Writing the Ultimate Mathematical Textbook:
Nicolas Bourbaki's *Éléments de mathématique*

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That mathematical textbooks have played a significant role in the history of mathematics goes without saying. Still, with counted–if important–exceptions, and especially in the twentieth century, mathematical textbooks do not in general convey specific new results. Rather, they attempt to summarize and present an updated picture of the state of the art in a certain discipline. Such summaries can hardly be neutral with regard to the body of knowledge they present. The writing of a textbook involves much more than simply putting together existing results that happen to be theretofore dispersed around various journals or other kinds of sources. Rather, it requires *selecting* topics and problems, and *organizing* them in a coherent and systematic way, while *favoring* certain techniques, approaches, and nomenclature over other available ones. In doing so, a mathematical textbook implicitly or explicitly opens the way for certain avenues of further research that are thus preferred over alternative ones. Producing a mathematical textbook requires, above all, providing a well-defined structure of the discipline as conceived by the author. But this structure is, in general, not forced upon the author in a unique way by an accumulated mass of known results. There is room for choice and the author makes meaningful choices while writing the book, adding up to a distinctive image of the discipline.[1] If the textbook turns out to be successful and influential, this influence will proceed in the first place through the dissemination of this image as a preferred one

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[1] I will refer to the distinction between "body" and "images" of mathematical knowledge, on which I have elaborated in greater detail in (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.

for the discipline in question. Had the author chosen a different image, or had a book conveying a different image of the discipline been more successful than it, then the subsequent historical development of the discipline might have been considerably different. Occasionally a new image of the discipline put forward in a textbook implies in itself an important innovation of no less importance than that of a breakthrough individual result.[2]

Euclid's *Elements* is, of course, the paradigmatic example of a textbook built as a compilation of existing knowledge in a discipline that put forward an enormously influential disciplinary image and that definitely shaped the historical course of development in mathematics (and beyond) for centuries to come. Gauss' *Disquisitiones Arithmeticae* is a second, prominent example, sometimes compared in relative importance to Euclid's, although more clearly circumscribed in its aims.[3] More recently, Nicolas Bourbaki's *Éléments de mathématique* embodied a unique attempt to play a similarly fundamental role within the context of twentieth-century mathematics, with far-reaching ambitions concerning the aims and the scope of its intended impact on the discipline at large. It comprised a collective undertaking that drew the efforts of scores of prominent mathematicians and appeared as a multi-volume series whose various parts were successively published between 1939 and 1998 (with new editions and printings appearing to this very day). Its influence spread all over the mathematical world and it was instrumental in shaping the course of mathematical research and training for decades.

The Bourbaki phenomenon and the specific presentation of mathematics embodied in the *Éléments de mathématique* was followed in the mathematics community with a mixture of curiosity,

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[2] For a comprehensive survey of the impact of mathematical textbooks on modern western mathematics see (Grattan-Guinness (ed) 2004).

[3] For a recent analysis of the impact of this book, see (Goldstein et al (eds) 2007).

excitement, awe, and, les frequently, criticism or even open disgust. The *Mathematical Reviews* features in this regard some examples that, concerning both style and contents, one can hardly find for any other publication ever reviewed in that journal. Indeed, the legend surrounding the group and the professional stature of the researchers who composed its membership occasionally impaired the objectivity of appraisals of Bourbaki's contributions. The following is an inspired description of these difficulties published in the *Reviews*:

> 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. Nevertheless, even a quite ordinary one-headed mortal may have notions of his own, and candor requires that they be set forth. (Hewitt 1956, 507. Italics in the original)

Or take, for example, the following review by Pierre Samuel (1921-), himself a member of the group, of a new edition of one of Bourbaki's books:

> If the preceding editions [of the book] were meant to represent an almost perfect account of the bases for present day mathematics, this is now the perfect basis; the author is sufficiently representative of the mathematical community to make such a claim quite

