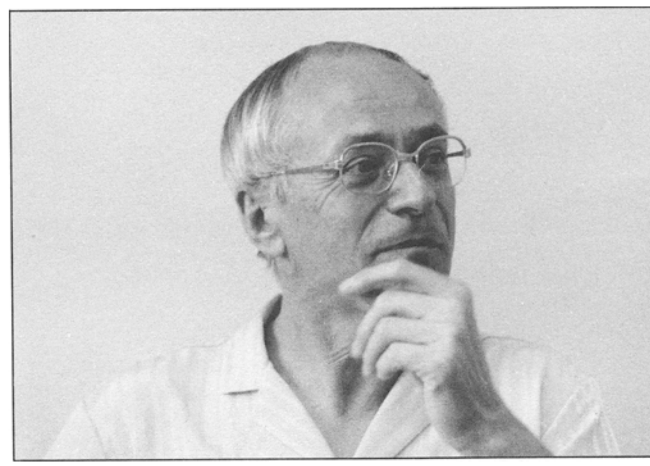
An Interview with Jean-Pierre Serre*

C. T. Chong and
Y. K. Leong

Editorial Note: *Jean-Pierre Serre was born in 1926 and studied at the Ecole Normale Supérieure in Paris. He was awarded a Fields medal in 1954 and has been Professor of Algebra and Geometry at the Collège de France since 1956.*

Professor Serre visited the Department of Mathematics, National University of Singapore, during February 1985 under the French-Singapore Academic Exchange Programme. In addition to giving several lectures organized by the Department of Mathematics and the Singapore Mathematical Society, he was also interviewed by C. T. Chong and Y. K. Leong on 14 February 1985.



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Q: *What made you take up mathematics as your career?*

A: I remember that I began to like mathematics when I was perhaps 7 or 8. In high school I used to do problems for more advanced classes. I was then in a boarding house in Nîmes, staying with children older than I was, and they used to bully me. So to pacify them, I used to do their mathematics homework. It was as good a training as any.

My mother was a pharmacist (as was my father), and she liked mathematics. When she was a pharmacy student, at the University of Montpellier, she had taken a first year course in calculus, just for fun, and passed the exam. And she had carefully kept her calculus books (by Fabry and Vogt, if I remember correctly). When I was 14 or 15, I used to look at these books, and study them. This is how I learned about derivatives, integrals, series and such (I did that in a purely formal manner—Euler's style so to speak: I did not like, and did not understand, epsilons and deltas). At that time, I had no idea one could make a living by being a mathematician. It is only later I discovered one could get paid for doing mathematics! What I thought at first was that I would become a high school teacher:

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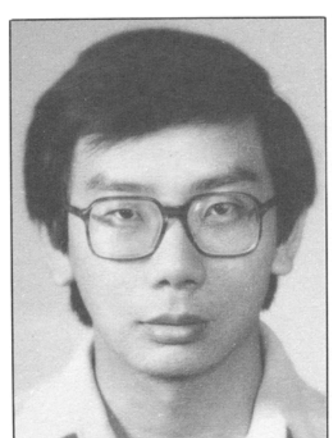

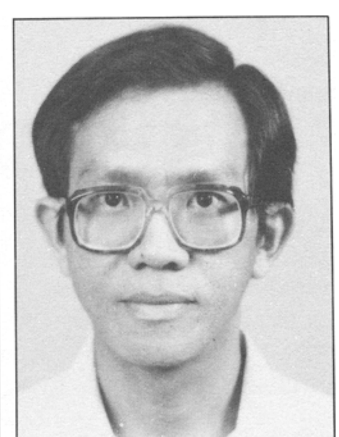

* This interview was held on 14 February 1985 in the Department of Mathematics, National University of Singapore. Reprinted with permission from the Singapore Mathematical Society. A reprint from the Mathematical Medley, Vol. 13, No. 1. (1985).

this looked natural to me. Then, when I was 19, I took the competition to enter the École Normale Supérieure, and I succeeded. Once I was at "l'Ecole," it became clear that it was not a high school teacher I wanted to be, but a research mathematician.

