The Work of Bourbaki During the Last Thirty Years

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The Birth of Bourbaki

The history of mathematics shows that, after active periods introducing new ideas or techniques, the necessity is felt of welding the novelties into a well-organized whole, accessible to all mathematicians and giving more powerful means to help them solve their problems. The famous works of Euclid and Pappus clearly belong to that category of treatises for geometry and arithmetic; Euler performed the same task for the algebra and analysis of his time, as did Laplace for celestial mechanics and probability. A large part of Cauchy's papers may be put in that class of didactic expositions for algebra, analysis and elasticity, enriched by many original views; after him, Frobenius, around 1880, similarly acted as a legislator in the more restricted field of linear algebra; Jordan did the same for classical analysis in his *Cours d'Analyse* which remained a model for forty years, and Hilbert in his two masterpieces, the *Zahlbericht* for algebraic numbers and the famous *Grundlagen der Geometrie.*

Around 1930, it had become obvious to most research mathematicians that it was imperative to take stock and put some order in the giant strides which had occurred since 1890 in almost all parts of mathematics. Think of all the new theories born during that period: the Cantor-Zermelo theory of sets, linear representations of groups and noncommutative algebra, class field theory, general and algebraic topology, Lebesgue integral, integral equations, spectral theory and Hilbert space, Lie groups and their representations, to name only the most conspicuous ones. Of course, in many of these theories, very competent monographs were soon written, sometimes by their originators. But very often they suffered from the lack of suitable references to the background they needed from other theories; for instance, the splendid monograph of Elie Cartan on Lie groups (1932) had first to deal with basic results on topological spaces and topological groups which were hard to find in the literature of that time in the exact formulation used by the author.

The conception of Bourbaki was much more ambitious: starting from scratch, to lay the groundwork for *all* theories then in existence in pure mathematics. Applied mathematics were never considered, due primarily to the lack of competence and lack of interest of the collaborators; for some time they toyed with the idea of including probability theory and numerical analysis, but this also was soon dropped.

This ambition was kindled by the strong desire to return to the tradition of universalism, which had marked French mathematics in the previous two centuries, and of which H. Poincaré had perhaps been the most outstanding representative; but after his premature demise, followed by the bleeding to death of the young generation in World War I, French mathematics had lapsed into more and more narrow specialization and provincialism; for instance, nobody in France at that time knew anything about class field theory or the Hilbert-Riesz spectral theory, nor (with the exception of Elie Cartan, then totally isolated) about group representations or Lie theory. Bourbaki's refusal to consider mathematics as a series of isolated compartments was from the beginning one of his fundamental tendencies.

The Main Features

Starting from scratch was meant literally, not, as in all treatises or monographs then in existence (even van der Waerden's *Moderne Algebra*), merely starting from some "naive" theory of sets. It meant including the fundamental rules of logic, whilst keeping as close as possible to the actual practice of mathematicians. Among the several logical systems put forward since 1900, the one which seemed to fit best with the general conception of the treatise was the axiomatic theory of sets, as defined by Zermelo and completed by Fraenkel and Skolem to include a system of formation of correct formulas, on an entirely formalistic basis, where no meaning is attached to the symbols in use, and in particular no definition is given of the words "element" or "set."

One may perhaps insert here a small digression concerning Bourbaki's attitude towards the problem of "foundations": it can best be described as total indifference. What Bourbaki considers as important is *communication* between mathematicians; personal philosophical conceptions are irrelevant for him. Hence, with respect to the meaning one should attach to mathematics and its connection with the world of senses, Bourbaki has never tried to express publicly any opinion, leaving everybody free to hold his own views, as long as his mathematical *writings* conform to the rules of logic which are generally accepted,

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i.e. those of Zermelo-Fraenkel. And regarding the noncontradiction problems, Bourbaki refuses to join those who wring their hands over the impossibility of proving them, considering with Hadamard that noncontradiction is something which can only be ascertained at any given moment, and not proved *a priori*; if a contradiction should occur, one would simply have to eliminate or modify the part of the Zermelo-Fraenkel axioms responsible for it.

