

Transcript of Jean-Pierre Serre's talk, on the day of his 100th birthday.

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Around the 1950s, there were extraordinary things that had just been done. One of the things that struck me most was Gelfand's theory of normed algebras. This dictionary between commutative algebras with evolution and a small condition on the norms, very innocent in appearance, which entailed, and still entails, that this normed algebra is the algebra of continuous functions on a compact space. And formidable theorems from my youth, such as the spectral decomposition of Hermitian operators, things like that, resulted from it almost immediately. So that kind of dictionary has always appealed to me a great deal in mathematics. And there have been more and more of them, as we go forward. So, among the things that were done in that period, after 1950, let us say, there were old questions that were resolved. And that was quite striking. For example, a question that was very fashionable around 1950–1951 was Hilbert's fifth problem. Namely, you have a locally topological, locally compact group, and you assume, say, that topologically it is a manifold. There is a manifold of dimension n , no matter what n is.

And what must one prove? One must prove that there is, well, prove or give a counterexample, that there is a differentiable structure on this group, moreover unique, which is compatible with its structure of topological group, in such a way that it becomes a Lie group. And indeed, that was proved. The essential point was made by Gleason around 1951, roughly, and completed by Montgomery–Zippin. And the definitive result is fairly simple. Locally compact groups, at bottom, all come from Lie groups in the following way. First, one replaces the group, it is too big, by an open subgroup. Then it is still a topological group. And this open group will be a projective limit of Lie groups. Which means that ultimately, the tools for manufacturing locally compact groups are Lie groups, with projective limits, with inductive limits too, since one can enlarge, and then totally disconnected groups, which at the time were regarded a little as a small piece of dirt that was added on. So obviously, as soon as I did Galois theory, this small piece of dirt became the essential thing. But for the moment, at the point where we were, that was the situation.

So, this tool was very satisfactory, but one could say to oneself : “what good can it possibly do in mathematics?” of course. Elsewhere, there is hardly any hope. But in mathematics, well, it was of no use for quite a long time. And then, after more than 20 or 30 years, it turns out that Gromov needed it for his theory of nilpotent groups. And it played an essential role in Gromov's proof. An essential role, but mathematics always has surprises. Well, then, Tao showed that this essential aspect was not essential. One can indeed obtain Gromov's results without using the solution of the fifth problem. Well, that is how it is. So that is one of the remarkable things of that period. There were other Hilbert problems that were solved. But the fifth was especially attractive, in particular for me. I would very much have liked to do my thesis on that subject.

And then, things turned out otherwise. What else was there? So, among Hilbert's problems, there is the fact that for polynomial equations with integer coefficients, one cannot decide in advance

1. *I forgot to start recording at the beginning of Jean-Pierre Serre's talk. He said that he wanted to talk to us about the pleasure of mathematics, but not the pleasure he had taken in doing it, rather the pleasure he had taken in watching it progress.*

that there will be solutions. I am incapable of giving you the precise statement of that. But that was Julia Robinson and Matiyasevich who did that. For the moment, no other things so important come back to me. But still, something that was already a little old when I started was Schwartz's theory of distributions. So, this theory, well, it had an aspect, how shall I put it, a little popular. It gave meaning to the Dirac function. The Dirac function, well, it was not a function, but it was a distribution. And as a result, one could differentiate it and one could interpret layer distributions, for example, of things. But there was an aspect in the theory of distributions that, at the time, was not well understood. What do we want for distributions? We want things that extend functions. And how do we define them? Well, we do not define them as functions, but as duals of functions. So, when you have an object that plays two roles like that, on one side or the other, what is the intelligent conclusion (intelligent or not?), the conclusion you can draw from it? Well, that there is a bilinear form behind it and a pairing.

