CHAPTER 6.4

Writing the ultimate mathematical textbook: Nicolas Bourbaki's *Éléments de mathématique*

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Mathematical textbooks have played a significant role in the history of mathematics. Still, with a few—if important—exceptions, and especially in the twentieth century, mathematical textbooks do not in general convey new results. Rather, they attempt to summarize and present an updated picture of a discipline. Such summaries can hardly be neutral with regard to the body of knowledge they present. Writing a textbook involves much more than simply putting together previously dispersed results. Rather, it requires *selecting* topics and problems, and *organizing* them in a coherent and systematic way, while *favoring* certain techniques, approaches, and nomenclature over others. A mathematical textbook thus privileges certain avenues of research rather than others. Producing a mathematical textbook involves, above all, providing a well-defined structure of the discipline. But this structure is, in general, not forced upon the author in a unique way. The author makes meaningful choices to produce a distinctive image of the discipline.[1] If the textbook turns out to be successful and influential, it will disseminate this image as the preferred one for the discipline in question. Had the

1. I will refer to the distinction between 'body' and 'images' of mathematical knowledge (Corry 2001; 2004). Roughly stated, answers to questions directly related to the subject matter of any given discipline constitute the body of knowledge of that discipline, whereas claims and knowledge *about* that discipline pertain to the images of knowledge.

author chosen a different image, or had a book conveying a different disciplinary image been more successful, then the subsequent development of that discipline might have been considerably different. Occasionally a new disciplinary image put forward in a textbook constitutes an innovation no less important than a breakthrough individual result (Grattan-Guinness 2004).

Euclid's *Elements* is, of course, the paradigm example of a textbook compiled from existing knowledge that promoted an enormously influential disciplinary image, definitively shaping mathematics (and more) for millennia. Gauss' *Disquisitiones arithmeticae* is a second prominent example, sometimes compared in importance to the *Elements*, although more clearly circumscribed in its aims (Goldstein *et al* 2007). More recently, Nicolas Bourbaki's *Éléments de mathématique* embodied a unique attempt to play a similarly fundamental role in twentieth-century mathematics, with far-reaching ambitions for its impact on the discipline at large. It comprised a collective undertaking that drew on the efforts of scores of prominent mathematicians and appeared as a multi-volume series published between 1939 and 1998 (with new editions and printings appearing to this very day). Its influence spread throughout the mathematical world and it was instrumental in shaping the course of mathematical research and training for decades.

Bourbaki's extremely austere and idiosyncratic presentation—from which diagrams and external motivations were expressly excluded—became a hallmark of the group's style. The widespread adoption of Bourbaki's approaches to specific questions, concepts, and nomenclature indicates the breadth of its influence. Concepts and theories were presented in a thoroughly axiomatic way and discussed systematically, always going from the more general to the particular and never generalizing a particular result. A noteworthy consequence was that the real numbers could only be introduced well into the treatise, and not before a very heavy machinery of algebra and topology had been prepared in advance.

The Bourbaki phenomenon and the presentation of mathematics embodied in the *Éléments de mathématique* was followed in the mathematics community with a mixture of curiosity, excitement, awe, and, less frequently, criticism or even open disgust. This piece from *Mathematical Reviews* is an inspired description of the difficulties readers faced:

> Confronted with the task of appraising a book by Nicolas Bourbaki, this reviewer feels as if he were required to climb the Nordwand of the Eiger. The presentation is austere and monolithic. The route is beset by scores of definitions, many of them apparently unmotivated. Always there are hordes of exercises to be worked painfully. One must be prepared to make constant cross references to the author's many other works. When the way grows treacherous and a nasty fall seems evident, we think of the enormous learning and prestige of the author. One feels that Bourbaki *must* be right, and one can only press onward, clinging to whatever minute rugosities the author provides and hoping to avoid a plunge into the abyss. (Hewitt 1956, 507)

This chapter is devoted to describing the origins and development of the enterprise of writing the *Éléments*, which was often seen, by those who took part in it, as the writing of the ultimate mathematical textbook. The chapter opens with an account of the origins of the group and the first stages of the project. This is followed by a more focused description of the writing of the volumes devoted to algebra and set theory, as well as their relationship to existing textbooks. The following section discusses the centrality of the idea of a mathematical structure for the Bourbakian image of mathematics, and its relationship to the technical contents of the *Éléments*. A final section discusses the conflict that rose in the mid-1950s within the group around the question of whether to adopt the language of categories and functors as a general, unifying language of mathematics.

Bourbaki: a name and a myth

Nicolas Bourbaki is the pseudonym adopted during the 1930s by a group of young French mathematicians who undertook the collective writing of an up-to-date treatise of mathematical analysis adapted to the latest advances and the current needs of the discipline. Among the ten founding members of the group Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, and André Weil—all former students of the *École Normale Supérieure* in the early 1920s—remained the most influential and active within the group for decades. Over the years, many younger mathematicians participated in the group's activities, while the older members were supposed to quit at the age of fifty. All were among the most prominent of their generation, actively pursuing their own research in different specialisms, while the activities of Bourbaki absorbed a part of their time and energies (Chouchan 1995; Mashaal 2006; Beaulieu 2007).

By the early 1930s, the future founders of the group had already launched successful careers and had started publishing important, original research. As was typical in French academic life at the time, their careers started in provincial universities. Weil and Cartan were colleagues at Strasbourg for several years, where they felt increasingly dissatisfied with the way that analysis was traditionally taught in their country and with the existing textbooks written by the old masters (Dieudonné 1970, 136; Weil 1992, 99–100; Beaulieu 1993, 29–30). Edouard Goursat's *Cours d'analyse mathématique* (1903–5) was the most commonly used at the time. Its standards of rigor were unsatisfactory for these representatives of the younger generation. It treated the classical topics of analysis by considering case after case in an extremely detailed fashion, rather than introducing general ideas that could account for many of them simultaneously.

The search for better ways to introduce the basic concepts and theorems of the calculus was a topic of constant conversations between Cartan and Weil. Their predicament also affected their contemporaries teaching at other universities around France and was part of a more general feeling that postwar French mathematics was lagging far behind research in other countries, especially Germany, because of the loss of an entire generation of young mathematicians in the war. This situation provided the central motivation for the deliberations that would lead to the Bourbaki project.

At that time, Cartan and Weil used to meet every fortnight in Paris with their friends Chevalley, Delsarte, Dieudonné, and René de Possel. The framework of the meeting was the 'Séminaire de mathématiques' held from 1933 at the Institut Henri Poincaré under the patronage of Gaston Julia. Visiting mathematicians often participated too, but the 'Séminaire Julia', as it came to be known, was above all a joint production of the proto-Bourbakians. Each academic year, the seminar was devoted to a single general topic in which the participants wished to gain a broader and more systematic knowledge: groups and algebras, Hilbert spaces, topology, and variational calculus. In each meeting, one of the participants was commissioned to prepare a topic for discussion, edit his talk, and then distribute it among the other participants. This approach would later develop into Bourbaki's famous *modus operandi*, described further below.