Q: *Did other subjects ever interest you, subjects like physics or chemistry?*

A: Physics not much, but chemistry yes. As I said, my parents were pharmacists, so they had plenty of chemical products and test tubes. I played with them a lot when I was about 15 or 16 besides doing mathematics. And I read my father's chemistry books (I still have one of them, a fascinating one, "Les Colloïdes" by Jacques Duclaux). However, when I learned more chemistry, I got disappointed by its almost mathematical aspect: there are long series of organic compounds like CH_4, C_2H_6, etc, all looking more or less the same. I thought, if you have to have series, you might as well do mathematics! So, I quit chemistry—but not entirely: I ended up marrying a chemist.

Q: *Were you influenced by any school teacher in doing mathematics?*

A: I had only one very good teacher. This was in my last year in high school (1943–1944), in Nîmes. He was nicknamed "Le Barbu": beards were rare at the time. He was very clear, and strict; he demanded that every formula and proof be written neatly. And he gave me a thorough training for the mathematics national competition called "Concours Général," where I eventually got first prize.

Speaking of Concours Général, I also tried my hand at the one in physics, the same year (1944). The problem we were asked to solve was based entirely on some physical law I was supposed to know, but did not. Fortunately, only one formula seemed to me possible for that law. I assumed it was correct, and managed to do the whole 6-hour problem on that basis. I even thought I would get a prize. Unfortunately, my formula was wrong, and I got nothing—as I deserved!

Q: *How important is inspiration in the discovery of theorems?*

A: I don't know what "inspiration" really means. Theorems, and theories, come up in funny ways. Sometimes, you are just not satisfied with existing proofs, and you look for better ones, which can be applied in different situations. A typical example for me was when I worked on the Riemann-Roch theorem (circa 1953), which I viewed as an "Euler-Poincaré" formula (I did not know then that Kodaïra-Spencer had had the same idea). My first objective was to prove it for algebraic curves—a case which was known for about a century! But I wanted a proof in a special style; and when I managed to find it, I remember it did not take me more than a minute or two to go from there to the 2-dimensional case (which had just been done by Kodaira). Six months later, the full result was established by Hirzebruch, and published in his well-known Habilitationsschrift.

Quite often, you don't really try to solve a specific question by a head-on attack. Rather you have some ideas in mind, which you feel should be useful, but you don't know exactly for what they are useful. So, you look around, and try to apply them. It's like having a bunch of keys, and trying them on several doors.

Q: *Have you ever had the experience where you found a problem to be impossible to solve, and then after putting it aside for some time, an idea suddenly occurred leading to the solution?*

A: Yes, of course this happens quite often. For instance, when I was working on homotopy groups (~1950), I convinced myself that, for a given space X, there should exist a fibre space E, with base X, which is contractible; such a space would indeed allow me (using Leray's methods) to do lots of computations on homotopy groups and Eilenberg-MacLane cohomology. But how to find it? It took me several weeks (a very long time, at the age I was then . . .) to realize that the space of "paths" on X had all the necessary properties—if only I dared call it a "fiber space," which I did. This was the starting point of the loop space method in algebraic topology; many results followed quickly.

Q: *Do you usually work on only one problem at a time or several problems at the same time?*

A: Mostly one problem at a time, but not always. And I work often at night (in half sleep), where the fact that you don't have to write anything down gives to the mind a much greater concentration, and makes changing topics easier.

Q: *In physics, there are a lot of discoveries which were made by accident, like X-rays, cosmic background radiation and so on. Did that happen to you in mathematics?*

A: A genuine accident is rare. But sometimes you get a surprise because some argument you made for one purpose happens to solve a question in a different direction; however, one can hardly call this an "accident."

Q: *What are the central problems in algebraic geometry or number theory?*

A: I can't answer that. You see, some mathematicians have clear and far ranging "programs." For instance, Grothendieck had such a program for algebraic geometry; now Langlands has one for representation theory, in relation to modular forms and arithmetic. I never had such a program, not even a small size one. I just work on things which happen to interest me at the moment. (Presently, the topic which amuses me most is counting points on algebraic curves over finite fields. It is a kind of applied mathematics: you try to use any tool in algebraic geometry and number theory that you know of . . . and you don't quite succeed!)