So, in the space $\mathbb{R}^n$, which was Schwartz's initial space, the $\mathbb{R}^n$ was given for the integral over the whole space, for the obvious form of the x_1 and the x_n . But if you are on two manifolds, there it becomes less obvious and even amusing. And in reality, it contains in germ practically what is called the duality theorem for certain sheaves, and even the duality theorem that is less well known than that one, the duality theorem for elliptic-type partial differential equations. There too, there is a duality, there are adjoint equations, and you have exactly as in Riemann–Roch formulas. Well, that, if something else comes back to me that strikes me a great deal, old things resolved, ah, well, old things resolved, well, obviously there is Fermat, there is no doubt, but I must say that at the time, I regarded Fermat, I still regard him somewhat in the same way now, as something rather for amateurs who amuse themselves with it and who believe they are solving it, but that such an equation has no solution, that is not... a priori, that is not fascinating. So, what happened is that, on the other hand, the proof was fascinating. That is it, and that, that is much better, at bottom. Well, so, now, how shall I group the things I want to talk to you about? Well, I will begin with a subject which, in my opinion, is not valued, as they say, at its true worth. More precisely, it is valued less than at its true worth. It is the notion of *counterexample*.

The term counterexample is a little... is a little unpleasant. One has the impression that it is something that is not good, you know. Whereas in fact, a counterexample is stated thus : “there exists such and such a thing, which has such and such a property”. That is the theorem. Simply, the property in question, people say “Ah, we did not believe that was true”. So, it is in fact a very interesting positive result. So, there have been quite a few interesting counterexamples. So, what are the counterexamples? Ah, for example, let us imagine that we are in the Hermite lecture hall. You know it. And you are, I no longer know what year. I have forgotten the year. I have forgotten the year and I am sitting in the third row, in the middle. And next to me, there is Terjanian. And Terjanian whispers in my ear while someone is speaking. It is Nicolas Bourbaki who is speaking! He whispers in my ear : “There, I have a counterexample to Artin's conjecture.”

So, which Artin conjecture? It is this one : you take the field of p -adic numbers, you fix a p , you take a homogeneous polynomial, with p -adic coefficients, of a certain degree and with a certain number of variables. And you assume that the number of variables is strictly greater than the square of the degree. So, the conjecture is that there is a zero, non-trivial, different from 0. So, Artin had made this conjecture, he had not published it, but it was at the same time that he was making the analo-

gous conjecture on finite fields, the one that was proved by Chevalley and Warning. And it became known, it was Lang, above all, who had propagandized for this conjecture. And it was known in degree 2 for a very long time, in degree 3 for not long, and in degree 4 for $\mathbf{Q}_p$ p different from 2. And it was precisely the speaker who had been given the task, perhaps it was me moreover who was in charge of the seminar, who had told him “You will expound the proof”, it was a theorem of Ax and Katz, that it works for all p large enough. And this “large enough”, it was ultimately strictly greater than 2. And then, he says to me like that at the seminar, “I have a counterexample in degree 4.” So, we wait for the talk to be over. We go to the blackboard and people gather around Terjanian and me.

And he writes for us a beautiful equation of degree 4 with integer coefficients, very small moreover, 2s and 4s. And he says to us “That has no solution.” I look and hop, I find a solution. So Terjanian says to us “Ah yes, I made a sign error.” He changes a sign. So, we look : “No solution.” And indeed, when I get home, I remember that in a half-sleep, I am going to verify completely that it was not difficult at all, that there was no solution. So, that is the kind of counterexample that, in a way, stops a subject. One cannot say that it had many consequences because people abandoned the idea of looking for variants of that. It was not very fascinating. There are other conjectures. Well, I am still going to talk about conjectures that do not have many consequences but that represent rather very difficult things.

Enflo’s work on Banach spaces and on the existence of bizarre things in certain Banach spaces with operators that have no stable sovereignty or things like that. There were two great counterexamples by Enflo, one answering a question of Grothendieck, on approximation by compact operators, but those were extremely difficult proofs. Bombieri had worked relentlessly to try to understand and simplify Enflo’s proof a little, and it took him time, and yet, he is quick, Bombieri.

So, what other examples ? Ah, here is another curious example. And you will first think that it is not a counterexample. It is Carlson who proves that every periodic L^2 function is almost everywhere the sum of its Fourier series. So, these are kinds of statements where, when one is a little innocent, one says to oneself “Ah well yes, of course, it is the Fourier expansion of a periodic function, well, period.” Then, when one is a little less innocent, one says to oneself “Ah, the devil ! Yes !” but one proves that in the space L^2 . And that is not the same.”

