THE WORK OF NICHOLAS BOURBAKI*

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Dear President and friend,
Ladies and Gentlemen,

Thank you for your kind words on my behalf. I must admit that it is a great pleasure for me to escape from my duties as dean and spend a week in such a warm and friendly atmosphere. The tradition of friendship between French and Roumanian scientists, particularly between our mathematicians, is old. I am very happy to be a link in this chain, which I hope will continue, stronger and more cordial, in the years to come. Well, if you don't mind, I shall not make a very long speech, and I should be happy to answer at the end of it the questions which, no doubt, will be put to me. I don't pretend to deal with all the history of the works of Bourbaki, and I shall give you all a chance to ask questions on the points I merely touch upon.

To understand the origins of Bourbaki, we shall have to go back to the years that Mr. Nicolescu was recalling a few moments ago. These were the years when we were students, the years after the 1914 war; and this war, we can very well say, was extremely tragic for the French mathematicians. I shall not try to judge or give a moral assessment of what happened at that time. In the great conflict of 1914–18, the German and French governments did not see things in the same way where science was concerned. The Germans put their scholars to scientific work, to raise the potential of the army by their discoveries and by the improvement of inventions or processes, which in turn served to augment the German fighting power. The French, at least at the beginning of the war and for a year or two, felt that everybody should go to the

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It is hardly necessary to identify Prof. Dieudonné to our readers; still a few facts may prove interesting. Prof. Dieudonné studied at the Ecole Normale supérieure from 1924–1927, was a fellow at Princeton, Berlin, and Zürich, and received his doctorate in 1931. He served on the faculties at Bordeaux, Rennes, Nancy, Sao-Paulo, Michigan, Northwestern, *l'Institut des Hautes Etudes Scientifiques*, and presently is the Dean of the Faculty at Nice. He held visiting professorships at Columbia, Johns Hopkins, Rio de Janeiro, Buenos Aires, Pisa, Maryland, Tata Institute Bombay, Notre Dame, and Washington. His honors include the Order of the Legion of Honor, the Order of the Academic Palms, and membership in the Academy of Sciences. He served as President of the Mathematical Society of France in 1964–65.

Prof. Dieudonné has published a number of books and about 135 research articles on analysis, topology, spectral theory, classical groups, formal Lie groups, and non-commutative rings.

It is believed by some that the original Bourbaki members were C. Chevalley, J. Delsarte, J. Dieudonné, and A. Weil. Apparently the name "Nicolaus Bourbaki" was that of a 19th century French general. It was part of an "initiation" of first year mathematics students that an upper classman, pretending to be a famous foreign mathematician, would deliver a lecture to them in which the theorems bore the names of famous generals and were all wrong in a nontrivial way. *Editor*.

front; so the young scientists, like the rest of the French, did their duty at the front line. This showed a spirit of democracy and patriotism that we can only respect, but the result was a dreadful hecatomb of young French scientists. When we open the war-time directory of the Ecole Normale, we find enormous gaps which signify that two-thirds of the ranks were mowed down by the war. This situation had unfortunate repercussions for French mathematics. We others, too young to have been in direct contact with the war, but entering the University in the years after the war ended, should have had as our guides these young mathematicians, certain of whom we are sure would have had great futures. These were the young men who were brutally decimated and whose influence was destroyed.

Obviously, people of previous generations were left, great scholars whom we all honour and respect. Masters like Picard, Montel, Borel, Hadamard, Denjoy, Lebesgue, etc., were living and still extremely active, but these mathematicians were nearly fifty years old, if not older. There was a generation between them and us. I am not saying that they did not teach us excellent mathematics: we all took first-class courses from these mathematicians (as Mr. Nicolescu is a witness), but it is indubitable (and true for that matter of every period) that a 50-year-old mathematician knows the mathematics he learned at 20 or 30, but has only notions, often rather vague, of the mathematics of his epoch, i.e., the period of time when he is 50. It is a fact we have to accept such as it is, we cannot do anything about it.

