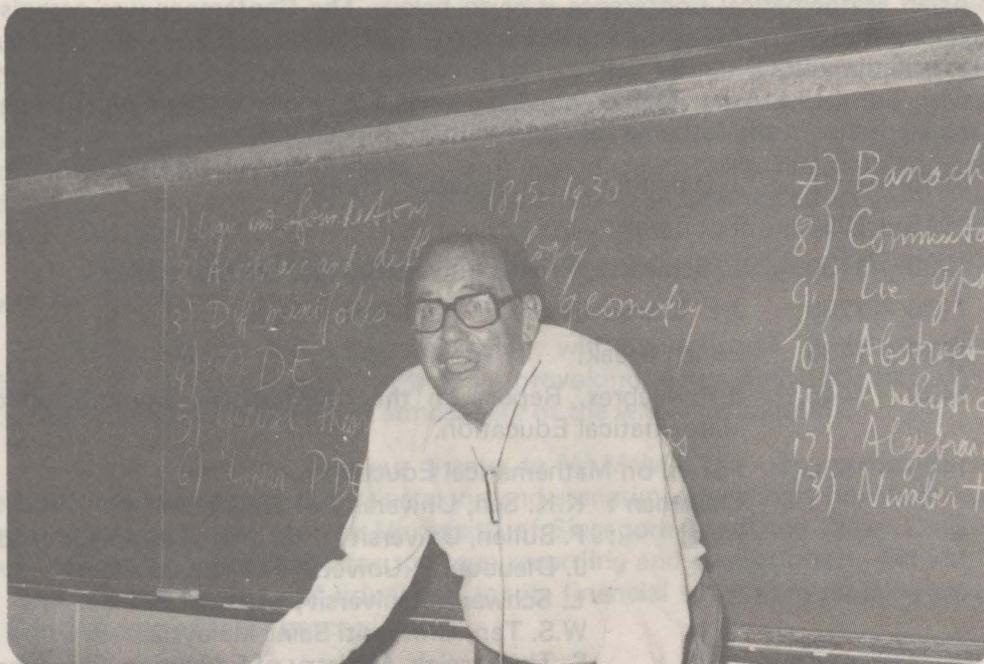


PROGRAMME OF THE SYMPOSIUM



(Courtesy L. Chen)

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## THE BOURBAKI CHOICE

J. Dieudonné

The Bourbaki group has not only undertaken to write a comprehensive treatise on a large part of mathematics but has also organised, ever since 1948, what is called the Bourbaki seminar — not to be confused with the Bourbaki book nor with the way the book is written by members of Bourbaki in seclusion and in secluded meetings where nobody else can participate except by very special favour. But the Bourbaki seminar is open to everybody and is usually attended by a large proportion of French and neighbouring mathematicians. It meets three times a year. At each session there are normally six talks on various parts of mathematics, the topics I will discuss with you later. And so these talks are quite detailed expositions on recent work in some part of mathematics. Each talk may take up 10 — 20 pages. So it is really an important exposition on some part of mathematics.

These talks are mimeographed. And now they have been published and are made available to the public by Benjamin, in number 346 I think, and since then by the Springer Lecture Notes. So every year Springer Lecture Notes publishes a book which is devoted to the Bourbaki talks given the year before. We now have approximately No. 530. So there are a little over five hundred talks which have been published and are available. In a sense I think they constitute a real encyclopedia on Mathematics or at least part of mathematics — the part which I am going to describe. The only trouble with these publications is that the order of the talks is completely random. It depends on what has happened the previous year. So the Bourbaki group will decide on two talks on number theory, maybe one on topology, one on partial differential equations, and so on. The order is completely arbitrary. If you want to pick up something about a particular subject among the 530 talks, you will be completely lost. It is almost impossible. So I was approached by a publisher two years ago to write something on Bourbaki. But I wondered what I should do until I thought that it should perhaps be a good thing for people who want to take advantage of the existence of this enormous body of mathematical literature, to write a kind of guide through the Bourbaki talks. So what I tried to do is to arrange them according to subjects, give a rapid exposition on what has been going on in that subject that is particularly interesting to the Bourbaki group, and to point out the numbers of the talks where the various parts of the subject may be found. And that is the book which has been published last year and which I called *Panorama des Mathematiques Pures* — a bird's eye view of pure mathematics, and the subtitle is *The Bourbaki choice*. Of course, I do not pretend that this book contains a description of all parts of Mathematics — only those which have been the subject of the Bourbaki talks. So it is quite necessary first to explain what is the Bourbaki choice. I think this is a purely personal point of view.