Over coffee after the meetings of the Séminaire Julia, Weil started to discuss with his friends an ambitious collective initiative to produce the much needed new textbook in analysis. In December 1934, a more clearly delineated plan was stated by Weil, Cartan, Chevalley, Delsarte, Dieudonné, and de Possel: 'to define for 25 years the syllabus for the certificate in differential and integral calculus by writing, collectively, a treatise on analysis. Of course, this treatise will be as modern as possible' (Beaulieu 1993, 28). Following a 'modern' perspective was one of the apparently clear and suggestive ideas that, once the project started to materialize, proved to be in need of a more detailed definition that was not always easily agreed upon. At this meeting several other ideas were suggested concerning the plan of action: subcommittees should be put in charge of the various parts of the treatise; an agreed synopsis should be ready by the summer of 1935; the treatise should be about a thousand pages long; all decisions should be taken by consensus. Even a potential publisher was already in sight: Hermann (whose chief editor, Enrique Freymann was Weil's friend), rather than the leading Gauthier-Villars, where the old masters typically published their treatises.

Under the provisory name of 'Comité de rédaction du traité d'analyse' the group met again in January 1935. This time detailed minutes were taken by Delsarte (2000), who would continue to fulfill this task until 1940. It was decided that the committee would also include Paul Dubreil, Jacques Leray, and Szolem Mandelbrojt. Dubreil and Leray, however, were soon replaced by Charles Ehresmann and Jean Coulomb.

At this second meeting Delsarte and Dubreil presented a list of topics they wanted in the treatise: modern algebra; integral equations with special emphasis on Hilbert space; the theory of partial differential equations with emphasis on more recent developments; and a long section devoted to special functions. Mandelbrojt brought forward a principle that he considered of the utmost importance: whenever a result was intended for discussion in full generality, the general theory needed to prove this result would never be developed in the course of the exposition itself. Rather, all the general, abstract theories would be developed *in advance*. This was in line with the idea of a 'paquet abstrait' that had already been mentioned in the first meeting, and all participants agreed that this principle should be thoroughly pursued. Weil insisted that the treatise should be useful for all possible audiences: researchers, aspiring school teachers, physicists, and 'technicians' of various kinds (Delsarte 2000, 17).

After several preliminary encounters in Paris, the first real Bourbaki working meeting took place in July 1935, at the little town of Besse-en-Chandesse, close to Clermont-Ferrand. It was here that the mythical name was adopted. The expected length of the treatise was now calculated at three thousand two hundred pages and it was planned to be completed within a year. Along with a treatment of the classical themes of analysis, increased attention was given to the basic notions of algebra, topology, and the theory of sets. These now appeared necessary to provide the presentation with the kind of coherence and modern perspective that the group insistently spoke about.

This was the starting point of a long and fascinating endeavor. Its scope, structure, and contents went far beyond the initial plans of the group and their initial assumptions about the amount of work it would require. Except for a break during the war years, over the following decades the group (in its changing membership) continued to organize 'congresses' three times a year at different places around France for a week or two. Minutes of these Bourbaki congresses were circulated among members of the group in the form of an internal bulletin initially called *Journal de Bourbaki* and, from 1940, *La Tribu*.[2] Although *La Tribu* abounds with personal jokes, obscure references, and slangy expressions which sometimes hinder their understanding, they provide a very useful source for the historian researching the development of the Bourbaki project.

At each meeting, individual members were commissioned to produce drafts of the different chapters, which were then subjected to harsh criticism by the other members, and reassigned for revision. Only after several drafts was the final document ready for publication (Cartan in Jackson 1999, 784; Schwartz 2001, 155–163). Each chapter and volume of Bourbaki's treatise was thus the outcome

2. For details on the Bourbaki archives and the issues of *La Tribu* quoted here, see Corry (2004, 293 n13; Krömer 2006, 156–158). Direct quotes are taken from volumes in the personal collection of Professor Andrée Ch. Ehresmann, Amiens, and used with her permission. Other issues are quoted indirectly as indicated.

of arduous collective work. The spirit and viewpoint of the person(s) who had written it was hardly recognizable. The personal dynamics at work in the group are a matter of considerable interest and it represents, no doubt, a unique case in the history of science. For many, the most surprising fact about Bourbaki is that it could work at all.

What was initially projected as a modern analysis textbook eventually evolved into a multi-volume treatise entitled *Éléments de mathématique*, each volume of which was meant to contain a comprehensive exposition of a different mathematical subdiscipline. As with any other textbook, the material covered was not meant to be new in itself, but the very organization of the body of mathematical knowledge would certainly embody a novel overall conception of mathematics and, above all, underlying unity would be stressed. The '*paquet abstrait*', initially conceived as a supporting toolbox of limited scope, gradually took center stage and became the hard core of the treatise, whereas classical topics of courses in analysis were continually delayed and some of them eventually left out of the treatise or relegated to specific sections or to the exercises.[3]

The first chapter of the *Eléments* appeared in 1939. By this time the plan had settled around six basic books: I. Theory of Sets; II. Algebra; III. General Topology; IV. Functions of a Real Variable; V. Topological Vector Spaces; VI. Integration. At a second stage in the 1950s additional chapters were added, including Lie Groups and Lie Algebras; Commutative Algebra; Spectral Theories; Differential and Analytic Manifolds (essentially no more than a summary of results). In its final form the treatise comprised over seven thousand pages, with new chapters continuing to appear until the early 1980s.

In the succeeding decades, Bourbaki's books became classics in many areas of pure mathematics, where the concepts and main problems, nomenclature, and Bourbaki's peculiar style were adopted as standard. The branches upon which Bourbaki exerted the deepest influence were algebra, topology, and functional analysis, becoming the backbone of mathematical curricula and research activity in many places around the world. Notations such as the symbol $\varnothing$ for the empty set, and terms like *injective*, *surjective*, and *bijective*, owe their widespread use to their adoption in the *Éléments de mathématique*. Bourbaki even influenced fields like economics (Weintraub and Mirowski 1994) and, especially in France, anthropology and literature (Aubin 1997).

Yet disciplines like logic, probability, and most fields of applied mathematics, which were beyond Bourbaki's scope, became under-represented in the many places worldwide where Bourbaki's influence was most strongly felt. This was the case for many French and several American universities at various times between 1940 and 1970 (Schwartz 2001, 162–164). Further, group theory and number theory, despite being strong points of some members (notably Weil for number theory) were not treated in the *Éléments*, mainly because they were less amenable to the kind of systematic, comprehensive treatment presented in the collection. As part of an underlying tendency of estrangement from the visual, geometry was completely omitted from the Bourbakian picture of mathematics, except for what could be reduced to linear algebra.