So we had excellent professors to teach us the mathematics of let us say up to 1900, but we did not know very much about the mathematics of 1920. As I said before, the Germans went about things in a different way, so that the German mathematics school in the years following the war had a brilliance which was altogether exceptional. We only need to think of the mathematicians of the highest order who illustrated this point: C. L. Siegel, E. Noether, E. Artin, W. Krull, H. Hasse, etc., of whom we in France knew nothing. Not only this, but we also knew nothing of the rapidly developing Russian school, the brilliant Polish school, which had just been born, and many others. We knew neither the work of F. Riesz nor that of von Neumann, etc. We had been closed in on ourselves and, in our world, the theory of functions reigned supreme. The only exception was Elie Cartan; but being 20 years ahead of his time, he was understood by no one. (The first to understand him after Poincaré was Hermann Weyl, and for 10 years he was the only one, so how could we poor little students have known enough to understand him?) So, apart from E. Cartan, who at this time didn't count—he only started to count 20 years later, but since then his influence has grown steadily—we were entirely folded in on that theory of functions, which, while being important, represented only a part of mathematics.

Our only opening onto the outside world at this time was the seminar of Hadamard, a professor, but not a very brilliant teacher, at the Collège de France. (He was a great enough scholar for me to be able to say this without harming his reputation.) He had the idea (apparently taken from abroad, because this had never been done in France) of inaugurating a seminar of analysis of current mathematical work. At the beginning of the year he distributed, to all those who wanted to speak on the subject, what he judged to be the most important memoirs of the past year, and they had to explain them at the blackboard. It was a novelty for the time, and to us an extremely precious one, because there we met mathematicians of many different origins. Also, it soon became a center of attraction for foreigners; they came in crowds. (Mr. Onicescu reminded me that he himself gave lectures at the Hadamard seminar in Paris.) So it was for us young students a source of acquaintances and views that we did not find in the formal mathematics courses given at the University. This state of affairs lasted several years, until certain of us—starting with A. Weil, then C. Chevalley, having been out of France meeting Italians, Germans, Poles, etc.—realized that if we continued in this direction, France was sure to arrive at a dead end. We would no doubt continue to be very brilliant in the theory of functions, but for the rest, French mathematicians would be forgotten. This would break a two-hundred-year-old tradition in France, because from Fermat to Poincaré, the greatest of the French mathematicians had always had the reputation of being universal mathematicians, as capable in arithmetic as in algebra, or in analysis, or in geometry. So we had this warning of the bubbling of ideas that was beginning to be seen outside, and several of us had the chance to go and see and learn at first hand the development that was going on outside our walls. After Hadamard retired in 1934, the seminar was carried on, in a slightly different form, by G. Julia. This consisted of studying in a more systematic manner the great new ideas which were coming in from all directions. This is when the idea of drawing up an overall work which, no longer in the shape of a seminar, but in book form, would encompass the principal ideas of modern mathematics. From this was born the Bourbaki treatise. I must say that the collaborators of Bourbaki were very young at the time and doubtless they would never have started this job had they been older and better informed. In the first meetings for the project, the idea was that it would be finished in three years, and in this time we should draft the basic essentials of mathematics. Events and history decided differently. Little by little, as we became rather more competent and more aware, we realized the enormity of the job that had been taken on, and that there was no hope of finishing it as quickly as that.

It is true that there were already excellent monographs at the time and, in fact, the Bourbaki treatise was modelled in the beginning on the excellent algebra treatise of Van der Waerden. I have no wish to detract from his merit, but as you know, he himself says in his preface that really his treatise had several authors, including E. Noether and E. Artin, so that it was a bit of an early Bourbaki. This treatise made a great impression. I remember it—I was working on my thesis at that time; it was 1930 and I was in Berlin. I still remember the day that Van der Waerden came out on sale. My ignorance in algebra was such that nowadays I would be refused admittance to a university. I rushed to those volumes and was stupefied to see the new world which opened before me. At that time my knowledge of algebra went no further than *mathématiques spéciales*, determinants, and a little on the solvability of equations and unicursal curves. I had graduated from the Ecole Normale and I did not know what an ideal was, and only just knew what a group was! This gives you an idea of what a young French mathematician knew in 1930. So we tried to follow Van der Waerden, but in effect he only covered algebra, and even then just a small part of algebra. (Since then, algebra has developed considerably, partly because of Van der Waerden's treatise, which is still an excellent introduction. I am often asked for advice on how to start out studying algebra, and to most people I say: First read Van der Waerden, in spite of what has been done since.)