The evolution of mathematics can be described in terms borrowed from the astrophysicists when they talk about the way stars are born. If I am right (I only know this through popular exposition), what I get from the astrophysicist is roughly this. There are lumps of diffuse matter in space. Under the influence of gravitation they tend to contract, and they contract more and more until they reach a point

where the pressure and temperature is high enough for the nuclear reactions to start. Then a star is born, and it has now reached a point on the main sequence. And there it will stay for various lengths of time, sometimes billions of years, until it has exhausted its regular nuclear fuel. Then really bad things may happen about which I will not enter into, and the star goes to various stages about which I will not mention.

Now this is just a comparison which I think is quite apt for the way mathematics develops. Usually what happens is this. There are a number of problems, isolated problems, which are studied by various mathematicians at various times, and usually there is not much connection between these problems. The methods which are used are mostly ad hoc methods. When you have solved one problem there is no reason why you should be able to solve another. But then sometimes, after this period, there appears, due to some bright mathematician, an idea that there might be a method which would cover many of these problems at the same time. And so these begin to coalesce into a kind of organic body of mathematics. If things are favourable, there will even be a theory which is born and which underlies a whole group of mathematical results and problems and enables them to be solved. Furthermore these theories sometimes turn out to be able to solve problems which apparently have no connection with the initial ones. This is the point corresponding to the stars where we have the birth of a theory of a mathematical method. What happens after that? Well, the theory or method stays for a long time in what I will call, borrowing again from the astrophysicists, the mainstream of mathematics. For how long? Maybe very long — usually until it has run short of big problems. Because what happens in mathematics is, as Hilbert emphasised strongly, the life of mathematics are problems. You must have problems to solve. As long as there remains a large number of problems to solve, the theory remains alive and stays in the mainstream. And the mainstream is precisely characterised by the fact that all these theories have fantastic inter-connections with one another. They are not isolated.

What happens when a theory has run short of big problem? Well, it has the tendency to lose contact with the rest of mathematics and that may be done in different ways: either by specialising too much too particular questions which have no relation anymore with the rest of mathematics, or by diverging too much into unmotivated axiomatic extension. Let me give you an example to illustrate what I have just said. A typical example of pre-mainstream mathematics. The innumerable problems of number theory are obviously of the type in combinatorics. When you have solved a problem in number theory or combinatorics, usually you are not able to solve any other one. Look at Diophantus. Or more recently, the work of Paul Erdős, who is a master at that kind of thing and probably the most clever mathematician alive. He has been able to solve a fantastic number of problems which have no connection whatsoever with one another. So that is an example of pre-mainstream mathematics which is still pre-mainstream. What about those that leave the mainstream? Well, some of them leave the mainstream because of their concentration on too special problems. You may quote, for example, elementary geometry, analytic functions, invariant theory in the 19th century. The two last examples are rather remarkable because they show that a theory may leave the mainstream and then return again afterwards. This, I think, cannot be done by stars. Elliptic functions have again become very important nowadays and the same thing is true for

invariant theory. For a long time they were completely cut off from the rest of mathematics. In our time, theory of analytic functions of one complex variable is typical of having specialised in very special problems which have no connection with the rest of mathematics. There are other examples such as non-communative or non-associative algebra, general topology, abstract functional analysis. All these exemplify the way a theory may drift off the mainstream.

So now after I have circumscribed the kind of mathematics which I call the mainstream of mathematics, I can say what the Bourbaki choice is. The Bourbaki choice is the mainstream of mathematics. Practically all the other theories have never or extremely seldom been considered in the Bourbaki talks. It is not part of my doing. Actually, if I may speak personally, I practically never took any part in the choice of the talks in the Bourbaki seminar. I think I gave one in 20 years when I was a member of Bourbaki and, of course, I have retired a long time ago. I think I gave one or two hints once on the choice of a particular talk. The thing was not done by me at all. I am perfectly free to speak of it.