Q: *What would you consider to be the greatest developments in algebraic geometry or number theory within the past five years?*

A: This is easier to answer. Faltings' proof of the Mordell conjecture, and of the Tate conjecture, is the first thing which comes to mind. I would also mention Gross-Zagier's work on the class number problem for quadratic fields (based on a previous theorem of Goldfeld), and Mazur-Wiles' theorem on Iwasawa's theory, using modular curves. (The applications of modular curves and modular functions to number theory are especially exciting: you use GL_2 to study GL_1, so to speak! There is clearly a lot more to come from that direction . . . may be even a proof of the Riemann Hypothesis some day?)

Q: *Some scientists have done fundamental work in one field and then quickly moved on to another field. You worked for three years in topology, then took up something else. How did this happen?*

A: It was a continuous path, not a discrete change. In 1952, after my thesis on homotopy groups, I went to Princeton, where I lectured on it (and on its continuation: "C-theory"), and attended the celebrated Artin-Tate seminar on class field theory.

Then, I returned to Paris, where the Cartan seminar was discussing functions of several complex variables, and Stein manifolds. It turned out that the recent results of Cartan-Oka could be expressed much more efficiently (and proved in a simpler way) using cohomology and sheaves. This was quite exciting, and I worked for a short while on that topic, making applications of Cartan theory to Stein manifolds. However, a very interesting part of several complex variables is the study of projective varieties (as opposed to affine ones—which are somewhat pathological for a geometer); so, I began working on these complex projective varieties, using sheaves: that's how I came to the circle of ideas around Riemann-Roch, in 1953. But projective varieties are algebraic (Chow's theorem), and it is a bit unnatural to study these algebraic objects using analytic functions, which may well have lots of essential singularities. Clearly, rational functions should be enough—and indeed they are. This made me go (around 1954) into "abstract" algebraic geometry, over any algebraically closed field. But why assume the field is algebraically closed? Finite fields are more exciting, with Weil conjectures and such. And from there to number fields it is a natural enough transition . . . This is more or less the path I followed.

Another direction of work came from my collaboration (and friendship) with Armand Borel. He told me about Lie groups, which he knows like nobody else. The connections of these groups with topology, algebraic geometry, number theory, . . . are fascinating. Let me give you just one such example (of which I became aware about 1968):

Consider the most obvious discrete subgroup of $SL_2(\mathbf{R})$, namely $\Gamma = SL_2(\mathbf{Z})$. One can compute its "Euler-Poincaré characteristic" $\chi(\Gamma)$, which turns out to be $-1/12$ (it is not an integer: this is because Γ has torsion). Now $-1/12$ happens to be the value $\zeta(-1)$ of Riemann's zeta-function at the point $s = -1$ (a result known already to Euler). And this is not a coincidence! It extends to any totally real number field K, and can be used to study the denominator of $\zeta_K(-1)$. (Better results can be obtained by using modular forms, as was found later.) Such questions are not group theory, nor topology, nor number theory: they are just mathematics.

Q: *What are the prospects of achieving some unification of the diverse fields of mathematics?*

A: I would say that this has been achieved already. I have given above a typical example where Lie groups, number theory, etc, come together, and cannot be separated from each other. Let me give you another such example (it would be easy to add many more):

There is a beautiful theorem proved recently by S. Donaldson on four-dimensional compact differentiable manifolds. It states that the quadratic form (on H^2) of such a manifold is severely restricted; if it is positive definite, it is a sum of squares. And the crux of the proof is to construct some auxiliary manifold (a "cobordism") as the set of solutions of some partial differential equation (non linear, of course)! This is a completely new application of analysis to differential topology. And what makes it even more remarkable is that, if the differentiability assumption is dropped, the situation becomes quite different: by a theorem of M. Freedman, the H^2-quadratic form can then be almost anything.

Q: *How does one keep up with the explosion in mathematical knowledge?*

A: You don't really have to keep up. When you are interested in a specific question, you find that very little of what is being done has any relevance to you; and if something does have relevance, then you learn it much faster, since you have an application in mind. It is also a good habit to look regularly at Math. Reviews (especially the collected volumes on number theory, group theory, etc). And you learn a lot from your friends, too: it is easier to have a proof explained to you at the blackboard, than to read it.