So we intended to do something of this kind. Now Van der Waerden uses very precise language and has an extremely tight organization of the development of ideas and of the different parts of the work as a whole. As this seemed to us to be the best way of setting out the book, we had to draft many things which had never before been dealt with in detail. General topology could only be found in a few memoirs and in Fréchet's book, which was, in effect, a compilation of an enormous quantity of results, without any kind of order. I can say the same of Banach's book, which is admirable for research but completely disorganized; in other subjects such as integration (as presented by Bourbaki) and certain algebra questions, there was nothing. Before the chapter of Bourbaki on multilinear algebra, I don't think there was a didactic work in the world that explained what exterior algebra was. We had to refer to the work of Grassmann, which is not particularly clear. Thus we quickly realized that we had rushed into an enterprise which was considerably more vast than we had imagined, and you know that this enterprise is still far from finished. In my briefcase I have the proofs of the 34th volume, which is devoted to three chapters of the theory of Lie groups. There are others, many others, being prepared; there are already three or four editions of preceding volumes, and the end of the work is not in sight.

We had to have a starting point—we had to know what we wanted to do. Of course, there was the idea of the Encyclopedia, which, in fact, already existed. As you know, it had been started by the Germans in 1900, and despite their proverbial tenacity and ardour for work, in 1930, after several editions and alterations, etc., it was hopelessly behind in comparison to the mathematical science of that time. Nowadays, nobody would think of starting on such an impossible enterprise, knowing the vast amount of mathematical publications released every year. I believe that we shall have to wait for the day when computers have minds and are able to assimilate all that in a few minutes. For the time being we have not progressed that far, nor had we gone that far in 1930. Moreover, it would have been useless to redo something which despite its merits had failed. The Encyclopedia, even at that period, was above all useful as a bibliographical reference, to find out where such and such a result could be found. But naturally, it contained no proofs, because if the Encyclopedia, already gigantic with its 25–30 volumes, had included proofs it would have been ten times larger. No, we did not want to produce a work of bibliographic reference, but one which would be a demonstrative mathematical text from beginning to end. And this forced us into making an extremely strict selection. What selection? Well, that is the crucial part in Bourbaki's evolution. The idea which soon became dominant is that the work had to be primarily a *tool*. It had to be something usable not only in a small part of mathematics, but also in the greatest possible number of mathematical places. So if you like, it had to concentrate on basic mathematical ideas and essential research. It had to reject completely anything secondary that had no immediately known application and that did not lead directly to conceptions of known and proved importance. There was much sifting, which started innumerable discussions among the collaborators, and which also earned Bourbaki a great deal of hostility. Because as the works of Bourbaki became known, all those who found that their favourite subject was not included were not inclined to do much propaganda in his favor. So I think that we can attribute much of the hostility that has been shown toward Bourbaki at certain periods, and which is still widespread in certain countries, to this extremely strict selection.

So how do we choose these fundamental theorems? Well, this is where a new idea came in: that of *mathematical structure*. I do not say it was an original idea of Bourbaki—there is no question of Bourbaki's containing anything original. Bourbaki does not attempt to innovate mathematics, and if a theorem is in Bourbaki, it was proved 2, 20, or 200 years ago. What Bourbaki has done is to define and generalize an idea which already was widespread for a long time. Since Hilbert and Dedekind, we have known very well that large parts of mathematics can develop logically and fruitfully from a small number of well-chosen axioms. That is to say, given the bases of a theory in an axiomatic form, we can develop the whole theory in a more comprehensible way than we could otherwise. This is what gave the general idea of the notion of mathematical structure. Let us say immediately that this notion has since been superseded by that of category and functor, which includes it under a more general and convenient form. It is certain that it will be the duty of Bourbaki, who, as I shall explain later, never fears change, to incorporate the valid ideas of this theory in his works.