Let me give you an exact list of the topics which are considered in my book. I have in my book twenty chapters in twenty parts. In each chapter I gave a kind of diagram in which I mentioned the connections between the theory and the other parts of the book. So you have a central topic and all the other topics which are connected with it. Then there is a description of the main problems of the theory, how they have been considered, what are the Bourbaki talks corresponding to it and a supplementary bibliography for those who are interested, and finally at the end of the chapter, I gave two things — one is a list of questions in the application of mathematics which are related to the theory which I have just mentioned and the other is a list of the most prominent mathematicians who have worked in that field. This is the organisation of the book.

I am not going to give you the 20 different chapters but I will condense them into 10 topics or so which I will now list:

1. Logic and foundations;
2. Algebraic and differential topology;
3. Differential manifolds and differential geometry;
4. Ordinary differential equations;
5. General theory of partial differential equations and foliations;
6. Linear partial differential equations;
7. Banach spaces, spectral theory, Banach algebra;
8. Commutative harmonic analysis, ergodic theory, probability, potential theory;
9. Lie groups, non-commutative harmonic analysis, automorphic forms;
10. Abstract groups;
11. Analytic geometry.

Now of course, you must understand it is analytic geometry as it is used now; that is, the definition of Serre, Analytic geometry is the theory of analytic manifolds and analytic spaces. That is what was previously called the theory of analytic functions of several complex variables. Now it has become so geometric that it deserves to be called analytic geometry. The old analytic geometry, of course, does not exist — it is a bad way of doing Lie algebra. Algebraic geometry and commutative algebra

cannot be separated anymore. Commutative algebra is part of algebraic geometry and algebraic geometry can only be done by commutative algebra. And so they are organic wholes. And last but not least, number theory.

Of course, in the Bourbaki seminar, not all theories have the same density. By density I mean the number of talks compared to, say the number of papers published in a year. Some of them have very high density, such as number theory, algebraic geometry; some of them rather low — logic and foundations, probability or potential theory. In my book, I have classified them according to four layers, the last layer being essentially empty because it is the part of mathematics for which there have been no Bourbaki talks and which consists of set theory, general algebra, general topology, classical analysis, topological vector spaces, integration theory. No Bourbaki talks have ever been given on this subject. Of course, it is impossible during the time given to me to take each of these parts and describe in detail what is happening in them. So I have to make a choice of two or three topics and try to tell you what is the situation in these parts of mathematics. There are so many techniques in Lie groups and non-commutative algebra for example and also in algebraic geometry and differential topology that it is almost impossible to talk about them except by giving a four-year course. So I will now confine myself to things where it is possible to be understood without too much technical terms. Logic and foundations is perhaps the easiest one.

So what has happened to logic in the twentieth century? Well, you very well know that there was a time at the beginning of the century and the end of the nineteenth century (1895 to 1930), where there was a great movement in logic and set theory because most of the mathematicians were led to believe that it had been found that the foundations of mathematics did not rest on a very secure basis, and so a lot of mathematicians were worried by what was happening. So they took a great interest, in fact, a passionate interest, in these questions even though they were not professional logicians nor do they work in set theory. It was a very remarkable period, quite interesting, quite lively and exciting, but the situation nowadays is completely different. I am not aware that any of the brightest young mathematicians of our time has ever expressed any interest in the problems of the foundations of mathematics unless they specialised in the field. Of course, a man like Paul Cohen was certainly interested in the question. He has certainly worked in that field — in the continuum hypothesis. Why is that? It is strange that there should be a difference between the opinion of mathematicians in the beginning of this century and what they are thinking now. I think this is due to the fact that we have had fortunately in the beginning of this century an axiomatic system formulated clearly by Zermelo, Fraenkel and Skolem, which have been later on organised into a whole within a logical system, and it has served practically all mathematicians with the exception of a small group of intuitionists and constructivists. It is a perfectly satisfactory foundation for our work to such an extent that practically nobody mentions it anymore. If you take a paper on mathematics you will never see in the beginning or introduction that "I am following the Zermelo-Fraenkel system." No mathematician says that anymore; it is simply taken for granted. In other words, he has reverted to the naive set theory of pre-Cantor set theory, where mathematicians rested on a common ground of common consensus on the manipulations of mathematical objects, and it is the same thing now. I do not believe that