Returning to the overall conception of Bourbaki, proofs were to be given in full (no part was ever "left to the reader" or relegated to exercises), and (barring accidental mistakes, of which there were a few) with the utmost precision. This requirement now seems a quite obvious one, but around 1930 it was satisfied by papers on algebra, number theory and most of classical analysis,[1] but there was still lots of controversy over proofs in topology, and even more in algebraic geometry. The style of proofs which most influenced Bourbaki was the one exemplified by van der Waerden's *Moderne Algebra*, itself derived, as is well known, from Dedekind and Hilbert through the German school of algebra of the 1920s, with Artin, E. Noether, Krull and Hasse as principal representatives. What Bourbaki wanted to do was to describe in a similar style all the basic theorems of mathematics, and to put in evidence their mutual relationships, using a uniform terminology and uniform notations throughout, instead of treating each one separately in disjoint monographs.

The connecting link was provided by the notion of *structure*. This is not an invention of Bourbaki, who anyway disclaims ever having invented anything. But it expresses in precise terms a trend which can be traced back at least as far as Gauss, namely the growing conviction that the traditional classification of mathematics was not really adapted to the deep nature of that science. This classical point of view distinguished the various mathematical disciplines according to the mathematical objects with which they dealt: arithmetic was the science of numbers, geometry that of spatial objects, algebra the study of equations, analysis the study of functions, etc. However, it was gradually realized that arguments or results belonging to one of these "slices" of mathematics, so to speak, unexpectedly could be applied to other "slices," and that what really mattered was not the *nature* of the objects under consideration, but their mutual *relations*, although this often only came to light after deep and painstaking study; for instance, it is true but far from evident that in many respects prime numbers behave as points in the complex plane, or square integrable functions as vectors in a euclidean space.

It was also during the nineteenth century (chiefly after 1840) that progress in various parts of mathematics brought to light that elementary abstract constructs such as groups, fields, rings, vector spaces, underlay many apparently disconnected theories dealing with all kinds of mathematical objects, and that their simple general properties very often implied without effort special results previously proved by complicated *ad hoc* arguments or computations. This sometimes took quite a long time: fifty years to reach the general concept of a group, eighty years of obdurate resistance to the ideas of vector space, linear mapping and exterior algebra,[2] or the similar persistence of the horrible ways in which local coordinates or tensor calculus were commonly conceived.

The contrast between the old obsolete classification of mathematics and the new outlook based on structures may be likened to the way naturalists changed their minds about the classification of animals: at first they only took into account superficial similarities (e.g. porpoises and tuna had very similar shapes and similar reactions to their environment); later they discovered that a rational classification had to rely on deeper anatomical and physiological characteristics. And just as in so doing, they gradually discovered the wonderful unity of all living beings, so the mathematicians realized the fundamental unity of mathematics behind the very different appearance of its various parts.

In 1930 this evolution had already occurred in algebra, as was clear when one compares van der Waerden's book with previous ones. What Bourbaki did was to extend a similar outlook to *all* mathematics, with steadfast consistency, and a complete disregard of traditions when they clashed with this new conception. It is immediately apparent that there are several types of structures, the simplest ones (i.e. those using the smallest number of kinds of objects and of axioms) being algebraic structures, topological structures and structures of order (the ones which Bourbaki called "fundamental structures"). But it has been clear since Cantor that in the concept of real number these three types of structures are all present with various relations between them. Accordingly, and in defiance of tradition, Bourbaki did not hesitate to postpone the introduction of real numbers to the third book, after the general properties of the three fundamental structures had been dealt with, and even combined as in the theory of topological groups.

So much then for the frame of the treatise, its skeleton so to speak. The big question was now what substance should be molded in that frame, what flesh would cover that skeleton. In other words, from the beginning the main problem was a problem of choice. Encyclopædic ambitions were ruled out from the start, and not only for obvious material reasons. What was envisioned was a repertory of the most useful definitions and theorems (with complete proofs, as said above) which research mathematicians might need; they should be easily accessible without

[1] I strongly doubt, however, that before 1930 there was a single textbook on holomorphic functions where the Cauchy formula was correctly stated and proved, with due mention of the index of a point with respect to the path of integration. Think also on all the muddleheaded nonsense about "multiform" (or "multivalued") analytic functions.

[2] Witness the infamous book of MacDuffee, published in 1933; in my opinion the worst book ever written on algebra, in spite of its great success.

painstaking bibliographical digging, and presented with a generality suited to the widest possible range of applications, so that a mathematician would not have to adapt them to his particular problem. For instance, in their work on functional analysis, Hilbert, F. Riesz, Helly and others had to use a device which they called a "principle of choice," for which each of them had first to give a special proof adapted to the space he was considering. But it turns out that all these "principles" are special cases of a single theorem, stating that for any Banach space, the closed unit ball in its dual is weakly compact. Therefore it is *that* theorem which the worker in functional analysis should find in Bourbaki.