And people had tried for a long time to prove that. And there had been a counterexample, I believe, for functions that are simply of summable absolute value, L^1 instead of L^2 . And Carlson recounts that for two years, he tried to make a counterexample. And then, after two years, by dint of running into the difficulty of making a counterexample, he made a proof. So that is also something for which I would tend to propagandize. The fact of seeking sometimes a counterexample, sometimes a proof for a subject. There is no reason to believe in conjectures, whether Hodge’s, or anyone’s, or Grothendieck’s... or mine. It does not matter. They are not proved. And I myself have made conjectures in things far easier than Enflo’s. I also have the experience of having spent almost a whole day trying to construct a counterexample and then having ended up understanding why it did not work, and by understanding why it did not work, making a proof.

And I know that Nagata, the great expert in commutative algebra, recounts that for all the questions of commutative algebra he studied, he always tried both points of view. It must be said that in

commutative algebra, you lack a little intuition about what must be true or false, stories of chains of prime ideals that do this or that. In any case, Nagata did that and being Nagata, in general, one of the two worked, reasonably, rarely both at once! So those are fairly simple cases, but that will nevertheless prompt me to tell you that I am sometimes shocked by what colleagues say or write. For example, Grothendieck, even, who one day, I believe, wrote to me : “For me, the Hodge conjecture and the Tate conjecture are, I no longer know, axioms, facts.”, they are true, you know! And that, that... That shocks me, that shocks me a great deal. In my opinion, they are not true, provisionally.

So, there are times when it is even much better. It is much better because the counterexample opens up a completely different domain. So there, there is a name that imposes itself, I am going to write it (we will see if this pen works), the name of Milnor. So, he provided a counterexample to what? In fact, it is not really a conjecture about something because no one had quite dared to say that if one has a topological manifold, there is a single differentiable structure on it up to homeomorphism, well, there were little things like that. No one had nevertheless dared to say that, but it was believed. In any case, one had the feeling that the differentiable structure on a manifold is a little, a little extra bit that one puts on it, without any gravity, without any importance. And then, Milnor arrives and he manufactures examples for us, and with extremely simple manifolds to write down. They were not at all the monsters that one sometimes makes in topology where one passes to the limit, where one entangles things. That is not it at all. Well-smooth manifolds, well, sphere bundles, things like that, absolutely splendid and which, as a result, change our ideas about this kind of thing. One stops seeing the differentiable structure as unimportant. It becomes decisive.

There have been lots of intermediate cases. The Kervaire–Milnor classification of structures on spheres of dimension 7, for example. Except that there are 7 of them, if I remember correctly, one must not generalize this statement. But the case that I find splendid is the case of manifolds of dimension 4. So that, those are two authors and I always have trouble finding their names. Someone will surely help me. Is it Freedman and Donaldson? Ah, bravo, thank you. Freedman and Donaldson, manifolds of dimension 4. So there, compact, connected, non-empty, simply connected. Which means that the great interest is H^2 , and on this H^2 , you have a bilinear, symmetric form, of the study for a square, which is of discriminant 1 or -1 . I believe that any quadratic form of discriminant 1 is possible. Donaldson, who is a former student of Atiyah, assumes that it is differentiable. And he shows that the only possible quadratic forms are sums of ± 1 , the stupidest sums you can make of discriminant 1. So, not only is it pretty, but in addition, it shows that there exist forms, that there exist topological manifolds of dimension 4, simply quadratic, etc., which have no differentiable structure. It suffices to make one, whose form, for example, is a form of the type of 8. So, that, really, is... So, Milnor did not stop there, because as long as he was demolishing conjectures, he continued. So, he continued and he also made counterexamples to what the Germans called the Hauptvermutung, the main conjecture in combinatorial topology, which was that, for example, on a manifold, there is a single possible combinatorial structure, up to subdivision, the notion of equivalence of this combinatorial case was, to tell the truth, rather unpleasant, to such an extent that there is a mathematician whom I shall not name, who worked in this subject and left it, because he could no longer manage to know exactly which definitions had to be used.