A more serious problem is the one of the "big theorems" which are both very useful and too long to check (unless you spend on them a sizable part of your lifetime . . .). A typical example is the Feit-Thompson Theorem: groups of odd order are solvable. (Chevalley once tried to take this as the topic of a seminar, with the idea of giving a complete account of the proof. After two years, he had to give up.) What should one do with such theorems, if one has to use them? Accept them on faith? Probably. But it is not a very comfortable situation.

I am also uneasy with some topics, mainly in differential topology, where the author draws a complicated picture (in 2 dimensions), and asks you to accept it as a proof of something taking place in 5 dimensions or more. Only the experts can "see" whether such a proof is correct or not—if you can call this a proof.

Q: *What do you think will be the impact of computers on the development of mathematics?*

A: Computers have already done a lot of good in some parts of mathematics. In number theory, for instance, they are used in a variety of ways. First, of course, to suggest conjectures, or questions. But also to check general theorems on numerical examples —which helps a lot with finding possible mistakes.

They are also very useful when there is a large search to be made (for instance, if you have to check 10^6 or 10^7 cases). A notorious example is the proof of the Four Colour theorem. There is however a problem there, somewhat similar to the one with Feit-Thompson: such a proof cannot be checked by hand; you need a computer (and a very subtle program). This is not very comfortable either.

Q: *How could we encourage young people to take up mathematics, especially in the schools?*

A: I have a theory on this, which is that one should first *discourage* people from doing mathematics; there is no need for too many mathematicians. But, if after that, they still insist on doing mathematics, then one should indeed encourage them, and help them.

As for high school students, the main point is to make them understand that mathematics *exists*, that it is not dead (they have a tendency to believe that only physics, or biology, has open questions). The defect in the traditional way of teaching mathematics is that the teacher never mentions these questions. It is a pity. There are many such, for instance in number theory, that teenagers could very well understand: Fermat of course, but also Goldbach, and the existence of infinitely many primes of the form $n^2 + 1$. And one should also feel free to state theorems without proving them (for instance Dirichlet's theorem on primes in arithmetic progressions).

Q: *Would you say that the development of mathematics in the past thirty years was faster than that in the previous thirty years?*

A: I am not sure this is true. The style is different. In the 50s and 60s, the emphasis was quite often on general methods: distributions, cohomology and the like. These methods were very successful, but nowadays people work on more specific questions (often, some quite old ones: for instance the classification of algebraic curves in 3-dimensional projective space!). They *apply* the tools which were made before; this is quite nice. (And they also make new tools: microlocal analysis, supervarieties, intersection cohomology . . .).

Q: *In view of this explosion of mathematics, do you think that a beginning graduate student could absorb this large amount of mathematics in four, five, or six years and begin original work immediately after that?*

A: Why not? For a given problem, you don't need to know that much, usually—and, besides, very simple ideas will often work.

Some theories get simplified. Some just drop out of sight. For instance, in 1949, I remember I was de-

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pressed because every issue of the Annals of Mathematics would contain another paper on topology which was more difficult to understand than the previous ones. But nobody looks at these papers any more; they are forgotten (and deservedly so: I don't think they contained anything deep . . .). Forgetting is a very healthy activity.

Still, it is true that some topics need much more training than some others, because of the heavy technique which is used. Algebraic geometry is such a case; and also representation theory.

Anyway, it is not obvious that one should say "I am going to work in algebraic geometry," or anything like that. For some people, it is better to just follow seminars, read things, and ask questions to oneself; and then learn the amount of theory which is needed for these questions.

Q: *In other words, one should aim at a problem first and then learn whatever tools that are necessary for the problem.*

A: Something like that. But since I know I cannot give good advice to myself, I should not give advice to others. I don't have a ready-made technique for working.

Q: *You mentioned papers which have been forgotten. What percentage of the papers published do you think will survive?*

A: A non-zero percentage, I believe. After all, we still read with pleasure papers by Hurwitz, or Eisenstein, or even Gauss.

Q: *Do you think that you will ever be interested in the history of mathematics?*

A: I am already interested. But it is not easy; I do not have the linguistic ability in Latin or Greek, for instance. And I can see that it takes more time to write a paper on the history of mathematics than in mathematics itself. Still, history is very interesting; it puts things in the proper perspective.