In other words, Bourbaki's treatise was planned as a bag of tools, a *tool kit* for the working mathematician, and this is the key word which I think everybody should keep in mind when talking about Bourbaki or discussing its plan or contents.

One of the consequences of this deliberate orientation is that even famous theorems are not included if they are essentially dead ends without foreseeable further possibilities of application. For instance, Galois theory is a wonderful tool in number theory and algebraic geometry in particular, and as such it is thoroughly treated in Bourbaki. However, Galois' criterion of resolubility by radicals (which was the goal he pursued in the first place and for which he invented the theory) is not mentioned at all, in contrast with many books on algebra, because since its discovery it has never found significant new applications; it beautifully solved an old and difficult problem, but intrinsically it was a dead end.

What I have said up to now exhibits a typical feature of Bourbaki, its austere, unrelenting, unbending and uncompromising disposition. There has been plenty of discussion between Bourbaki's collaborators during the elaboration of the various chapters, which generally extended over many years; but none ever transpired publicly, and Bourbaki has never engaged, under his name, in polemics with the "outside world," so to speak, in spite of the many criticisms to which he has been subjected. Neither has there ever been any propaganda published by him in favor of his ideas, nor even a statement of policy or purpose, with three exceptions: first, in the early years of Bourbaki, an article on "the architecture of mathematics" describing his concept of the hierarchy of structures, and another one on "foundations of mathematics for the working mathematician"[3] on the system of logical rules adopted as a basis for the treatise. The third exception is the leaflet entitled *Directions for use* inserted in every volume, which essentially is limited to a description of the material organization of the books into chapters, sections, lists of problems and their mutual dependence. I think this refusal of all discussion or controversy stems from the belief that a mathematical text has to stand or fall on its own merits, and should not need any advertising—an attitude which can be summarized as "take it or leave it."

[3]Published in the Journal of Symbolic Logic **14** (1949), 1–8.

Regarding the choices made by Bourbaki of the notions and results which he considers important, it is therefore useless to look for his official pronouncements on the matter, since they don't exist; and nobody, including myself, has ever been authorized to speak in his name. All one can do is to offer his own interpretation, and I beg you to keep in mind that what follows is purely personal.

This opinion is based on my conviction (which I believe is shared, at least implicitly, by most collaborators of Bourbaki) that mathematics is the opposite of a democracy. History shows that the really seminal ideas are due to a small number of first-rate mathematicians: certainly not more than 20 in the eighteenth century, maybe 100 in the nineteenth, many more in our time, but still a very small percentage of all professional mathematicians; it is as childish to deny such a well established fact as it would be to rebel against the force of gravity. Everyone has his own limits, and those who did not receive the gift of inventive imagination bestowed on the great creators can still bring a very useful contribution to science by elaborating the new ideas into more palatable forms, enriching them with pertinent commentaries and disseminating them among students and colleagures. If I may here inject a personal note, I would say how grateful I am that fate granted me the privilege of living and working in contact with some of the greatest mathematicians of our time, and thus being able to fulfill the role I just mentioned of spreading their ideas and discoveries.

It therefore seems to me that, when examining which tools should be included in Bourbaki, a decisive element was whether or not they had been used by great mathematicians, and what degree of importance these mathematicians had attached to these tools; opinions of other people were deliberately ignored. The mathematicians who had the deepest influence on Bourbaki were probably, in Germany, Dedekind, Hilbert, and the school of algebra and number theory of the 1920s; and in France, H. Poincaré and Elie Cartan. Although these great mathematicians are very different from one another, both in their style and in their fields of research, they have had a common philosophy of mathematics, namely to try to solve classical problems by methods involving "abstract" new concepts, and that is also, in my opinion, the central idea of Bourbaki. This means that on one hand Bourbaki is strongly in favor of applying to old problems all the power gained from the axiomatic study of structures; but on the other hand he refuses to have anything to do with the mathematicians who indulge in abstract theories with no motivation, the axiomatic trash which fills so many of our journals.