pure mathematicians have any fear about contradictions in that system in which they have been working for almost a century without any trouble. Furthermore, I say that the famous paradoxes of set theory have been invented, not by professional mathematicians, but by philosophers — turned-mathematicians, because they were used in the kind of arguments which no mathematician in his good sense will ever have used. So no wonder they got all sorts of fantastic and extraordinary remarks. But it was very easy to use the ZF system to put things in order and so ever since mathematicians have never to bother about that kind of thing.

This contrasts, in a sense, with what people in mathematical logic are doing. Mathematical logic is more active than it ever was. The number of papers is increasing all the time. What are they doing? On the one hand, they are exploring other logical systems such as second-order logic, model logic, many — valued logic. The trouble is that it may be very interesting for them but no mathematician as far as I know has ever found any use for these systems at all. So one would be tempted to say that logic and foundations is off the mainstream. However, this is not true because it has scored a number of spectacular successes which have kept it in close contact with the rest of mathematics. I am referring, of course, to the two famous problems of Hilbert concerned with logic, the No. 2 and No. 10, and their solutions in our time. The No. 2 is the question of independence of the axiom of choice and of the continuum hypothesis. As you know it has been proved that these propositions are undecidable within the ZF system. Furthermore, the proof by Cohen which clinched the final result rested on a new method which he called "forcing" and which has been applied by others to establish the undecidability of a lot of other open problems in set theory. So we are faced with the unforeseen situation that we may have to choose beyond the ZF system not including the axiom of choice, an infinity of different systems of axioms for set theory, without fear of running into contradiction anymore than within the ZF system itself. This disturbs some mathematicians, and nobody can guess what will happen in the future. It may very well be, as some people believe, that some day there will be a consensus among mathematicians as to which kind of axioms should be added to the ZF system to get a good kind of mathematics. Or it may well be that this will never happen and that we will have, as we have now, an infinity of possible systems above the ZF system. I should say that this does not very much disturb people interested in the Bourbaki choice because it turns out that almost never is the axiom of choice nor the continuum hypothesis used in any of their theorems. Very, very seldom does one use the generalised axiom of choice. What you use is the enumerable axiom of choice. This is essential for analysis. Beyond that, in most questions, you can dispense with the other one. And still more so for the continuum hypothesis.

The other Hilbert problem is whether there exists an algorithm which would decide in a finite number of steps whether a system of diophantine equations has or has no solutions. I wonder whether Hilbert expected a positive answer because it seems so strange to have this idea. Most mathematicians after Hilbert would probably expect the answer to be no. And actually this is what happened. In 1970 Matyasevich, a young Soviet mathematician, proved that there was no possibility of doing what Hilbert was contemplating, resting on very fundamental work previously done by Julia Robinson, Davis and Putnam. So this again is something in which logicians have taught us very valuable results in our conception of mathematics.

And finally, there is the influence which is starting right now through the use of ultra-products and non-standard analysis by logicians in introducing new ideas in mathematics. Actually one could give the definition of these things without ever mentioning logic at all. They are bona fide in mathematics. But I think it will be misleading. Because they definitely come from logic, they should be kept in contact with logic. Eventually they may rise to very powerful methods, new methods of solution for problems which are inaccessible now. Only the future will tell. So this is what one may say about the status of logic and foundations of mathematics today. It must be said in conclusion that their result are really marginal here — very interesting but completely marginal. Although many beautiful results proved by logicians are quite interesting to the non-mathematicians of these days, I would say even if mathematical logic has completely ceased to exist in 1925, no mathematician would ever missed it.