3. Reviewers of Bourbaki, favorable and critical alike, typically describe the choice of exercises as one of the outstanding features of the collection. In most cases Dieudonné was in charge of this choice (Kaplansky 1953). In fact, for many years Dieudonné was the official scribe of the project and 'every printed word came from his pen' (Senechal 1998, 28).

Writing a textbook on modern algebra

As mentioned above, one of the group's declared aims was that their analysis treatise would be 'as modern as possible'. Most likely, the word 'modern' referred in their minds to the current trends in German mathematical research, especially to Bartel van der Waerden's epoch-making *Moderne Algebra* (1930). This book, the most important individual influence behind the entire Bourbaki project, represented the culmination of the deep transformation of algebra that had begun in the last third of the nineteenth century. Before then, algebraic research had mainly focused on theories of polynomial equations and polynomial forms, including algebraic invariants. The ideas implied by the works of Évariste Galois had become increasingly central after their publication by Joseph Liouville (1846). Together with important progress in the theory of fields of algebraic numbers, especially in the hands of Leopold Kronecker and Richard Dedekind, they gave rise to new concepts such as groups, fields, and modules. But this development was only gradually reflected in textbooks.

Towards the end of the 1920s, a growing number of works investigated the properties of the abstractly defined mathematical entities now seen as the focus of algebraic research: groups, fields, ideals, rings, and others. Like many other important textbooks, *Moderne Algebra* arrived at a time when the need was felt for a comprehensive synthesis of what had been achieved since its predecessor, in this case Heinrich Weber's *Lehrbuch der Algebra* (1895). It presented ideas that had been developed by Emmy Noether and Emil Artin—whose courses van der Waerden had attended in Göttingen and Hamburg—and by algebraists such as Ernst Steinitz, whose works he had studied under their guidance (van der Waerden 1975).

Van der Waerden masterfully incorporated many of the important innovations of the early twentieth century into the body of algebraic knowledge. But his book's originality and importance comprises its totally new way of conceiving the discipline. Van der Waerden systematically presented those mathematical branches then related to algebra, deriving all relevant results from a single, unified perspective, and using similar concepts and methods for all of them. The resultant image was based on the realization that a certain family of notions (groups, ideals, rings, fields, and so on) are individual instances of the general idea of an algebraic structure, and that the aim of research in algebra is the full elucidation of those notions. None of them, to be sure, appeared for the first time in this book. Groups had featured in the third edition of Joseph Serret's *Cours d'algèbre supérieure* (1866). Ideals and fields had been introduced by Dedekind in his elaboration of Ernst Edward Kummer's factorization theory of algebraic numbers (1871). But the unified treatment they were accorded in *Moderne Algebra*, the single methodological approach adopted to define and study them, and the compelling new picture of a variety of domains that had formerly been seen as only vaguely related, constituted a striking and original innovation.

One fundamental advance was an implicit redefinition of the conceptual hierarchy underlying the discipline of algebra. Under this image, rational and real numbers no longer had conceptual priority over, say, polynomials. Rather, they were defined as particular cases of abstract algebraic constructs. Thus, for instance, van der Waerden introduced the concept of a field of fractions for integral domains in general, and then obtained the rational numbers as a particular case of this kind of construction, namely as the field of quotients of the ring of integers. His definition of the system of real numbers in purely algebraic terms was based on the concept of a 'real field', recently elaborated by Artin and Otto Schreier, whose seminars van der Waerden had attended in Hamburg.

The task of finding the real and complex roots of an algebraic equation, which was the classical main core of algebra in the previous century, was relegated to a subsidiary role in van der Waerden's book. Three short sections in his chapter on Galois theory dealt with this specific application of the theory, and they assume no previous knowledge of the properties of real numbers. In this way, two central concepts of classical algebra (rational and real numbers) were presented merely as final products of a series of successive algebraic constructs, the 'structure' of which had been gradually elucidated. On the other hand, additional, non-algebraic properties such as continuity and density were not considered at all by van der Waerden as part of his discussion of those systems.

Another of *Moderne Algebra's* important innovations concerns the way in which the advantages of the axiomatic method were exploited in conjunction with all other components of the structural image of algebra. Once one has realized that the basic notions of algebra (groups, rings, fields, and so on) are, in fact, different kinds of algebraic structures, their abstract axiomatic formulation becomes the most appropriate one. The central disciplinary concern of algebra became, in this conception, the systematic study of those different varieties through a common approach, underpinned by the idea of isomorphism. This fundamental recognition is summarized in the *Leitfaden* 'leading threads' in the

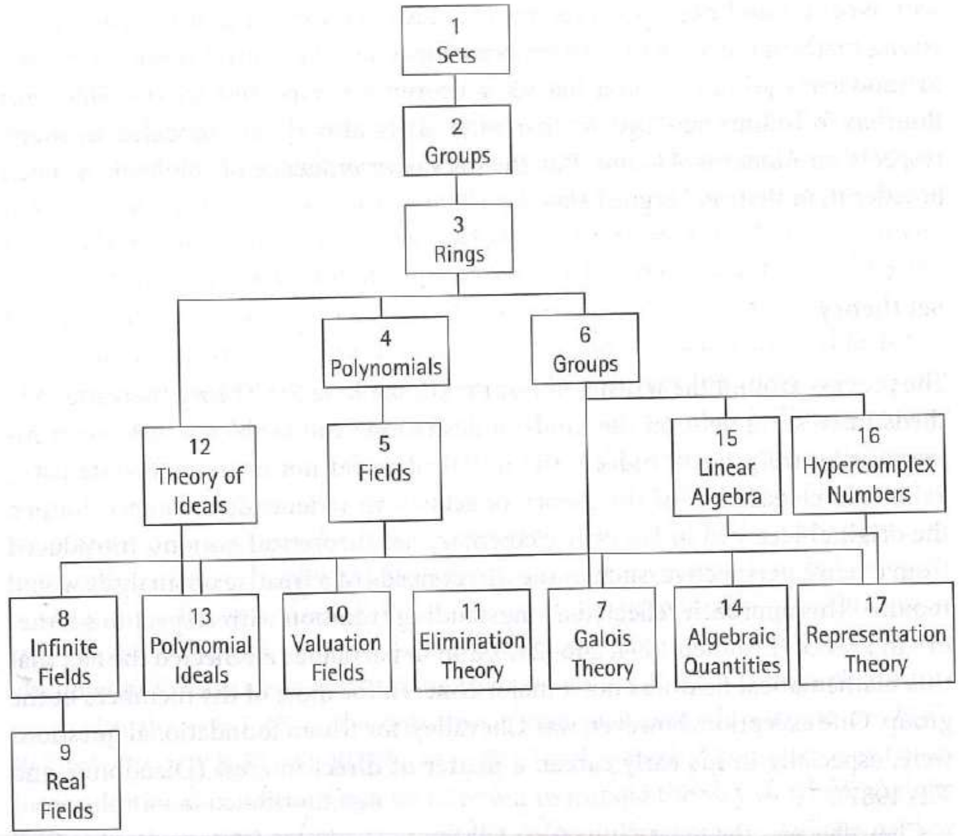

Figure 6.4.1 A translation of the diagram encapsulating the hierarchical relationships between algebraic concepts, presented in the introduction to van der Waerden's seminal *Moderne Algebra* (1930)

introduction to the book, which pictures the hierarchical, structural interrelation between the various concepts investigated in it (Fig. 6.4.1).