Summing up, we see that, in spite of its initial aim at universality, the scope of the Bourbaki treatise has finally been greatly reduced (although to a still respectable size) by successive elimination of:

1) the end products of theories, which do not constitute new tools;
2) the unmotivated abstract developments scorned by the great mathematicians;

3) a third restriction comes from the fact that some very active and very important theories (in the opinion of great mathematicians) still seem very far from a clear description in terms of an interplay of perspicuous structures; examples are finite groups or the analytic theory of numbers;
4) finally, there are parts of mathematics where the underlying structures are well in evidence, but in such an ebullient state, with an unending influx of powerful new ideas and methods, that any attempt at organization is doomed to almost immediate obsolescence: think of algebraic and differential topology, or algebraic geometry, or dynamical systems.

The initial plan of Bourbaki was thus limited to the three basic structures of order, "general algebra" and "general topology," to some of their combinations such as topological groups and topological vector spaces, and finally to elementary calculus and integration theory. Later, it turned out that it was also feasible to include commutative algebra, Lie groups and algebras (but not algebraic groups), and some spectral theory; and at least parts of analytic geometry are now being considered for possible inclusion.

The Reactions to Bourbaki

Let us now turn to the reactions of the mathematical world towards Bourbaki. The publication of the first volumes unfortunately was made at a most inauspicious time, since it coincided with the outbreak of World War II. So, at first, they were of course totally unnoticed, and I think it is probable that they came to the attention of mathematicians through mention made of them (mostly in footnotes) in the individual papers of collaborators of Bourbaki. As the time needed for the elaboration of each volume varied a great deal, the order of publication was very different from the logical order of the chapters and books, and it was hardly possible to have a reasonable grasp of the global organization of the treatise before the mid 1950s. Since then the number of references to Bourbaki's books has appreciably increased, mostly of course in papers written in French (although an English edition has been made of some parts of the treatise). To these should be added what I think one could call indirect quotations, namely to monographs written after the publication of Bourbaki's books, but clearly inspired by them (much as Bourbaki's algebra had been inspired by van der Waerden); specialists may well consider it easier to refer to these monographs rather than to the bulky Bourbaki.

As I said above, the choice of notations and terminology was done by Bourbaki with extreme care. In set theory he popularized the notations $\cap$, $\cup$ and $\emptyset$ for intersection, union and the empty set, which have become universally accepted; he did not have the same success for $\subset$ (a large proportion of authors still prefer $\subseteq$), and still less for $\complement$ (complementary), and pr_1 and pr_2 (projections).

Before Bourbaki, authors such as van der Waerden had used the capitals N, Z, R, C to designate the sets of natural integers, rational integers, real and complex numbers respectively. Bourbaki advocated the use of bold face characters for these notations, in order to free the usual Roman or Italic characters for other purposes, and he added to these letters Q for rational numbers and H (first proposed by Bott) for quaternions. These notations (happily completed by the "Japanese" convention: $\mathbb{N}$, $\mathbb{Z}$, etc. in manuscripts or mimeographed texts) are now accepted by a great majority. But most Anglo-Saxon writers have refused to accept F_q for finite fields and still use Dickson's $GF(q)$. Other notations endorsed by Bourbaki and now widespread are $x \otimes y$ and $x \wedge y$ for tensor and exterior products, $\langle x, y\rangle$ for bilinear forms and $\sigma(E, F)$ for weak topology.

Concerning terminology, Bourbaki's attitude, as expressed in his *Directions for use*, is to accept or at least tolerate traditional terminology, unless it is ambiguous, or ungrammatical, or incompatible with the normal use of language. It turns out that in many fields of recent origin, a number of mathematicians, mainly of Anglo-Saxon or German origin, were guilty of particularly acute carelessness in showing a total lack of imagination and a complete contempt for their languages; a typical sin was their abuse of abbreviations, against which Bourbaki reacted strongly, thinking ink and paper are cheap enough to avoid them altogether. In such cases, Bourbaki felt it a duty to introduce a new terminology; he was chiefly guided by two principles: use words as short as possible and easy to translate into the main scientific languages, and if possible find names evocative of the notions they designate or of their originators.

Take for instance Baire's unfortunate terminology of "nowhere dense sets" and "sets of first category"; Bourbaki, with limited success, tried to replace it by "rare sets" and "meager sets". Or consider Krull's book of 1935, *Idealtheorie*, with the following terminology:

- O-Ring: ring with ascending chain condition;
- U-Ring: ring with descending chain condition;
- ZPI-Ring: ring with unique decomposition of an ideal into prime ideals;
- ZPE-Ring: ring with unique decomposition of an element into prime elements.