> close to the truth. Furthermore, in a time in which indiscriminate use of science and technology threatens the future of the human race, or at least the future of what we now call civilization, it is surely essential that a well integrated report about our mathematical endeavors be written and kept for the use of a later day 'Renaissance'. As Thucydides said about his 'History of the Peloponesian War', this is ... a treasure valuable for all times. (Samuel 1972)

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 specific disciplines such as algebra, topology and set theory, as well as the relation between previously existing texts for those disciplines and what Bourbaki did in the relevant volumes. The following section discusses the centrality of the idea of a mathematical structure for the Bourbakian image of mathematics, and its relation with the technical contents of the *Éléments*. A final section discusses the conflict that aroused in the mid 1950s within the group around the question of whether or not to adopt the language of categories and functors as a general, unifying language of mathematics and of the topics presented in the treatise.

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. The founding members of the group included Henri Cartan (1904-), Claude Chevalley (1909-1984), Jean Coulomb (1904-1999), Jean Delsarte (1903-1968), Jean Dieudonné (1906-1992), Charles Ehresmann (1905-1979), Szolem Mandelbrojt (1899-1903), René de Possel (1905-1974) and André Weil (1906-1998). Cartan, Chevalley, Delsarte, Dieudonné, and Weil–all former students of the *École Normale Supérieure* in the early 1920s–remained the most influential and active force within the group for decades to come. Jacques Leray (1906-1998) and Paul Dubreil (1904-1994) had attended the meetings leading to the creation of the group but then did not join in. Over the years, many younger, prominent mathematicians participated in the group's activities, while the elder members were supposed to quit at the age of fifty. Among younger-generation Bourbaki members the most prominent include: Samuel Eilenberg (1913-1998) (one of the few who was not a Frenchman), Laurent Schwartz (1915-2002), Armand Borel (1923-2003), Jean-Pierre Serre (1926-), Serge Lang (1927-2005), Alexander Grothendieck (1928-), Pierre Samuel, Pierre Cartier (1932-), and Jean-Louis Verdier (1935-1989). All members were among the most prominent of their generation, actively pursuing separately their own individual research in different specialties, while the activities of Bourbaki absorbed a part of their time and energies.[4]

By the early 1930s, the would-be 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. Starting in 1933, Weil and Cartan were colleagues at Strasbourg for several years. In preparing their lectures, 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: Jacques Hadamard (1865-1963), Emile Picard (1856-1941),

[4] Besides the relevant secondary sources specifically referred to in this article, the interested reader may find general accounts of Bourbaki in (Chouchan 1995, Mashaal 2006). A full bibliography of secondary sources on Bourbaki is maintained and continually updated at (Beaulieu 2007).

and Edouard Goursat (1858-1936).[5] Goursat's treatise 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 the two young teachers at Strasbourg, Cartan and Weil. They were not alone in this predicament, to be sure. It also affected their one-time fellow students at the ENS, who were then teaching at other universities around France. As a matter of fact, this was part of a more general feeling common to all of them, that French mathematical research was lagging far behind that of other countries, especially Germany, as the war had taken a precious toll among an entire generation of French young mathematicians. 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 two weeks in Paris with their friends Chevalley (who finished his doctorate in 1933), Delsarte (then at Nancy), Dieudonné (then at Rennes) and de Possel (then at Clermont-Ferrand). The framework of the meeting was the 'Séminaire de mathématiques' held since 1933 at the Institut Henri Poincaré under the patronage of Gaston Julia (1893-1978). Julia had been severely wounded in his face during the war, losing his nose. He was the youngest among the professors of mathematics at the ENS, and he attended the seminar very often. Very often, in attendance was also his famous French colleague Henri Cartan's father, Élie Cartan (1869-1951). Occasional guests included foreigners like John von Neumann (1903-1957), Carl Ludwig Siegel (1896-1961), and Kurt Reidemeister (1893-1871). But the 'Séminaire Julia', as

[5] See (Dieudonné 1970, 136; Weil 1992, 99-100). On the French tradition of *Cours d'analyse*, based on lectures delivered by leading mathematicians see (Beaulieu 1993, 29-30).