Obviously, van der Waerden's new image of algebra reflected the current state of the body of algebraic knowledge. However, that image was *not* a *necessary* outcome of the body, but rather an independent development of intrinsic value. Several other contemporary algebra textbooks also contained most of the latest developments in the body of knowledge, but essentially preserved the classical image of algebra. Perhaps the most interesting example is Robert Fricke's *Lehrbuch der Algebra* (1924), with the revealing subtitle *Verfasst mit Benutzung vom Heinrich Webers gleichnamigem Buche* 'based on Heinrich Weber's book of the same name'.

The main idea embodied in van der Waerden's book—the structural conception of algebra—became highly influential for Bourbaki. Before receiving his doctorate in 1928, Weil had visited Göttingen, where he came into direct contact with Noether and her collaborators. This visit left a significant imprint on the young mathematician, which reverberated through the centrality later accorded to modern algebraic approaches as a unifying perspective in the *Éléments*. Bourbaki's volume on *Algebra* (hereafter *A*) is also closely modeled in many respects on *Moderne Algebra*. But the pervasive influence of the book is much broader than that, as I argue below.

Set theory

The process around the writing of Bourbaki's book on Set Theory (hereafter *ST*) sheds interesting light on the kinds of hesitations and problems that accompanied the entire project. Indeed, the initial plan did not envisage a systematic, axiomatic elaboration of the theory of sets as an independent subject. Rather, the original idea was to use only elementary set-theoretical notions, introduced from a naive perspective, such as the direct needs of a treatise on analysis would require. This approach reflected a longstanding tradition with respect to set theory in France (Beaulieu 1994, 246–247), and in particular it reflected the fact that this mathematical field was not a major concern for most of the members of the group. One exception, however, was Chevalley, for whom foundational questions were, especially in his early career, a matter of direct interest (Dieudonné and Tits 1987).

Chevalley was the most active force behind the inclusion of a separate book on set theory as the plan evolved for the contents of the *Éléments*. In 1949 *La Tribu* pointed to the underlying discussions around one of the main questions that had occupied the Bourbaki project from the beginning: the possibility of presenting a self-contained, highly formalized treatment of the entire body of mathematics, with little or no external motivation of the topics treated. Discussions repeatedly arose around the exact way to present many individual topics or theories. This was clearly the case with sets, debates around which continually delayed publication. In the final account, the contents of *ST* were a compromise between the attempt to fully formalize the topic and explore it in detail, as demanded by Chevalley, and the need to produce a relatively easily readable book that would provide a basic language for the treatise while fitting the general reader's interest. Thus, set theory was adopted as a universal language underlying all mathematical domains because of its unifying capabilities (Bourbaki 1968, 9). But this very basic theory was not presented in a truly formalized language, because Bourbaki acknowledged that no mathematician actually works like that: 'his experience and mathematical flair tell him that translation into formal language would be no more than an exercise of patience (though doubtless a very tedious one)' (Bourbaki 1968, 8). Within the entire treatise, only in *ST* does one find explicit statements like this.

The question of the consistency of set theory also arises here. Bourbaki did not attempt to address the question head-on but rather reverted to a strongly empiricist position. In an ironic turn, Bourbaki simply stated that a contradiction was not expected to appear in set theory because none had appeared after so many years of fruitful research (Bourbaki 1968, 13). Yet one of Bourbaki's earlier publications had stated that 'absence of contradiction, in mathematics as a whole or in any given branch of it, thus appears as an empirical fact rather than as a metaphysical principle... We cannot hope to prove that every definition... does not bring about the possibility of a contradiction' (Bourbaki 1949, 3).[4]

A *Fascicule de résultats*, 'Summary of results', on set theory was published as early as 1939. The final volume was published only during the 1950s, comprising the following chapters: 1. Description of Formal Mathematics; 2. Theory of Sets; 3. Ordered Sets, Cardinals, Integers; 4. Structures. This fourth chapter introduced the new concept of *structure*,[5] which was meant to provide a formal notion that supposedly underlies all other mathematical theories described in the remaining parts of the treatise. Briefly put, in order to define this concept Bourbaki considered a finite collection of sets $E_1, E_2, \ldots, E_n$, and used an inductive procedure, each step of which consists either of taking the Cartesian product $(E\times F)$ of two sets obtained in former steps or of taking their power set $\mathbf{B}(E)$. For example, beginning with the sets E, F, G the outcome of one such procedure could be: $\mathbf{B}(E)$; $\mathbf{B}(E)\times F$; $\mathbf{B}(G)$; $\mathbf{B}(\mathbf{B}(E)\times F)$; $\mathbf{B}(\mathbf{B}(E)\times F)\times \mathbf{B}(G)$ and so forth. Upon such constructs some additional conditions can be imposed to imitate the way in which various known mathematical entities are typically defined. For instance, an internal law of composition on a set A is a function from $A\times A$ into A. Accordingly, given any set A, one can form the scheme $\mathbf{B}((A\times A)\times A)$ and then choose from all the subsets of $(A\times A)\times A$ those satisfying certain conditions of a 'functional graph' with domain $A\times A$ and range A. The axiom defining this choice is a special case of what Bourbaki calls an algebraic *structure*. Similarly, Chapter 4 of *ST* showed how the general concept allowed for the definition of other types, such as ordered *structures* or topological *structures*. Finally, the general definition of *structures* led to some further, related concepts such as isomorphism among *structures*, deduction of *structures*, poorer and richer *structures*, equivalent species of *structures*, etc. Chapter 4 was the most idiosyncratic of the volume and of the entire collection, and in an important sense the most problematic one.

4. Imre Lakatos (1978, II 24–42) has called attention to the fact that foundationalist philosophers of mathematics, from Russell onwards, when confronted with serious problems in their attempts to prove the consistency of arithmetic, have not hesitated to revert to empirical considerations as the ultimate justification for it. Although Bourbaki is not mentioned among the profusely documented quotations selected by Lakatos to justify his own claim, it seems that these passages of Bourbaki could easily fit into his argument. See also Israel and Radice (1976, 175–176).