Once this idea had been clarified, we had to decide which were the most important mathematical structures. Naturally, this was the root of many discussions before we found ourselves in agreement. I might say that Bourbaki does not pretend to be infallible; he has been mistaken several times about the future of structures, and apologized when it was necessary, withdrawing his original ideas. Successive editions trace some changes clearly. Bourbaki does not pretend to want to fix or nail down mathematics; that would be exactly contrary to his original purpose. But if one does not recoil from new ideas, even when they go beyond Bourbaki, one has no respect for tradition. Consequently this open systematic attitude of Bourbaki has also been a cause of hostility, this time on the part of people of previous generations, who criticized the liberties Bourbaki took with the mathematics of their time. In particular, the choice of definitions and the order in which the subjects were arranged were decided according to a logical and rational scheme. If this did not agree with what was done previously, well, it means that what was done previously had to be thrown overboard, without sparing even long-established traditions. To give you an example: Bourbaki refuses to say *non-decreasing* when referring to an increasing function because this would be a total absurdity. We know that this term means what we want to say only when talking about linear (total) order relations. (If one says non-decreasing in the setting of a non-linear order relation, this hardly means *increasing but not strictly increasing.*) So Bourbaki purely and simply abolished this terminology, as he did many others. He also invented terminology, using Greek when it was necessary, but also using many words from ordinary speech, which made traditionalists wince. They did not admit easily that what we now call *boule* or *pavé* used to be called *hypersphéroide* or *parallélotope*, and their reaction was: "This work is not to be taken seriously." A little book came out recently, which we liked very much. It is called *"Le Jargon des Sciences"* by Etiemble, vigilant guardian of the French language. He insists on preserving it in its original purity and is up in arms against the gibberish of most scientists. Happily, he makes an exception of French mathematicians, saying that they had the good sense to take simple, authentic French words from ordinary speech, sometimes changing their meaning. He cites attractive examples, recent titles such as *Platitude et privilège* and *Sur les variétés riemanniennes non suffisamment pincées*. This is the style in which Bourbaki is written—in a recognizable language and not in a jargon sprinkled with abbreviations, as in Anglo-Saxon texts where you are told about the C.F.T.C. which is related to an A.L.V. unless it is a B.S.F. or a Z.D., etc. After ten pages of this you have no idea what they are talking about. We think that ink is cheap enough to write things in full, with a well-chosen vocabulary.

I told you then that we made a selection. I shall explain this choice in more detail, using a metaphor. We realized very quickly that despite introducing the idea of structure, which was meant to clarify and separate things, mathematics refused to separate into small pieces. On the other hand, it was clear that the old divisions, Algebra, Arithmetic, Geometry, Analysis were out of date. We had no respect for them and abandoned them from the start, to the fury of many. For example, it is well known that euclidean geometry is a special case of the theory of hermitian operators in Hilbert spaces. The same goes for the theories of algebraic curves and numbers, which come essentially from the same structures. I compare the old mathematical divisions with the divisions of the ancient zoologists, who, seeing that a dolphin and a shark or a tuna-fish were similar animals, said: These are fish because they all live in the sea and have similar shapes. It was quite a while before they realized that the structures of these animals were not at all similar, and they had to be classified very differently. Algebra, Arithmetic, Geometry and all that nonsense compare easily to this. One has to look at the structure of each theory and classify it in this way. In spite of everything though, it does not take long to make one realize that despite this effort towards the isolation of structures, they have a way of mixing very quickly and extremely fruitfully. One could say that the great ideas in mathematics have come when several very different structures met. So here is my picture of mathematics now. It is a ball of wool, a tangled hank where all mathematics react one upon another in an almost unpredictable way. Unpredictable, because a year almost never passes without our finding new reactions of this kind. And then, in this ball of wool, there are a certain number of threads, coming out in all directions and not connecting up with anything else. Well, the Bourbaki method is very simple—we cut the threads. What does this mean? Let us look at what remains; then we make a list of what remains and a list of what is eliminated. What remains: The archiclassic structures (I don't speak of sets, of course), linear and multilinear algebra, a little general topology (the least possible), a little topological vector spaces (as little as possible), homological algebra, commutative algebra, non-commutative algebra, Lie groups, integration, differentiable manifolds, riemannian geometry, differential topology, harmonic analysis and its prolongations, ordinary and partial differential equations, group representation in general, and in its widest sense, analytical geometry. (Here of course I mean in the sense of Serre, the only tolerable sense. It is absolutely intolerable to use *analytical geometry* for linear algebra with coordinates, still called analytical geometry in the elementary books. Analytical geometry in this sense has never existed. There are only people who do linear algebra badly, by taking coordinates and this they call analytical geometry. Out with them! Everyone knows that analytical geometry is the theory of analytical spaces, one of the deepest and most difficult theories of all mathematics.) Algebraic geometry, its twin sister, is also included, and finally the theory of algebraic numbers.