Bourbaki replaced it by: noetherian rings, artinian rings, dedekind rings and factorial rings respectively; the first three terms have been readily accepted, but not the fourth, for no apparent particular reason except the obdurate conservatism ingrained in so many mathematicians. For other examples of that conservatism consider Bourbaki's failure to replace "division ring" by "field" ("skew field" if necessary); one will also recall the protest of S. MacLane (reviewing Bourbaki's *Algebra* in 1948) against the use of the word "algebra" when the scalars form a ring and not a field (I think since then he has changed his mind); and the refusal of Soviet mathematicians to use "compact" instead of the meaningless "bicompact" (a space cannot be twice compact!). A proposed terminology may be perfectly logical, but this is not enough to ensure its acceptance; an example is given by Bourbaki's proposal to understand "positive" in an ordered group as ≥ 0 (of course Bourbaki calls "ordered" what many persist in calling "partially ordered" in spite of the fact that the latter are much more numerous than the former); however the old terminology "nonnegative" is still in use, in spite of the fact that it is patently absurd (in the ordered group of real functions on an interval, a "nonnegative" function f would be such that $f(x) \geq 0$ for *some* x, and not $f(x) \geq 0$ for *all* x).

One of the few great successes of Bourbaki in the matter of terminology has been the introduction of the words "surjective" and "bijective", which together with the already used "injective" formed a coherent system, replacing the ungrammatical use of "onto" (which is even worse in French); this was universally adopted almost overnight. Another widely accepted terminology was the distinction between "closed balls," "open balls" and "spheres" in metric spaces, where a lot of confusion existed before 1940. But when Bourbaki tried to repeat these successes with "algebra", "cogebra" and "bigebra", he was only followed by a few people, although "bialgebra" is as absurd as "bicompact" (it does *not* imply two *algebra* structures!!).

The Evolution of Bourbaki

I shall conclude with some (again *strictly personal*) comments on the initial conceptions of Bourbaki and how they eventually were modified in the span of 30 years. Contrary to what is often said, Bourbaki's attitude is not at all rigid, and he is always willing to change his point of view whenever the evolution of mathematics justifies the inclusion of new notions, or shows that existing ones which he had neglected are more useful than he had previously thought. But he will never yield to a mere fad, even a popular one; his motto is "Wait and see."

I have already described Bourbaki's attitude towards logic and set theory, which is to include as little of it in the treatise as possible, namely just what is absolutely necessary for the proofs of what Bourbaki considers to be important theorems. Since 1950 logic has made tremendous progress, but in Bourbaki's opinion it has not brought enough new tools for mathematicians to compel a change of attitude. It is possible that in the future the present growth of non-standard analysis and similar uses of mathematical logic will yield startling discoveries, but this is not yet the case; so "Wait and see" is what prevails in this respect.

One often hears people wondering why Bourbaki has not undertaken to publish a chapter on categories and functors. I think one of the reasons is the following: the parts of mathematics where these concepts are extremely useful, such as algebraic geometry and algebraic and differential topology, are among those which Bourbaki cannot contemplate including in the treatise, for reasons mentioned above. For many other parts of mathematics, it is certainly possible to use the language of categories and functors, but they do not bring any simplification to the proofs, and even in homological algebra (treated, for modules, in a recent Bourbaki chapter), one can entirely do without their use, which would only amount to introducing extra terminology.

When Bourbaki's algebra was published, it could not, of course, do much better than van der Waerden in many parts of algebra; but two theories, which were to become basic in all mathematics, were not yet to be found in van der Waerden: duality of modules and multilinear algebra. In particular, I think it can be said that Bourbaki's treatise was the first to give a complete and useful exposition of Grassmann's exterior algebra, which was unreadable in Grassmann, and even worse in the books written by the third-rate mathematicians who had followed him. The new edition, which is not yet finished, includes more material (found necessary in many applications, in particular in commutative algebra) but no major changes, with the exception of the new chapter on homological algebra mentioned above.

The rapid and spectacular transformation of algebraic geometry after 1945 led Bourbaki to undertake the publication of a new book on commutative algebra; although it is not yet complete, it seems likely that a new edition will be necessary to take into account many new concepts and results which have been found useful in algebraic geometry.

The basic notions of general topology had been treated in the books of Hausdorff, Kuratowski and Sierpiński before the publication of Bourbaki's book on the subject. But again that book was the first in the literature to consider filters, uniform spaces, topological groups and function spaces. The main changes in the new edition were again chiefly due to the influence of algebraic geometry: a section on proper mappings was added, and an even more conspicuous change was a more thorough study of spaces which do not satisfy Hausdorff's separation axiom. In the first edition, these spaces were considered pathological, since no one knew of any part of mathematics where they naturally occurred. But after 1950 the Zariski topology became a tool of great importance in algebraic geometry, and of course it is not Hausdorff; other kinds of non-Hausdorff spaces also came to light at about the same time in spectral theory.