5. Hereafter I write *structures* (italicized) to indicate this specific, Bourbakian technical term, as opposed to the non-formal, general usage of the term.

The *Fascicule de résultats* is strikingly different from the chapters themselves. Whereas the book's stated aim is to show that it is possible to provide a sound, formal basis for mathematics as a whole, the *Fascicule* aims simply to provide the basic lexicon and to explain the non-formal meaning of the terms used. Thus, the opening lines read:

As for the notions and terms introduced below without definitions, the reader may safely take them with their usual meanings. This will not cause any difficulties as far as the remainder of the series is concerned, and renders almost trivial the majority of the propositions. (Bourbaki 1968, 347)

Thus, for example, the painstaking effort invested in Chapters 2 and 3 is here represented by the laconic statement: 'A set consists of *elements* which are capable of possessing certain *properties* and of having relations between themselves or with elements of other sets' (Bourbaki 1968, 347). As for *structures*, the *Fascicule* reduces the whole formal development to a very short, intuitive explanation of the concepts in which the main ideas are explained. The only important related concept which is mentioned is that of isomorphism.

Between the appearance of the *Fascicule* in 1939 and the four chapters in 1954–7 there were many important developments in mathematics, in particular the emergence of category theory. As a consequence, some of the ideas that had perhaps looked very promising in 1939 soon became obsolete. Thus, *ST*, and especially its chapter on *structures*, became one of the least interesting of the entire collection. As a textbook for the discipline, it received little attention and very few of its concepts and notations were widely adopted. As Paul Halmos put it:

It is generally admitted that strict adherence to rigorously correct terminology is likely to end in being pedantic and unreadable. This is especially true of Bourbaki, because their terminology and symbolism are frequently at variance with commonly accepted usage. The amusing fact is that often the 'abuse of language' which they employ as an informal replacement for a technical name is actually conventional usage: weary of trying to remember their own innovation, the authors slip comfortably into the terminology of the rest of the mathematical world. (Halmos 1957, 90)[6]

Even more interesting, the terminology and the concepts introduced in the set theory book, and particularly on the chapter on *structures*, were *hardly used in the other parts of Bourbaki's own book*. And, on the few occasions when it was used, this only made more patent the ad hoc character of the supposedly fundamental part of the treatise. In order to understand this important point in its precise context, it is necessary now to discuss the role of 'structure' in Bourbaki's overall conception of mathematics.

6. For a detailed review of Chapters 1 and 2 of *ST*, see Halmos (1955). For an assessment of the technical shortcomings of Bourbaki's system of axioms for the theory of sets, see Mathias (1992).

Two meanings of 'structure'

As work on the treatise developed, an implicit but pervasive idea increasingly came to underlie the overall approach. This was the conception of mathematics as a systematic, elaborate hierarchy of structures: essentially an extension of the idea from van der Waerden's algebra textbook. He had undertaken a unified 'structural' investigation of several concepts that were defined in similar, abstract terms (groups, rings, ideals, modules, fields, hypercomplex systems) while asking similar kinds of questions about them and using similar kinds of tools to investigate them. Now, in Bourbaki's textbooks, algebra, topology, and functional analysis started to appear as individual materializations of one and the same underlying, general idea: the idea of a mathematical structure. Bourbaki attempted to present a unified, comprehensive picture of what they saw as the main core of mathematics, using a standard system of notation, addressing similar questions in the various fields investigated, and using similar conceptual tools and methods across apparently disparate mathematical domains.

In 1950 Dieudonné, under the name of Bourbaki, published an article that came to be identified as the group's manifesto, 'The architecture of mathematics'. Faced with the unprecedented growth and diversification of the discipline, Dieudonné again raised the well-known question of the unity of mathematics. Mathematics was a strongly unified branch of knowledge in spite of appearances, he claimed, and now it was clear that the basis of this unity was the use of the axiomatic method, as the work of David Hilbert had clearly revealed.[7] Mathematics should be seen, Dieudonné added, as a hierarchy of structures at the heart of which lie the so called 'mother structures':

At the center of our universe are found the great types of structures,…they might be called the mother structures…Beyond this first nucleus, appear the structures which might be called multiple structures. They involve two or more of the great mother-structures not in simple juxtaposition (which would not produce anything new) but combined organically by one or more axioms which set up a connection between them…Further along we come finally to the theories properly called particular. In these the elements of the sets under consideration, which in the general structures have remained entirely indeterminate, obtain a more definitely characterized individuality. (Bourbaki 1950, 228–229)

Thus, the idea that van der Waerden had applied successfully and consistently but only implicitly—namely the centrality of the hierarchy of structures—became now explicit and constitutive for Bourbaki. At the same time, an elaborate attempt was made in Chapter 4 of *ST* to present a formal definition of *structure*, which was somehow meant to provide a solid conceptual foundation on which

7. Dieudonné frequently described Bourbaki as Hilbert's 'natural heir'. Nevertheless, there were very significant differences between their respective conceptions. See Corry (1997; 2001).

the whole edifice of mathematics could be built. Thus, two different meanings of the term mathematical 'structure' appeared in Bourbakian discourse, which were not always properly distinguished from one another: (1) a non-formal and perhaps even metaphorical meaning, used for example in Dieudonné's manifesto to present the entire science of mathematics as a hierarchy of structures, and implicitly implemented by van der Waerden in his new image of algebra, and (2) a formal technical term, *structure*, appearing in a mathematical theory that was never incorporated into current mathematical research or exposition, and was not even really used by Bourbaki in their own treatise.

As already stated, the main interest of most members of the group was in the various disciplines covered in the treatise but not in *ST* or in its chapter on *structures*. And yet many discussions about the correct way to present those various disciplines were necessarily influenced by the introduction of the basic concepts associated with *structures*. It is remarkable that members of the group tended not to separate the two meanings clearly, thus giving the impression that it was Bourbaki's own formal concept of *structure*, and not the general, structural image of mathematics, that was so central to much of twentieth century mathematics.

Bourbaki's theory of *structures* never received any real attention from working mathematicians, even Bourbaki's members when involved in their own research. When we look at how the concept of *structure* was used in the treatise, all we see is that in the opening chapters of the books on branches such as algebra and topology, some sections were devoted to showing how that branch could, in principle, be formally connected to the general concept of *structure*. This connection, however, was rather feeble, a formal exercise that was forgotten after the first few pages. For instance, while *A* presents vector spaces as a special case of groups, so that all the results proved for groups hold for vector spaces too, this hierarchical relationship is not presented in terms of the concepts defined in *ST*. Likewise, neither commutative groups nor rings are presented as *structures* from which a group can be 'deduced', nor is it proved that Z-modules and commutative groups are 'equivalent' *structures*. *Structure*-related concepts do appear in the opening sections of *A*, but the rather artificial use to which they are put and their absence from the rest of the book suggests that this initial usage was an ad hoc recourse to demonstrate the alleged subordination of algebraic concepts to the more general ones introduced within the framework of *structures*. Neither new theorems nor new proofs of known theorems are obtained through the *structural* approach.