Well, but in any case, Milnor, he demolished that. Well, but I believe I have done enough propa-

ganda on the notion of counterexample. But I would nevertheless have added a small criticism to that. This talk should not have any criticism. I once gave a talk at Harvard entirely of criticisms, on how to write mathematics badly. So there, I want to talk to you about things, but nevertheless, it annoys me when I see people who say “Unfortunately, condition A is not equivalent to condition B”. What idiots! They know to that extent of their proof that A is not B. So by saying that, they are saying “Oh, what a pity that 0 is not equal to 1!”. I am always shocked by people who put an emotion of that kind. The emotion of knowing, yes, but the emotion of regretting something that is true, I do not understand. It is absolutely contrary to our profession. Well. So, we will stop there.

And I will continue, by taking up, now, from 1950, what are the different stages that struck me. In general, there is the fact, which goes entirely in the direction of Bourbaki, that the different branches of mathematics have come closer and closer. And with great success. For example, take the theory of Lie groups. Traditionally, they are real Lie groups. So, all right, one accepts complex Lie groups, but that is already a little different. But one realized that if one took the field of p -adics, one found groups. Well, locally, they are not very pretty. A p -adic space is full of holes. But they nevertheless have a tangible space, a Lie algebra. One can classify them. One can classify them most often, at the cost of replacing them by an open subgroup of finite index. And that was a cause of great progress, because one realized that, in Galois theory, one finds profinite groups, and sometimes, and even often, they are p -adic groups of finite dimension, Lie groups. And quite at the beginning of the theory, that served enormously. I was interested in trying to show that the Galois groups for elliptic curves are big. The first thing I said to myself was : “Ah, what is their Lie algebra?” Because the Lie algebra is the most important, at bottom, because it is invariant when one replaces the group by an open subgroup. Which means that, if you make a finite extension of the field, the technique does not change. So, quickly, Grothendieck said to me : “That is not good either. What is better is to take the algebraic envelope, the smallest algebraic group”. In practice, it came almost to the same thing, but it was the right point of view... once again!

And since I was citing Grothendieck, I might as well say that among the things that struck me, it was Grothendieck’s beginnings, with in particular his theory of nuclear spaces, of making theories of topological spaces that are at once useful, they are spaces of distributions, for example, and that behave almost like spaces of finite dimension. They have a notion of tensor product that has a whole series of good virtues. I remember having been at the Institute with Bott and having said to Bott : “Listen, the thing you are doing, if you took distributions, you would see, you do as if it were a tensor product. The cohomology will be the tensor product and so on.” That was magnificent. He left the subject, as you know, because at bottom, he had resolved interesting questions, except the one that Enflo resolved negatively.

But I have lost the thread of what I wanted to tell you. So, a surprising thing of that time was a theorem of Grothendieck, that is it. It was a Grothendieck theorem because, of course, it was his form of Riemann–Roch that struck everyone at the time, his Riemann–Roch with parameters, one had never thought of that kind of thing and then, it became standard. And for him, for him, at bottom, spaces did not exist. It was arrows that interested him, not objects. Moreover, it is a point of view. It is not totally ridiculous. That is what one must tell oneself. For example, when you teach linear algebra, so now, you teach linear algebra by saying that they are modules, whether they are free or not free. There are matrices. No, there are no matrices, they are linear maps. And

in the old days, it was, one takes $\mathbb{R}^n$ and one looks. In fact, from the point of view of categories, the old point of view with integers is very clean. The set of objects is the set of positive or zero integers. The set of arrows between 3 and 5, they are the 3×5 matrices. And moreover, Deligne, who is laughing, has manufactured magnificent categories, like the category of $GL(n)$, but n is a parameter t , or else the symmetric group S_n and n is a parameter t .

I hope he published that, I no longer remember. Did you publish that? Yes, ah, very good. Well, so, let us continue, because nevertheless, a great deal happened in those years, and in the 1950s in particular. So, the clarification of the great categories, like algebraic geometry, analytic geometry, differential geometry, all right, but in a more original way, Ah, nevertheless, what struck me most of all, so, it is really what is called the Langlands programme. There, so, it is a different level, because it is really making connections. I do not know anyone who makes such astonishing connections. The only one who comes close, for me, was André Weil. He had seen long before Cartan or others, for example, that there were fibre spaces in algebraic geometry. So, but the Langlands programme, and what was beautiful, what was beautiful, what is still beautiful in the Langlands programme, is that it contains quantities of small particular cases.