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 of the state of the art. The topics covered along the years included, among others, groups and algebras, Hilbert spaces, topology, and variational calculus. In each meeting, one of the participants was commissioned with preparing a topic for discussion, editing the text of his talk, and then distributing it among the other participants. This approach would later develop into Bourbaki's famous *modus operandi* which I describe below in greater detail.

Over coffee after the meetings of the Séminaire Julia, Weil started to discuss with his friends an ambitious initiative to be pursued collectively, and aimed at producing the much needed new text in analysis. On 10 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'. 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. Similarly, several other ideas were suggested at this meeting, concerning the proposed plan of action: sub-committees should be nominated to be in charge of the various parts of the treatise; an agreed syllabus of a book should be ready by the summer of 1935; the length of the treatise should be of about one thousand pages; all decisions should be taken by consensus and without a designated expert leadership in any specific topic. 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 on 14 January 1935. This time detailed minutes were taken by Delsarte (Delsarte 2000). Delsarte would continue to fulfill this task in the future meetings of Bourbaki until 1940. It was decided that the committee would comprise nine members, and three new names were added: Dubreil, Leray and Mandelbrojt. Dubreil and Leray, however, would not remain for long in the project. Instead Ehresmann and Coulomb would soon join.

The minutes of this second meeting indicate that Delsarte and Dubreil presented a list of topics they would want to see treated in the projected treatise: modern algebra; integral equations with special emphasis on the Hilbert space; the theory of partial differential equations with a view to the more recent developments; and a long section devoted to special functions. Mandelbrojt, in turn, rather than raising specific topics, brought forward a principle that he considered of the utmost importance for the success of the entire undertaking: whenever a result is intended for discussion in full generality–he stated–the general theory needed to prove this result will never be developed in the course of the exposition itself. Rather, all the general, abstract theories will 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:

> It is necessary to eliminate a certain prejudice of rigor from the souls of some mathematicians and of almost all physicists. Many physicists calculate integrals, summations of series, and other calculations that yield exact results, with an intimate conviction of being continually incurring in mathematical heresies. This derives from the

> fact that in most classical texts the fundamental theorems … are presented with impressive luxury of precautions. The assumptions required are in many cases exaggerated. It will be necessary, in many cases, to reconsider all these theorems. It is important to provide the readers with a collection of tools, as robust and as universal as possible. It is the principle of utility and convenience which must be used as guide. It goes without saying that the committee is the only judge of what is useful and convenient for the people. Like Cartan said concisely, this is a principle of the 'enlightened despotism'. (Delsarte 2000, 17)

Pursuing this train of ideas, Weil expressed his opinion about the most adequate way to deal, for instance, with special functions: they should appear only in the framework of the applications of general theorems or principles, rather than as an independent topic to be developed in itself. Likewise, as another example of pursuing a very general point of view, the real integration to be presented in the treatise–integration 'tout court'– would be Lebesgue integration. The Riemann integral, Weil asserted, works well for continuous functions but if often fails. It is noteworthy, however, that when the project eventually materialized, it contributed more than any other contemporary mathematical textbook to promote (both within and outside mathematics) an image of the field very much along the lines against which Weil warned in these early meetings, especially on the matter of eliminating 'a certain prejudice of rigor'.

After several preliminary encounters in Paris about every two weeks, 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 3200 pages and it was planned to be completed within a year. Side by side with a necessary treatment of the classical themes of analysis, increased attention was devoted to a limited number of chapters devoted to the basic notions of algebra, topology and the theory of sets. These now appeared as necessary for providing 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 most fascinating mathematical endeavor that produced the multi-volume *Éléments de mathématique*. Its scope, structure and contents went much beyond the initial plans of the group and, by and large, beyond their initial assumptions about the amount of work that it would requite. Except for a break during the years of the war, over the following decades the group (in its changing membership) continued to organize 'congresses', meeting three times a year at different places around France for one or two weeks. Minutes of these Bourbaki congresses were taken and circulated among members of the group in the form of an internal bulletin initially called '*Journal de Bourbaki*' and since 1940 '*La Tribu*'.[6] Although the contents of the issues of *La Tribu* abound 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. The drafts were then subjected to harsh criticism by the other members, and then reassigned for revision. Only after several drafts had been written and criticized was the final document ready for publication. Later in his life Cartan described the group's *modus operandi* in the following terms:

> We often disagreed, we often had big arguments - but we remained good friends. For each subject, a 'rédacteur' was appointed. Later, his rédaction was read aloud and thoroughly examined. The next 'rédacteur' was given the appropriate instructions, and so on. For each

[6] For details on the Bourbaki archives and the issues of *La Tribu* quoted here, see (Corry 2004, 293, n. 13; Krömer 2006, 156-158). Direct quotes are from the personal collection of Professor Andrée Ch. Ehresmann, Amiens, and with her permission. Other issues are quoted indirectly as indicated.

> chapter there could be up to nine rédactions. But in the end, everybody was fatigué - tired. And Dieudonné would say, 'It is finished now. I shall write the last rédaction.' Which he did. And eventually, although it seemed to be impossible to reach a complete agreement, there was an agreement. But it took time. It is perhaps not the best way in terms of teamwork, but that was the way we took. (Jackson 1999, 784)[7]

Each chapter and each volume of Bourbaki's treatise was the outcome of arduous collective work and the spirit and point of view of the person or persons 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. A full analysis of this is beyond the scope of this chapter. For many, the most surprising fact related to Bourbaki is that is could work, to begin with.

What was initially projected as a modern textbook for a course of analysis eventually evolved into a multi-volume treatise entitled *Eléments de Mathématique*, each volume of which was meant to contain a comprehensive exposition of a different mathematical sub-discipline. 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 discipline would certainly embody a novel, overall conception of what is mathematics and, above all, underlying unity would be stressed. The 'paquet abstrait', initially conceived as a supporting toolbox of limited scope, gradually took central 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.[8]

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[7] For additional testimonies, see (Schwartz 2001, 155-163).

[8] It is worth pointing out that 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 it was Dieudonné who was in charge of this

The first published chapter of the *Eléments* appeared in 1939. By this time the plan had settled around the writing of 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 that came in the 1950s, additional chapters were added including the following: Lie Groups and Lie Algebras; Commutative Algebra; Spectral Theories; Differential and Analytic Manifolds (which is essentially no more than a summary of results). The treatise came in its final form comprised a large collection of more than seven thousand pages, with new chapters continuing to appear until the early 1980s.

In the decades following the founding of the group, Bourbaki's books became classic in many areas of pure mathematics in which the concepts and main problems, the nomenclature and the peculiar style introduced by Bourbaki were adopted as standard. The branches upon which Bourbaki exerted the deepest influence were algebra, topology and functional analysis and they became the backbone of mathematical curricula and research activity in many places around the world. Notations such as the symbol ∅ for the empty set, and terms like *injective*, *surjective*, and *bijective* owe their widespread use to their adoption in the *Éléments de mathématique*.

Disciplines like logic, probability, and most fields of applied mathematics were not within the scope of interests of Bourbaki and they were therefore hardly represented in the many places in the world where Bourbaki's influence was more strongly felt. This was the case for many French and several American universities at various times between 1940 and 1970 (Schwartz 2001, 162-164). Still Bourbaki influenced in various ways fields like economics (Weintraub & Mirowski 1994), on the one hand, and, especially in France, anthropology and even literature (Aubin 1997), on the

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choice. See, for instance, (Kaplansky 1953). In fact, as implied in Cartan's quotation above, for many years Dieudonné was the official scribe of the project and 'every printed word came from his pen'. See (Senechal 1999, 28).

other hand. In addition, disciplines like group theory and number theory, in spite of being strong points of some of the members (notably Weil for number theory) would not be treated in the *Éléments,* mainly for their character as mathematical disciplines less amenable to the kind of systematic, comprehensive treatment typical of the fields treated in the collection. As part of an underlying, basic tendency to estrange themselves in the treatise from visual elements, geometry was completely left out of the Bourbakian picture of mathematics, except for what could be reduced to linear algebra.