As the book advances further into theories of the hierarchy of algebraic structures, the connection with *structures* is scarcely mentioned. Ironically, the need for a stronger unifying framework was indeed felt in later sections. For instance, Chapter 3 discusses three types of algebras defined over a given commutative ring: tensor, symmetric, and exterior algebras. Bourbaki defines each kind and then discusses for each case, 'functorial properties', 'extension of the ring of

scalars', 'direct limits', 'free modules', 'direct sums', and so on (Bourbaki 1973, 484–522). Not only would a unified presentation of the three have been more economical and direct but their properties lend themselves naturally to a categorical treatment, a possibility which is not even mentioned. The 'functorial properties' of the algebras are explained through the use of the standard categorical device of 'commutative diagrams', but without mentioning the concepts of functor or category.

The volume on *General Topology* is the most outstanding example of a theory presented through Bourbaki's model of the hierarchy of *structures*, starting from one of the 'mother structures' and descending to a particular *structure*, namely that of the real numbers. And yet, as with *A*, the hierarchy itself is not introduced in terms of *structure*-related concepts. Thus for instance, topological groups are not characterized as a *structure* from which the *structure* of groups can be 'deduced'. *Structure*-related concepts appear in this book more than anywhere else in the treatise but, instead of reinforcing the purported generality of such concepts, a close inspection of their use immediately reveals their ad hoc character.

The central notion of structure, then, had a double meaning in Bourbaki's mathematical discourse. On the one hand, it suggested a general organizational scheme for the entire discipline, which turned out to be very influential. On the other hand, it comprised a concept that was meant to provide the underlying formal unity but was of no mathematical value whatsoever either within Bourbaki's own treatise or outside it. But Bourbaki's theory of *structures* was only one among several attempts after 1935 to develop a general mathematical theory of structures, and was not even the only such attempt in which members of the group were involved.[8] Thus, in order to understand the full historical and mathematical context of the theory of *structures* and its role within the *Éléments*, we now discuss the conflict created by the rise of its most serious competitor, the theory of categories.

The categorical imperative and its demise

In the early 1940s, Samuel Eilenberg and Saunders Mac Lane, who would both later become involved in Bourbaki, introduced the concepts of category and functor. These concepts and the general perspective they furnished gradually became a widely adopted unifying tool and language for mathematical disciplines, and pursued a structural spirit similar to Bourbaki's. Groundbreaking early instances

8. In Corry (2004) I presented a full account of such reflexive theories of structures, their origins and their interrelations.

appear in the works of two younger-generation Bourbaki members, Alexander Grothendieck and Jean-Pierre Serre, who used categories in the early 1950s as basic tools for their own research in homological algebra and algebraic geometry (Krömer 2007, 117–190). Against this background, it is only natural to expect that the categorical approach would easily find its way into Bourbaki's debates as an ideal candidate to support the unifying, structure-oriented perspective that the group had been striving for. Indeed, the idea was discussed at various Bourbaki congresses but in the end it never materialized.[9]

If categorical language were to be adopted by Bourbaki as a unifying language for the *Éléments*, this would entail the reformulation of considerable parts of existing chapters to make them fit the new approach. The chapter on *structures* in *ST* would be a particularly obvious nuisance. As already mentioned, this entire chapter was rather ad hoc and in any case did not represent a main focus of interest for most members of the group. This in itself was a meaningful obstacle to incorporating categories into the treatise; additional obstacles came from diverging views about the intrinsic value of the categorical approach in general. Weil, for one, actively opposed the introduction of categories in any way into the *Éléments*.

Some topics discussed in Bourbaki's book on *Commutative Algebra* were presented in a manner for which the categorical formulation would have been the most natural, but without explicitly doing so (Corry 2004, 327–328). This was also the case with other topics on which Bourbaki had already published by 1950 or would soon publish. During the 1950s *La Tribu* documented recurring attempts to write chapters on homological algebra and categories, and the discussions that ensued. In 1951, Eilenberg was commissioned several times to prepare drafts to be discussed. He had not only created the theory of categories with Mac Lane. In the 1950s he was collaborating on the first two books to systematically use this language to present elaborate mathematical disciplines that had emerged and developed in completely different terms: algebraic topology (Eilenberg and Steenrod 1952) and homological algebra (Cartan and Eilenberg 1956). When it came to Bourbaki, however, he immediately realized the serious difficulties to be expected in the context of the Bourbaki treatise, because it had already introduced *structures*. In an undated, unpublished text possibly written around that time, he said so explicitly:

> The method of functors and categories is in some sort of 'competition' with the method of structures as developed at present. Unless this 'competition' is resolved only one of these methods should be presented at the early stage.... The resolution of the 'competition' is only possible through the definition of the 'structural homomorphism' which would certainly require a serious modification of the present concept of structure. It would certainly complicate further this already complicated concept. Despite my willingness to complicate things I am still unable to produce a general definition that would fit known typical cases.[10]

9. In Corry (1992) I called attention to the inherent tension between *structures* and categories, and published some illuminating related documents (mainly issues of *La Tribu*), some of which are also included here. More recently, Ralf Krömer (2006) has added significant insights to this important point, using previously unpublished material, some of which I quote below.

Over the next few years, the younger-generation Bourbaki members increasingly adopted categorical language for their own research, and repeatedly attempted to introduce it to the *Éléments*. At this time, *structures* had been announced in the *Fascicle* of 1939 but the related chapter in *ST* had not yet been worked out. In principle, there was still room for categories, but, as Eilenberg was quick to see, this would require more than trivial reformulation. *La Tribu* documents heated debates around *structures* and categories throughout the 1950s, which culminated on publication of Grothendieck's famous *Tohoku* article (1957), a milestone in the history of category theory. In it Grothendieck innovatively applied cohomological methods (fully couched in categorical language) to algebraic geometry, thus opening the road for developments that would continue to engage mathematicians for decades. *La Tribu* and the contemporary Serre-Grothendieck correspondence (Colmez and Serre 2001) provide clear evidence that Grothendieck had conceived his famous article as a possible contribution to the Bourbaki treatise. Grothendieck's functorial ideas were well received by most of the group's younger generation, and by Dieudonné, but the continued opposition of others, especially Weil, prevented their adoption in the *Éléments*.