This makes an imposing list. Let us now see what is excluded. The theory of ordinals and cardinals, universal algebra (you know very well what that is), lattices, non-associative algebra, most general topology, most of topological vector spaces, most of the group theory (finite groups), most of number theory (analytical number theory, among others). The processes of summation and everything that can be called hard analysis—trigonometrical series, interpolation, series of polynomials, etc.; there are many things here; and finally, of course, all applied mathematics.

There I wish to explain myself a little. I absolutely do not mean that in making this distinction Bourbaki makes the slightest evaluation on the ingeniousness and strength of theories catalogued in this way. I am convinced that the theory of finite groups, for example, is at the present time one of the deepest and richest in extraordinary results, while theories like non-commutative algebra are of medium difficulty. And if I had to make an evaluation I should probably say that the most ingenious mathematics is excluded from Bourbaki, the results most admired because they display the ingenuity and penetration of its discoverer.

We are not talking about classification then, the good on my right, the bad on my left—we are not playing God. I just mean that if we want to be able to give an account of modern mathematics which satisfies this idea of establishing a center from which all the rest unfolds, it is necessary to eliminate many things. In group theory, despite the extraordinary penetrating theorems which have been proved, one cannot say that we have a general method of attack. We have several of them, and one always has the impression that one is working like a craftsman, by accumulating a series of stratagems. This is not something which can be set forth by Bourbaki. Bourbaki can only and only wants to set forth theories which are rationally organized, where the methods follow naturally from the premises, and where there is hardly any room for ingenious stratagems.

So, I repeat, those which Bourbaki proposes to set forth are generally mathematical theories almost completely worn out already, at least in their foundations. This is only a question of foundations, not details. These theories have arrived at the point where they can be outlined in an entirely rational way. It is certain that group theory (and still more analytical number theory) is just a succession of contrivances, each one more extraordinary than the last, and thus extremely anti-Bourbaki. I repeat, this absolutely does not mean that it is to be looked down upon. On the contrary, a mathematician's work is shown in what he is capable of inventing, even new stratagems. You know the old story—the first time it is a stratagem, the third time a method. Well, I believe that greater merit comes to the man who invents the stratagem for the first time than to the man who realizes after three or four times that he can make a method from it. The second step is Bourbaki's aim: to gather from the diverse processes used by mathematicians whatever can be shaped into a coherent theory, logically arranged, easily set forth and easily used.

The work method used in Bourbaki is a terribly long and painful one, but is almost imposed by the project itself. In our meetings, held two or three times a year, once we have more or less agreed on the necessity of doing a book or chapter on such and such a subject (generally, we foresee a certain number of chapters for a book), the job of drafting it is put into the hands of the collaborator who wants to do it. So he writes one version of the proposed chapter or chapters from a rather vague plan. Here, generally, he is free to insert or neglect what he will, completely at his own risk and peril, as you will see. After one or two years, when the work is done, it is brought before the Bourbaki Congress, where it is read aloud, not missing a single page. Each proof is examined, point by point, and is criticized pitilessly. One has to see a Bourbaki

Congress to realize the virulence of this criticism and how it surpasses by far any outside attack. The language cannot be repeated here. The question of age does not come into it. The ages of the Bourbaki members vary considerably—later I shall tell you the maximum age limit—but even when two men have a 20-year age difference, this does not stop the younger from hauling the elder, who he feels has understood nothing of the question, over the coals. One has to know how to take it, as one should, with a smile. In any case, the reply is never late in coming, no one can boast of being infallible before Bourbaki members, and in the end, everything works out fine, despite the very long and extremely animated arguments.