Bourbaki's extremely austere and idiosyncratic presentation of the topics discussed in each of the chapters–from which diagrams and external motivations were expressly excluded–became a hallmark of the group's style and a main manifestation of its thorough influence. Also the widespread adoption of approaches to specific question, concepts, and nomenclature promoted in the books of the series indicate the breadth of this influence. Concepts and theories were presented in a thoroughly axiomatic way and systematically discussed always going from the more general to the particular, and never generalizing a particular result. A noteworthy consequence of this 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.

Writing a Textbook on Modern Algebra

As mentioned above, one of the declared aims of the group was that their projected new treatise of analysis 'will be as modern as possible'. Most likely, the word 'modern' referred in their minds to the current trends of German mathematical research in general, but more specifically, and above all, to one recently published book, the epoch-making *Moderne*

Algebra, published in 1930 by Bartel L. van der Waerden (1903-1996) (van der Waerden 1930). When trying to understand the roots of the *Éléments* and the image of mathematics embodied in it, it is necessary to consider van deer Waerden's textbook, which can easily be singled out as the most important individual influence behind the entire project. A brief account of the process leading to its publication illuminates the historical context within which Bourbaki's desire to pursue a 'modern approach' in a new textbook of analysis should be understood.

Moderne Algebra represented the culmination of a deep transformation process underwent by the discipline of algebra between the last third of the nineteenth century and the first third of the twentieth, and which comprised the addition of important new results, new concepts and new techniques, but also profound changes in the very way that the aims and scope of the discipline were conceived by its practitioners. During the nineteenth century, algebraic research had meant mainly research on the theory of polynomial equations and the theory of polynomial forms, including algebraic invariants. The ideas implied by the works of Évariste Galois (1811-1832) had become increasingly visible and central after their publication in 1846 by Joseph Liouville (1809-1882). Together with important progress in the theory of fields of algebraic numbers, especially in the hands of Leopold Kronecker (1823-1891) and Richard Dedekind (1831-1916), they gave rise to an increased interest in new concepts such as groups, fields and modules. This development was only gradually reflected in the existing textbooks. The body of algebraic knowledge embodied in them continually added the new concepts and the results associated with them, but the overall image of the discipline remained very much the same until 1930.

Thus, for instance, a very popular textbook of algebra from the middle of the century was the *Cours d'algèbre supérieure* by Joseph Serret (1819-1885), which underwent three editions in 1849, 1854, and 1866, respectively (Serret 1849). In these successive editions, this book gradually incorporated the techniques introduced by Galois, and in the third, it became the first university textbook to publish a full exposition of the theory. Still, it continued to formulate the main results of Galois theory in the traditional language of solvability dating back to the works of Lagrange and Abel at the beginning of the century. Thus, it did not even include a separate discussion of the concept of group. A second important, contemporary textbook was the *Traité des substitutions et des équations algebriques* by Camille Jordan (1800-1888) (Jordan 1870). It included a more elaborate presentation of the theory of groups, but still treated this theory as subsidiary to the main tasks of algebra, and above all to the elucidation of solvability conditions for polynomial equations.

Towards the end of the century, Heinrich Weber (1842-1913) published a three-volume textbook, *Lehrbuch der Algebra* (Weber 1895), that incorporated an entire body of new ideas and techniques developed in the nineteenth century, thereby providing a full picture of what the body of algebraic knowledge looked like at the time. Concurrently, it implicitly embodied in the most elaborate and detailed way to date the disciplinary conception of algebra over the century. It laid down the main aims of this discipline, stressed the most relevant questions that practitioners had and should address, and presented the main techniques available to do so successfully. And yet, in spite of the great amount of specific knowledge it added over books like Serret's or Jordan's, Weber's *Lehrbuch* did not embody an overall disciplinary conception that essentially differed from theirs: algebra was still seen here as the discipline of polynomial equations and polynomial forms. Abstract concepts such as groups, in so far as they appeared in the book, were subordinate to the main classical tasks of algebra. And, most importantly, all the results assumed a thorough knowledge of the basic properties of the systems of rational and real numbers: these systems were conceived as conceptually prior to algebra. Whatever was said about polynomials or about factorization properties of algebraic numbers was based on what was known about the various systems of numbers.