The chapter on *structures* came out in 1957 without the slightest explicit reference to categorical ideas. The incompatibility of the two approaches and the work already invested were the main reasons behind this decision. Cartan wrote that the *structural* point of view should not be abandoned without 'very serious reasons'. Some members of the group, however, notably Grothendieck, were highly dissatisfied. He continued to suggest that a new Chapter 4 of *ST* should replace the old one, 'unusable in all respects' (Krömer 2006, 144).

It is important to delineate more precisely the internal historical context within which this discussion was taking place. By the mid-1950s younger members (Serre, Grothendieck, and others) had started to join the group. Naturally, and partly because of Bourbaki's influence, the mathematical scene was by then very different to that faced by the founding fathers over twenty-five years earlier. At the same time, the age of self-imposed retirement at fifty had arrived for the latter (but was not always strictly adhered to). To the extent that the younger generation members wanted to invest their energies in the Bourbaki project they pursued agendas that differed at various levels from the original one, and also, sometimes,

10. Quoted in (Krömer 2006, 142), from an original document in the Eilenberg archive, Columbia University, New York.

from those of each other. Towards the end of the decade, the first six books of the *Éléments* had essentially been completed, covering much of what the group had come to consider as the hardcore of the project. It was time to deal with more advanced and specialized topics, while the younger members wanted a say in the project's overall direction. The possibility of universal participation in each topic and the original view that writing should not be assigned to 'specialists' were both reconsidered. Basic questions about the entire enterprise arose anew, provoking conflicting views and sometimes personal tensions. The debate around the adoption of categories was part of this situation, particularly the opposition between Grothendieck and Weil, two strongly opinionated mathematicians and difficult people to deal with.

Indeed, Weil was a very dominant character whose mathematical prestige and intellectual personality, coupled with his authority as one of the leading forces in the Bourbaki project, bestowed upon him an undisputed, unique position within the group. The retirement of some prominent members over the years has commonly been attributed to conflicts or tensions with Weil. That was certainly the case with de Possel, to whom Evelyn, Weil's wife since 1939, had previously been married. Weil had been the first to suggest that members should retire from active participation at the age of fifty, but ironically, on arriving at that age in 1956 he gave very little sign of wanting to diminish his influence on the project.

Grothendieck, in turn, was a highly unconventional personality even by the standards of this bunch of rather unconventional individuals. He was born in Germany, but escaped during the war to France. He remained an alien citizen, which created obstacles to finding a position in his new country. In 1959, Grothendieck got a research position in the newly created Institut des Hautes Etudes Scientifiques (IHES), where he spent twelve years creating and teaching his revolutionary ideas. In the framework of Bourbaki, he favored the continuation of the generalizing spirit that had permeated the early books, but with more powerful, increasingly abstract, algebraic tools. Not all members, however, approved. Many years later, Armand Borel recalled that Grothendieck's approach was at times 'discouragingly general, but at others rich in ideas and insights', and thus, 'it was rather clear that if we followed that route, we would be bogged down with foundations for many years, with a very uncertain outcome' (Borel 1998, 376).

In Grothendieck's memoirs, a remarkable document called *Récoltes et semailles* 'Reaping and sowing', which initially circulated only within closed circles,[11] he referred to his special status within the group, while pointing to the underlying tension with Weil:

11. Two useful websites containing digital editions of *Récoltes et semailles*, and additional material related to Grothendieck, are http://math.jussieu.fr/~leila/grothendieckcircle/index.php and http://kolmogorov.unex.es/~navarro/res.

> ...until around 1957 I was regarded with certain reservations by more than one member of the Bourbaki group after it had finally co-opted me, I believe, with some reticence.... More often than not, I was, moreover, the one most frequently excluded from the Bourbaki congresses, especially during the common readings of the drafts, as I was rather incapable of following the readings and discussions at the pace in which they were conducted. I am possibly not really gifted for collective work. However, the difficulty I had in coping with group-work or the kind of reservation I may have elicited for other reasons from Cartan and others did not once attract sarcastic remarks or rebuffs, or even a shadow of condescension, except once or twice from the part of Weil (evidently a very different case!).[12] (Grothendieck undated I, 142–143)

From Grothendieck's correspondence with Serre in 1956, it is quite evident that both mathematicians disliked Weil's style, although they surely recognized the importance and originality of his ideas for their own concerns (Colmez and Serre 2001, 49–53). Writing retrospectively about this period, Grothendieck put matters in proportion, stressing the positive balance that he attributed to the project and to Weil's role within it (Grothendieck undated, I 46). As it happened, however, Grothendieck quit the group around 1958–59 while some members, such as Serre and Dieudonné, continued to be his close friends and collaborators. In 1970 he completely retired from public scientific life, when he discovered that IHES was partly funded by the military.

Laurent Schwartz, who had directed Grothendieck's dissertation, explained why the latter remained in the group for only a few years: 'he lacked humor and had difficulty accepting Bourbaki's criticism' (Schwartz 2001, 284). There is every reason to accept this explanation, yet there is also clear evidence that the non-adoption of category theory and Weil's attitude towards this question and towards Grothendieck were the main reasons for the latter's decision to quit. An anonymous text (possibly by Serge Lang) was appended to *La Tribu* in the early 1960s under the title *Ad majorem fonctori gloriam*, 'To the greater glory of functors'. It described Grothendieck's departure as a clear indication of a decline in the originally innovative spirit of the Bourbaki enterprise, implying that Weil was to blame:

> I have learnt that Grothendieck is no longer a member of Bourbaki. I regret that very much, as I regret the circumstances that led to this decision... [namely,] a systematic opposition, more or less explicit depending on this or that person, against his mathematical point of view, and especially against the use of the latter by Bourbaki.... It is

12. Ce fait est d'autant plus remarquable que jusque vers 1957, j'étais considéré avec une certaine réserve par plus d'un membre du groupe Bourbaki, qui avait fini par me coopter, je crois, avec une certaine réticence.... J'étais d'ailleurs le plus souvent largué pendant les congrès Bourbaki, surtout pendant les lectures en commun des rédactions, étant bien incapable de suivre lectures et discussions au rythme où elles se poursuivaient. Il est possible que je ne suis pas fait vraiment pour un travail collectif. Toujours est-il que cette difficulté que j'avais à m'insérer dans le travail commun, ou les réserves que j'ai pu susciter pour d'autres raisons encore à Cartan et à d'autres, ne m'ont à aucun moment attiré sarcasme ou rebuffade, ou seulement une ombre de condescendance, à part tout au plus une ou deux fois chez Weil (décidément un cas à part !).

a scandal that Bourbaki not only did not take the lead in the functorial movement, but rather that is not even in its tail.... If some of the founding members (e.g., Weil) wish to revert on the decision not to influence the direction that Bourbaki wants to follow, he should say so explicitly.... If Bourbaki refuses, not just to join the new movement, but to take the lead in it, then those treatises pursuing the formulation of the elements of mathematics (and not just those dealing with algebraic geometry) will be written by others who will take inspiration not in the spirit of Bourbaki 1960, but in his spirit 1939. That would be a great pity.[13] (Krömer 2006, 152–153)

The consequences of the debate around categories and *structures* continued to be felt for many years, and are manifest in Bourbaki's book on homological algebra, published in 1980 as a chapter 10 of *A*. Categories had become the standard framework for treating homological concepts ever since Cartan and Eilenberg's famous textbook of 1952. In Bourbaki's presentation, however, these concepts are defined within the narrower framework of modules, as using the language of categories here would have gone against the most basic principles that had guided the enterprise since it inception. Thus, whereas Bourbaki's treatment of general topology in the 1940s had embodied a truly innovative approach that many others were to follow, this was hardly the case with homological algebra in the 1980s. This irony is further enhanced by the fact that Bourbaki's own theory of *structures* was not even mentioned in this final volume of the by now truly classic treatise.