And one of these small cases gave Wiles and the resolution of Fermat auxiliarily, but above all the fact that elliptic curves are modular. So, this link between the modular and the rest is extremely powerful. Moreover, our predecessors, in particular our German predecessors, were very fond of modular forms and old books, books, let us say, for example, I think, Fricke (his Algebra) or else Weber (his Algebra), two books, so, each having three volumes. And there was an entire volume on modular forms. In particular, it explicitly gave modular equations linking elliptic curves linked by isogenies of such and such a type. And that had been a little forgotten by people between 1900 and 1950.

The Japanese school, around 1950, had come back to that, no doubt under the influence of Iyanaga, who had studied in Germany and in France, moreover, I believe, too. So there, so let us see, do I have a little time? Yes, I even have a lot of time. But I am nevertheless not going to try to make a list, because there are so many things that happened in that period of the 1950s. For example, the Mordell conjecture by Faltings, and which was essentially the culmination of a series of programmes made by the Russian school and in France by Spiro. But it is Faltings who made the definitive proof. On the modular side, lots of things were also happening, because the relation between modular form and Galois representation, this aspect of group theory, of congruences, for example, when I noticed that, it was delicious! Somewhat bizarre Ramanujan formulas. Ah, well yes, there is a representation of degree 2. And then there it was... And it is Deligne who proved that it existed. I had confined myself to seeing them, that is not the same. Well, listen, I believe I am almost going to finish there, because I have not yet cited one of the things of this century that struck me a great deal. But I have reserved it for the *bonne bouche*, as they say.

That is to say the proof by the gentleman who is here, his name is Deligne, of the Weil conjectures. So, on the Weil conjectures, I have already sometimes given talks where I tried to present it... I am not going to tell you the proof, and even following the proof. It is not at all that it is difficult, because what is remarkable in this proof is that it is not difficult, modulo all that was done before. No, there is nevertheless... Well. So, there it is. In 1995, unless I am mistaken, in 1995, I believe, on

30 January 1995, we are at the École Normale Supérieure. Small room. Doctorates honoris causa are to be awarded. So, a doctorate honoris causa is awarded to a historian, and there is someone who presents the historian's work. And this someone, he tells us... Well, with my hands, I explain to you (*Serre waves his hands to show that the talk preceding his is long, but long.*)... He recounts lots of things. In principle, a little talk like that should not be long. Well, in any case, fortunately, he finishes.

And it is I who am supposed, who am charged with recounting the reasons why Deligne is awarded a Doctorate honoris causa from the École Normale Supérieure. So, I said : "Imagine...". I raise my hand, and I say : "Once upon a time...". And I take the voice of the grandfather addressing his grandchildren to tell them a fairy tale. "So, once upon a time... Once upon a time in a forest, a clearing, with a beautiful garden, brilliantly lit, with splendid flowers, but the garden was surrounded by railings. There was a door, a large armoured door, where there was no lock, but through the railings, one could see that there were very beautiful flowers, and one could guess that these flowers, if one picked them, they would grow back. It was a magic garden, it was not an everyday garden. So, obviously, I tried to enter this garden, I too, but it did not work. It was a garden that was very well protected. For example, one could try, by brute force, to break down the door. The door resisted. One could try to slip through the railing, but the bars narrowed, or lengthened. It did not work. And then, this Mr Deligne, who is here, arrived, he arrived in front of the door, he made little gestures, he took from his pocket a little of something, he did not say much. And the door opened. The door opened, and we could go in, and we could all pick flowers and all that. So, there were, behind the garden, other gardens that were also closed, but that would be for another time. But that garden...", that is what I said that day, "that garden, it is the one who is here, who opened it for us. And that is why we give the title of Doctor honoris causa to Mr Pierre Deligne.", and I stopped there.

And I stop there.