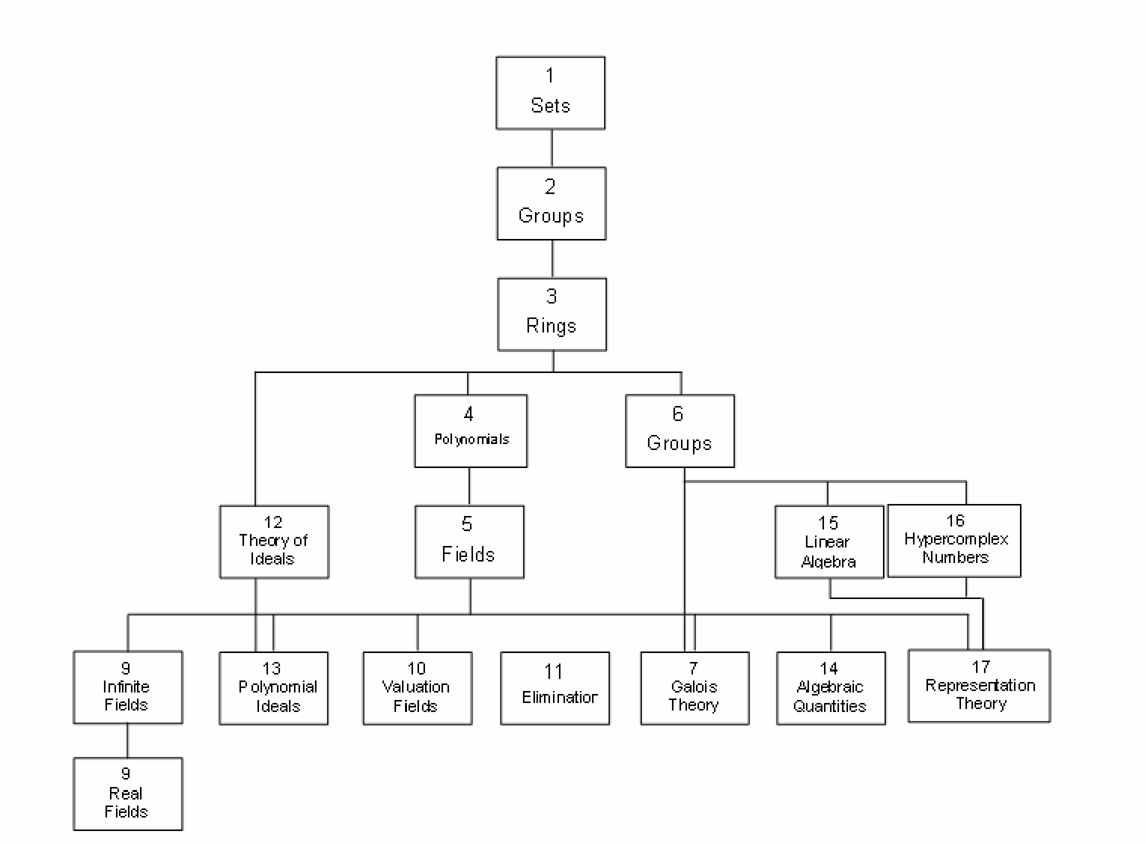
The first two decades of the twentieth century were ripe with new algebraic ideas. Towards the end of the 1920s, one finds a growing number of works that can be identified with only recently consolidated theories, usually aimed at investigating the properties of abstractly defined mathematical entities now seen as the focus of interest in algebraic research: groups, fields, ideals, rings, and others. Like many other important textbooks, *Moderne Algebra*, appeared at a time when the need was felt for a comprehensive synthesis of what had been achieved since the publication of its predecessor, in this case Weber's *Lehrbuch*. It presented ideas that had been developed earlier by Emmy Noether (1882-1935) and Emil Artin (1898-1962)–whose courses van der Waerden had recently attended in Göttingen and Hamburg, respectively – and also by other algebraists, such as Ernst Steinitz (1871-1928), whose works he studied under their guidance (van der Waerden 1975).