Certain foreigners, invited as spectators to Bourbaki meetings, always come out with the impression that it is a gathering of madmen. They could not imagine how these people, shouting—some times three or four at the same time—about mathematics, could ever come up with something intelligent. It is perhaps a mystery but everything calms down in the end. Once the first version has been torn to pieces—reduced to nothing—we pick a second collaborator to start it all over again. This poor man knows what will happen because although he sets off following the new instructions, meanwhile the ideas of the Congress will change and next year *his* version will be torn to bits. A third man will start, and so it will go on. One would think it was an unending process, a continuous recurrence, but in fact, we stop for purely human reasons. When we have seen the same chapter come back six, seven, eight, or ten times, everybody is so sick of it that there is a unanimous vote to send it to press. This does not mean that it is perfect, and very often we realize that we were wrong, in spite of all the preliminary precautions, to start out on such and such a course. So we come up with different ideas in successive editions. But certainly the greatest difficulty is in the delivery of the first edition.

An average of 8–12 years is necessary from the first moment we set to work on a chapter to the moment it appears in the bookshop. The ones that are coming out now are the ones that were discussed for the first time about 1955.

I said earlier that there is a maximum age limit. This was recognized quite quickly for the reason I was speaking about at the start of this talk—a man of over 50 can still be a very good and extremely productive mathematician but it is rare for him to adapt to the new ideas, to the ideas of people 25 and 30 years younger than he. Now, an enterprise like Bourbaki seeks to be permanent. There is no question of saying that we nail down mathematics to such or such a period. If the mathematics set forth by Bourbaki no longer corresponds to the trends of the period, the work is useless and has to be redone. This has already happened, for that matter, with several volumes of Bourbaki. If there were elderly members of Bourbaki, they would tend to put a brake on this healthy tendency, believing that everything being fine at the time of their youth, there is no reason for change. This would be disastrous. So, to avoid tensions such as this, which sooner or later would cause Bourbaki's break-up, it was decided at the time the question arose, that all the Bourbaki collaborators retire at 50.

And it is so; the present Bourbaki collaborators are all under 50. The founder-members, of course, retired almost ten years ago, and even those who not long ago were considered young are already past—or about to reach—retiring age. So it is a question of replacing the members who leave. How do we do that? Well, there are no rules, because in Bourbaki the only formal rule is the one I have just told you, retirement at 50. Apart from this, we can say that the only rule is that there are no rules. There are no rules in the sense that there is never a vote, we have to have unanimity on every point. Each member has the right to veto any chapter he feels is bad. The veto simply signifies that we do not allow the printing of the chapter and we have to go back and re-study it. This explains the lengthiness of the process—the fact that we have such a hard time agreeing on a final version.

We are concerned then with replacing members affected by the age limit. We do not replace them formally (this would be a rule and there are no rules). There is no vacant seat, as with an academy. As most of the members of Bourbaki are professors—many in Paris—they have a chance to see at close range the young mathematicians, the youths who are just starting mathematical research. A youth of value who shows promise of a great future is quickly noticed. When this happens, he is invited to attend one of the Congresses as a guinea pig. This is the traditional method. You all know what a guinea pig is—the small animal that we use to test all the viruses. Well, it is much the same thing; the wretched young man is subjected to the ball of fire which constitutes a Bourbaki discussion. Not only must he understand, but he must also participate. If he is silent, he is simply not invited again.

He must also show a certain quality. The absence of this tendency has stopped many great and valuable mathematicians from joining Bourbaki. During a Congress, the chapters come up in the order of the day, in no particular order, and we never know in advance if we shall be doing only differential topology at this Congress, or if at the next one we shall be doing commutative algebra. No, everything is mixed—I cite the same example, the symbol that could be thought of as the Bourbaki symbol, the ball of wool. Consequently a Bourbaki member is supposed to take an interest in everything he hears. If he is a fanatical algebraist and says "I am interested in algebra and nothing else," fair enough, but he will never be a member of Bourbaki. One has to take an interest in everything at once. Not to be capable of creating in all fields, that is all right. There is no question of asking everyone to be a universal mathematician; this is reserved for a small number of geniuses. But still, one should take an interest in everything, and be able, when the time comes, to write a chapter of the treatise, even if it is not in one's speciality. This is something which has happened to practically every member, and I think most of them have found it extremely beneficial.