Let me try to say something about Topic No. 3: differential manifolds and differential geometry. Here the fundamental fact about differential manifolds is that the cohomology of such a manifold can be described in terms of differential forms. This is the famous de Rham's theorem which has been more or less predicted by Poincare and Élie Cartan. Very recently, Sullivan, an American mathematician, has remarked that one can extract much more information from the differential behaviour of differential forms using, of course, much more refined algebraic and topological tools, culminating in what is perhaps one of the oldest dreams of people working in differential topology, classifying compact differential manifolds. Actually Sullivan can give for a given differential manifold a system of rather complicated algebraic invariants such that for the given invariants only a finite number of diffeomorphism classes of manifolds have these invariants. A rather remarkable result. Of course, the other old problem of differential topology was yielded to powerful topological tools. For instance, John Frank Adams was able to solve a long-standing problem: how many independent vector fields are there on a sphere? A beautiful result. A little earlier, for instance, Borel and Serre proved that  $S^2$  and  $S^6$  are the only two spheres which can have almost a complete complex structure.

There has been a lot of progress made in what is called the theory of singularities. If you have two differential manifolds and a mapping  $u$  from  $M$  into  $N$  say, take a  $C^\infty$  mapping. A very old problem, which certainly goes back to the founders of differential calculus, is how to classify these mappings. When are two such mappings similar in some ways? And what has happened in the nineteenth century is that people have tried and very soon they found that the number of possibilities increase in complexity with the dimension of the manifold. So the question was practically abandoned for a long time. What has revived the theory and brought a remarkable new progress is the concept of genericity. It is a very beautiful idea. When you have to classify something in a class of objects which turns out to be so completely complicated that the whole thing seems hopeless, the idea is to single out among these objects some of them which you will call "good" and the others called "bad" in such a way that for the good objects, there is a very reasonable classification which can be handled. What about the bad? Well, you must have two things. First of all, a good classification for the good objects, a meaningful classification I would say, and then a fundamental fact that the bad ones are not too numerous. In other words, you must have the good objects forming a very large class and the bad ones

left in the lurch in the small parts. And that is what people have done, essentially by two mathematicians, Whitney and Thom. They have been able to provide a way of classifying mappings in such a way that they can say what is a generic mapping. A generic mapping is a good one. Essentially, a generic mapping is something which, when you vary everything, does not change its nature. But I cannot give any more details. And then after Whitney and Thom, other mathematicians have taken the subject and made remarkable progress, notably the American mathematician, Mather. This is one of the big areas where there is great progress: the theory of differential manifolds.

Now differential geometry is something slightly different. It is something where you put on a differential manifold additional structure. The most important, of course is the Riemannian structure. When you put a Riemannian structure on a manifold, you get a Riemannian space. In this direction, there are many important properties which link together the properties of the Riemannian structure and the topological properties on the manifold. For instance, if a compact, connected Riemannian manifold is such that the sectional curvature at every point is bigger than  $\frac{1}{4}$  then the manifold is homeomorphic to a sphere. A very remarkable result. There are plenty of things like that; for example, geodesics. Great progress has been made.

Finally, I would like to mention a third subject which is easy to understand: Abstract groups (No. 10). There is a rather curious situation here. On one hand, good progress has been made using only old tools which were available in the early 1900 to people like Frobenius, Schur and Burnside, namely combinatorial arguments, the theory of characters, the Sylow subgroups. For instance, the Russian mathematician, Novikov, has finally succeeded by a long combinatorial argument to solve the Burnside problem. If a group has a finite number of generators satisfying the condition that for a given  $n$ , all elements of the group have order at most  $n$ , is the group finite? The answer found by Novikov is that when  $n \geq 697$ , there are such groups which are infinite.

But much of the recent advances in group theory have been achieved through contact with other branches of mathematics. Group theory is far from being isolated. For instance, a discrete group in an abstract group (by an abstract group I mean a group with no other structure on it, no topology) may very well be embedded in a particular way in a semi-simple Lie group, for instance. This is very important for then it acts on an object which has a very rich structure, such as the homogeneous spaces of the group, particularly symmetric spaces when we deal with semi-simple Lie groups. This idea has very recently been cleverly exploited by Borel and Serre, using compactification of the symmetric spaces.