Bourbaki's volume on topological vector spaces was also the first textbook on locally convex spaces. It contained, of course, all the results proved in Banach's famous book, but put in a more general and more useful perspective; in addition it contained all the tools necessary for the applications of topological vector spaces to the modern theories of distributions and partial differential equations. Of course, another fundamental tool in modern functional analysis is spectral theory, and Bourbaki has undertaken to write a book on that subject, but a large part of it is still in the preparatory stage. I don't think it is also necessary to speak of Bourbaki's book on Lie theory (also not yet complete); it comes after many others, and is only notable for its thoroughness.

The final and most controversial Bourbaki book is the one on integration and measure theory. Bourbaki considers the integral as an essential tool in functional analysis, applicable in particular to continuous functions, on which it is a *linear form*; it thus appears as one member of a whole family of linear forms, the *distributions*. But of course this means that the measures linked to integrals are Radon measures defined on topological spaces. This does not meet the needs of probabilists, who use measures defined on "tribes" ("σ-rings" in the awful Anglo-Saxon terminology) of subsets of sets without any topology, and frequently they have to deal with variable tribes of subsets; in consequence they reject the Bourbaki point of view. At present the deadlock has not been resolved.

Finally, I would try to answer some criticisms directed, in fact, not at Bourbaki himself but at the use which is sometimes made of his treatise. The easiest and simplest answer is, of course, to say that an author cannot be held responsible for the fact that some people refer to his books, once they have been published, to justify theories or actions which he had neither intended nor foreseen. To be more specific, one of the things for which Bourbaki is blamed is the early introduction, in elementary teaching, of abstract and generally useless (at that level) notions, a tendency sometimes referred to by the name "new maths." Let me remind you that the purpose of Bourbaki's treatise is to provide tools for "working mathematicians," i.e. those engaged in research; therefore it has nothing to do with any teaching below the level of the graduate schools of the major universities. There is not the slightest trace of opinions expressed by Bourbaki as to the feasibility or advisability of introducing the concepts described in his treatise at a lower level, and certainly not in primary or secondary schools; the proponents of such aberrations are usually people whose knowledge of present day mathematics is scanty, to say the least.

It turns out that the same answer may be given to another attack on Bourbaki's point of view, namely that he would have encouraged the tendency to publish empty mathematics and to do generalizations for generality's sake. Whenever it was decided to include a notion or a main theorem in Bourbaki, it was only after prolonged discussion, in which its advocates were required to show in which results these notions or results were a crucial tool. In other words, no one can understand or criticize the choices made by Bourbaki unless he has a solid and extended background in many mathematical theories, both classical and more recent; that is, of course, why Bourbaki thinks a premature reading of the treatise may do more harm than good. But he has no means to prevent such a misguided behavior, for it would imply engaging in polemics, and as I told you that is something he has steadfastly refused to do. However, there is a way to test his opinions on that question, namely to look at the list of the nearly 600 "exposés" which were given in the Seminar he has sponsored during the last 25 years; I think an impartial observer will not fail to recognize that they include most of the theories which have been the subject of talks made at the invitation of International Congresses, or on which the recipients of international prizes have worked; and on the other hand, almost none of them deals with empty generalizations.

Of course nobody can predict the future of Bourbaki, for it cannot be separated from the future of mathematics. It may be that once the books on which he is now working reach completion, his collaborators will decide that, for reasons mentioned above, it is not advisable to undertake writing up other parts of mathematics, and that the treatise could come to an end, until future progress makes it obsolete, as happened to all its predecessors. But it is also conceivable that sudden unexpected developments may bring drastic changes in our present conception of mathematics, and then it might be useful to contemplate at least a partial revision to maintain the fundamental purpose: to provide tools for the working mathematician.

Beginning in 1983, the proceedings of the Bourbaki Seminars will be published by the Société Mathématique de France in the series *Astérisque*. The American Mathematical Society has recently concluded an agreement with the Société Mathématique de France for the distribution of *Astérisque* in North America, effective January 1983. See page 704 of this issue of the *Notices* for a detailed announcement, including subscription information and a list of the past titles which will be available from the AMS.