Conclusion

The Bourbaki project reached its high-point of success and influence during the 1960s but the impetus that had characterized the project in its early years could not be maintained indefinitely. Dieudonné's catalyzing role could hardly be matched after his retirement. Some new chapters were proposed which never materialized, on topics such as analysis of several complex variables, homotopy theory, spectral theory of operators, and symplectic geometry. Nothing came either of plans to rewrite the first six books. The new books that did appear by

13. J'apprends que Grothendieck n'est plus membre de Bourbaki. Je le regrette beaucoup, ainsi que les circonstances qui ont amené cette décision... Ce qui importait, c'est une opposition systématique, plus ou moins explicitée selon les uns ou les autres, contre son point de vue mathématique, ou plutôt son emploi par Bourbaki.... C'est un scandale que Bourbaki, non seulement ne soit pas à la tête du mouvement functorial, mais encore n'y soit même pas à la queue.... Si certains membres fondateurs (e.g., Weil) désirent revenir sur leur décision de ne pas influencer Bourbaki dnas la direction qu'il désire prendre, qu'ils le disent explicitement.... Si Bourbaki refuse, non pas de se mettre dans le nouveau mouvement, mais d'en prendre la tête, alors les traités visant à la redaction des éléments des mathématiques (et pas seulment à ceux de la géométrie algébrique) seront rédigés par d'autres, qui s'inspireront non pas de l'esprit de Bourbaki 1960, mais de son esprit 1939. Ce serait dommage.

1980 included a summary of differential and analytic manifolds, seven chapters on commutative algebra, eight chapters on Lie groups and Lie algebras, and two chapters on spectral theories. In the 1970s the group found itself involved in a legal dispute with its publisher, which absorbed a great amount of energy.

Partly because of the very success and impact of the project, the need for its continued development became much less pressing. The name of Bourbaki also started to elicit negative reactions: for many it represented a style to be avoided, rather than emulated. The backlash was gradually felt by the younger members of the group, which probably affected their own willingness to invest their efforts in the project. Grothendieck, for one, wrote openly about it in his memoirs:

I can recall my astonishment when in 1970 I discovered the extent to which the name itself, Bourbaki, had become unpopular within large circles (theretofore unknown to me) of the mathematical world, which considered it more or less a synonym of elitism, of narrow-minded dogmatism, of a cult of 'canonical' form at the expense of concrete understanding, of hermetism, of castrating anti-spontaneity and so on! (Grothendieck undated I, 49)[14]

Grothendieck also disapproved of the way some of his colleagues (possibly mainly Weil), disparaged interests and approaches that differed from the typical Bourbakian ones:

It was only during the sixties that, as I remember, some of my friends would denigrate mathematicians whose work did not interest them as 'bullshitters'. Since this concerned matters hardly known to me at the time, I tended to accept such appraisals at face value, for I was impressed by such off-hand assurance—until the day when I discovered that such and such 'bullshitters' were persons endowed with deep and original minds who had not had the luck of pleasing my brilliant friend. (Grothendieck undated I, 148)[15]

Of course, one must bear in mind that these memoirs were written from a position of total retirement and deep hostility towards not just individual members of Bourbaki, but the scientific community in general.

Bourbaki's *Éléments de mathématique* became a most influential and widely used classic textbook of twentieth-century mathematics. Generations of students learnt their algebra or topology from the treatise. More than that, it was a

14. Je me rappelle encore de mon étonnement, en 1970, en découvrant à quel point le nom même de Bourbaki était devenu impopulaire dans de larges couches (de moi ignorées jusque là) du monde mathématique, comme synonyme plus ou moins d'élitisme, de dogmatisme étroit, de culte de la forme 'canonique' aux dépens d'une compréhension vivante, d'hermétisme, d'antispontanéité castratrice et j'en passe !

15. C'est au cours des années soixante seulement que je me rappelle tel de mes amis, qualifiant d' 'emmerdeurs' tels mathématiciens dont le travail ne l'intéressait pas. S'agissant de choses dont je ne savais pratiquement rien par ailleurs, j'avais tendance à prendre pour argent comptant de telles appréciations, impressionné par tant d'assurance désinvolte - jusqu'au jour où je découvrais que tel 'emmerdeur' était un esprit original et profond, qui n'avait pas eu l'heur de plaire à mon brillant ami.

highly useful work of reference. Further, the fact that Bourbaki chose to include some disciplines in the treatise while omitting others was itself an influential factor in the way that mathematical careers were built in various places around the world. Some readers may have been aware of the connection between the distinctive mathematical style of the text and the unique collective mechanism that produced it. Most of them surely knew, at least, that the Bourbaki enterprise involved something different from other textbooks authored in the standard way. Very few, of course, knew the details of the internal debates and how they had led to the final product. In all likelihood, no one outside the inner circle was aware of the tension and conflicts surrounding the *structures* versus categories question discussed above. But the truly curious point is that for all of its success and impact, the *Éléments* did not become a textbook of choice for the study of analysis, as originally intended by the founding members. Much less was it used by 'all possible audiences: researchers, aspiring school teachers, physicists, and "technicians" of various kinds' as Weil had initially called for. Goursat's *Cours d'analyse mathématique* was superseded, both in France and elsewhere, by more up-to-date textbooks soon after Bourbaki started its activities and in accordance with their original motivation. Students around the world who took traditional introductory courses in differential and integral calculus went on to study from the many texts that became available over the next decades in a multitude of languages and that followed a multitude of approaches, but never did so with the text that the 'Comité de rédaction du traité d'analyse' had had in mind in their early meetings of 1935.[16]

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16. An expanded version of this article, including several additional original documents and a detailed section on the origins of Bourbaki's book on General Topology can be downloaded from http://www.tau.ac.il/~corry/publications/articles/Bourbaki%20-%20OHHM.html.

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INTERACTIONS AND INTERPRETATIONS

7. Intellectual