There are groups which do not admit such an embedding but there are substitutes for embedding, namely the so-called Tits-Bruhat buildings. These are rather complicated simplicial complexes — combinatorial objects which were extracted by Tits from the theory of Chevalley, about which I am going to say a few words, then developed by Bruhat and Tits himself in the theory of algebraic group on local field. But now they seem to pop up almost everywhere — in the theory of invariants of Mumford, in the K-theory of Quillen and in the analogy of symmetric spaces which extends to all sorts of things such as harmonic forms, special functions,

potential and so on. Although they are purely combinatorial, Serre has recently discovered that if you have a group acting on such a building you can deduce a lot of properties of the group itself. This is a very remarkable happening.

Another remarkable relationship emerged in 1965 when Chevalley discovered a general method by which to every complex Lie algebra and every field you can associate an abstract group which is finite and which is simple in the sense of abstract group theory. This remarkable thing explains coincidences, which have been observed since Jordan and Dickson, of the classical groups of Lie theory and the classical finite simple groups. And furthermore, it gave rise to an active revival of group theory, especially finite simple groups which have been more or less dormant since early 1900. First of all, you will observe that a slight variation in the method of Chevalley gives rise to new theories of finite simple groups. At the same time, by a remarkable tool which takes 270 pages long, Feit and Thompson succeeded in proving by contradiction an old conjecture of Burnside that all non-commutative simple groups have even order. In other words, a group of odd order is always soluble. This is an old conjecture and it is proved in the following way. Suppose the theorem is false. Then there is a group of smallest possible order which is odd and simple. Alright, study that group. This takes 270 pages. All possible tools of group theory are used and the final result is that such a group does not exist. This is a contradiction. So the theorem is proved. Thompson went a little further. He was able to determine all minimal simple groups, that is those which do not have non-trivial proper subgroups which are simple.

This was around 1963 and at that time, people began to be quite optimistic and thought that all simple groups have been discovered, either by Chevalley or else they are the Lie-type groups, the Chevalley groups and their refinements. Of course, one has to add a few groups which do not fit into that scheme. First of all, the alternating groups  $A_n$  which are known to be simple for  $n \geq 5$ , and then also five other curious groups which have been discovered by Mathieu in the 1860s. But that kind of optimism did not last very long. Already in 1966 a young Yugoslav mathematician named Janko discovered a new simple group which has nothing to do with the others, of order 175, 506. And in the next few years, all hell broke loose. We now have something like, in addition to the Mathieu groups, 20 or even more (the number changes every year) new simple groups. The Mathieu groups and these new groups are called sporadic groups because they cannot be classified, and the largest has order greater than  $10^{24}$ . And there is one, probably of the order of  $10^{50}$  or more, which is still in doubt, called the "monster". There is also the "little monster" which, I think, has been found.

The problem is that these groups are described by various means, and the chief problem is that they must have some properties. Some bright mathematician thinks of a situation, usually combinatorial, in which some groups have such and such properties, and then tries to deduce from these properties a number of things about the groups if they exist. For instance, very often he is able to find the table of characters. It is a big problem sometimes: imagine what the table of characters of a group of order  $10^{24}$  must be. So it is only done by computers. The only possibility for these bigger groups is to use computers to compute the table of characters and other things. And then after all sorts of such things, there may be a possibility

of proving that the group actually exists; give a definition of the group and prove that it has all the required properties. For the monster these have not been done. For the others, I think, they have already been done.

So we are in a very peculiar situation. Until recently nobody understood this situation at all. We are in the same predicament as the physicists with their hundreds of elementary particles which they do not know what to do with. We do not know anymore what to do with the sporadic groups. Now the latest news are more optimistic. A new batch of very talented young mathematicians, Americans mostly, have been coming to this field and they have brought in new ideas. There is now some hope that after 20 years, and probably after 20 papers of 300 pages each, we will finally reach the list of finite simple groups. This may be over-optimistic but the experts are quite sanguine about it nowadays.

I would like to end with a general remark. This kind of excerpt which I have given, gives you an idea of the tremendous scope of mathematics in our time. I claim that there have been more talented mathematicians, more new methods and ideas and more important problems solved in mathematics since the year 1940 than there have ever been from Greek times to 1940. And I think the records prove it conclusively.

Almost every year we have, somewhere in the world, a young mathematical genius, usually below 25, who will discover a new bright idea and push part of mathematics to unprecedented progress. So I think we have no reason at all to be doubtful about the progress of mathematics as long as our present civilisation is able to survive.