Q: *Do you believe in the classification of finite simple groups?*

A: More or less—and rather more than less. I would be amused if a new sporadic group were discovered, but I am afraid this will not happen.

More seriously, this classification theorem is a splendid thing. One may now check many properties by just going through the list of all groups (typical example: the classification of n-transitive groups, for $n > 4$).

Q: *What do you think of life after the classification of finite simple groups?*

A: You are alluding to the fact that some finite group theorists were demoralized by the classification; they said (or so I was told) "there will be nothing more to do after that." I find this ridiculous. Of course there would be plenty to do! First, of course, simplifying the proof (that's what Gorenstein calls "revisionism"). But also finding applications to other parts of mathematics; for instance, there have been very curious discoveries relating the Griess-Fischer monster group to modular forms (the so-called "Moonshine").

It is just like asking whether Faltings' proof of the Mordell conjecture killed the theory of rational points on curves. No! It is merely a starting point. Many questions remain open.

(Still, it is true that sometimes a theory can be killed. A well-known example is Hilbert's fifth problem: to prove that every locally euclidean topological group is a Lie group. When I was a young topologist, that was the problem I really wanted to solve—but I could get nowhere. It was Gleason, and Montgomery-Zippin, who solved it, and their solution all but killed the problem. What else is there to find in this direction? I can only think of one question: can the group of p-adic integers act effectively on a manifold? This seems quite hard—but a solution would have no application whatsoever, as far as I can see.)

Q: *But one would assume that most problems in mathematics are like these, namely that the problems themselves may be difficult and challenging, but after their solutions they become useless. In fact there are very few problems like the Riemann Hypothesis where even before its solution, people already know many of its consequences.*

A: Yes, the Riemann Hypothesis is a very nice case: it implies lots of things (including purely numerical inequalities, for instance on discriminants of number fields). But there are other such examples: Hironaka's desingularization theorem is one; and of course also the classification of finite simple groups we discussed before.

Sometimes, it is the method used in the proof which has lots of applications: I am confident this will happen with Faltings. And sometimes, it is true, the problems are not meant to have applications; they are a kind of test on the existing theories; they force us to look further.

Q: *Do you still go back to problems in topology?*

A: No. I have not kept track of the recent techniques, and I don't know the latest computations of the homotopy groups of spheres $\pi_{n+k}(S_n)$ (I guess people have reached up to $k = 40$ or 50. I used to know them up to $k = 10$ or so.)

But I still use ideas from topology in a broad sense, such as cohomology, obstructions, Stiefel-Whitney classes, etc.

Q: *What has been the influence of Bourbaki on mathematics?*

A: A very good one. I know it is fashionable to blame Bourbaki for everything ("New Math" for instance), but this is unfair. Bourbaki is not responsible. People just misused his books; they were never meant for university teaching, even less high school teaching.

Q: *Maybe a warning sign should have been given?*

A: Such a sign was indeed given by Bourbaki: it is the séminaire Bourbaki. The séminaire is not at all formal like the books; it includes all sorts of mathematics, and even some physics. If you combine the séminaire and the books, you get a much more balanced view.

Q: *Do you see a decreasing influence of Bourbaki on mathematics?*

A: The influence is different from what it was. Forty years ago, Bourbaki had a point to make; he had to prove that an organized and systematic account of mathematics was possible. Now the point is made and Bourbaki has won. As a consequence, his books now have only technical interest; the question is just whether they give a good exposition of the topic they are on. Sometimes they do (the one on "root systems" has become the standard reference in the field); sometimes they don't (I won't give an example: it is too much a matter of taste).

Q: *Speaking of taste, can you say what kind of style (for books, or papers), you like most?*

A: Precision combined with informality! That is the ideal, just as it is for lectures. You find this happy blend in authors like Atiyah or Milnor, and a few others. But it is hard to achieve. For instance, I find many of the French (myself included) a bit too formal, and some of the Russians a bit too imprecise . . .

A further point I want to make is that papers should include more side remarks, open questions, and such. Very often, these are more interesting than the theorems actually proved. Alas, most people are afraid to admit that they don't know the answer to some question, and as a consequence they refrain from mentioning the question, even if it is a very natural one. What a pity! As for myself, I enjoy saying "I do not know."

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