In any case, in my personal experience, I believe that if I had not been submitted to this obligation to draft questions I did not know a thing about, and to manage to pull through, I should never have done a quarter or even a tenth of the mathematics I have done. When one starts to write on questions one does not know and if one is a mathematician, one is forced to put questions to oneself. This is characteristic of the mathematician. Consequently one tries to solve them, and this leads to personal work, independent of Bourbaki, and more or maybe less valuable, but which was born of Bourbaki. So one cannot say that this is a bad system. But there are excellent minds which cannot adapt to this sort of obligation, profound minds which are first-class in their field, but to whom one must not mention other fields. There are unbending algebraists who will never be made to swallow analysis, and analysts for whom the field of quaternions is a monstrosity. These mathematicians may be first-class mathematicians, superior to most Bourbaki members—we admit it freely and I could give you illustrious examples—but they could never be members of Bourbaki.

To return to the guinea pig. When he is invited, we start by looking for this quality of adaptation. Often it is not there, so we wish him luck and he goes on his way. Fortunately one finds from time to time among the youths, this tendency, this appetitie for universal knowledge of mathematics and adaptation to diverse theories. After a very short time, if we find that he gives a good return, he becomes a member without any voting, election, or ceremony. Bourbaki, I repeat, has one rule, which is not to have rules, except for retirement at 50.

To end, I should like to reply to a recent attack on Bourbaki by certain young men of a certain country. Bourbaki is accused of sterilizing mathematical research. I must say that I completely fail to comprehend this, since Bourbaki has no pretension of being a work stimulating to research. I was saying earlier that Bourbaki can only allow himself to write on dead theories, things which have been definitely settled and which only need to be gleaned (except for the unexpected, of course). Actually one must never speak of anything dead in mathematics, because the day after one says it, someone takes this theory, introduces a new idea into it, and it lives again. Rather let us say theories dead at the time of writing, that is to say, nobody has made any significant discoveries in these theories Bourbaki develops for 10, 20, or 50 years, whereas they are in the part judged important and central, serving as tools for research elsewhere. But they are not necessarily stimulants for research. Bourbaki is concerned with giving references and support to anyone who wants to know the essentials in a theory. He is concerned with knowing that when one wants to work, for example, on topological vector spaces there are three or four theorems one has to know: Hahn-Banach, Banach-Steinhaus, the closed graph; it is a question of finding them somewhere. But nobody has the idea of ameliorating the theorems; they are what they are, they are extremely useful (this is the fundamental point) so they are in Bourbaki. This is the important thing. As for stimulating research, if open problems exist in an old theory, obviously they are pointed out, but this is not the aim of Bourbaki.

The aim is, I repeat, to provide worktools, not to give stimulating speeches on the open problems of the new mathematics, because these open problems are in general much farther than Bourbaki can go. This is living mathematics and Bourbaki does not touch living mathematics. He cannot when, by definition, it changes each year. If one wrote a book on that, following Bourbaki's method, i.e., taking eight or ten years to work it out, you can imagine the book after twelve years. It would represent absolutely nothing. It would have to be modified continually and would be like the old Encyclopedia, never finished.

Those are the few explanations I wanted to give you. Now I shall be very happy to answer questions, to add to what I have said.

Answers to questions.

. . . Bourbaki sets off, if you like, from a basic belief, an unprovable metaphysical belief we willingly admit. It is that mathematics is fundamentally simple and that for each mathematical question there is, among all the possible ways of dealing with it, a best way, an optimal way. We can give examples where this is true and examples where we cannot say, because up to now we have not found the optimal method.

I cited, for example, group theory and analytical number theory, which are characteristic. In both one has a quantity of methods, each one more clever than the last. This is splendid and ingenious and of a complexity never before known, but we are sure that this is not the final way to deal with the question. On the other hand, take algebraic number theory. Since Hilbert, it is so systematized that we know there is a right way to handle its questions. We change them sometimes, but in the end, little by little, we manage to find one way which is better than the others. This is only a belief, I repeat, a metaphysical belief.

. . . On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say: "Mathematics is just a combination of meaningless symbols," and then we bring out Chapters 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. This sensation is probably an illusion, but is very convenient. That is Bourbaki's attitude towards foundations.

* An address before the Roumanian Institute of Mathematics, Bucharest, Oct. 1968.