Van der Waerden masterfully incorporated a great deal of the important innovations accumulated over the early decades of the twentieth century at the level of the body of algebraic knowledge. But the originality and importance of this book is best recognized by focusing on its totally new way of conceiving the discipline. Van der Waerden presented systematically those mathematical branches then related to algebra, deriving all the relevant results from a single, unified perspective, and using similar concepts and methods for all those branches. The image of algebra put forward in van der Waerden's textbook was based on the realization that a certain family of notions (i.e., groups, ideals, rings, fields, etc.) are, in fact, individual instances of one and the same underlying idea, namely, the general idea of an algebraic structure, and that the aim of research in algebra is the full elucidation of those notions. None of these notions, to be sure, appeared as such for the first time in this book. Groups, as noted, had appeared in mainstream textbooks on algebra as early as 1866, in the third edition of Serret's *Cours*. Ideals and fields, in turn, had been introduced in 1871 by Dedekind in his elaboration of Ernst Edward Kummer's factorization theory of algebraic numbers. But the unified treatment they were accorded in *Moderne Algebra*, the single methodological approach adopted to define and study each and all of them, and the compelling, new picture it provided of a variety of domains that were formerly seen as only vaguely related, all these implied a striking and original innovation.

One fundamental innovation implied by van der Waerden's approach was an implicit redefinition of the conceptual hierarchy underlying the discipline of algebra. Under this image, rational and real numbers no longer have conceptual priority over, say, polynomials. Rather, they are 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 (1901-1926), 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 in van der Waerden's book for the first time to a subsidiary role. 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) are presented here merely as final products of a series of successive algebraic constructs, the 'structure' of which was 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 important innovation implied by the book concerns the particular way in which the advantages of the axiomatic method were exploited in conjunction with all other components of the structural image of algebra, such as those mentioned above. Once one has realized that the basic notions of algebra (groups, rings, fields, etc.) are, in fact, different varieties of a same species ("varieties" and "species" understood here in a "biological," and not mathematical sense), namely, different kinds of algebraic structures, the abstract axiomatic formulation of the concepts becomes, in a natural way, 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, with the idea of isomorphism fundamentally underlying the feasibility of this kind of pursuit. In fact, this fundamental recognition appears in *Moderne Algebra* not only implicitly, but rather explicitly and even didactically epitomized in the *Leitfaden* that appears in the introduction to the book, and that pictures the hierarchical, structural interrelation between the various concepts investigated in the book.



Obviously, the new image of algebra presented by van der Waerden reflected the then current state of development of the body of algebraic knowledge. However, the important point is that the image was *not* a *necessary* outcome of the body, but rather an independent development of intrinsic value. This becomes clear when we notice that parallel to van der Waerden's, several other textbooks on algebra were published which also contained most of the latest developments in the body of knowledge, but which essentially preserved the classical image of algebra. Examples of these are Leonard Eugene Dickson's *Modern Algebraic Theories* (Dickson 1926), Helmut Hasse's *Höhere Algebra* (Hasse 1926) and Otto Haupt's *Einführung in die Algebra* (Haupt 1929). But perhaps the most interesting example in this direction is provided by Robert Fricke's *Lehrbuch der Algebra* (Fricke 1924), published in 1924, with the revealing subtitle: "*Based on Heinrich Weber's Homonimous Book.*" (*Verfasst mit Benutzung vom Heinrich Webers gleichnamigem Buche.*) These books were by no means of secondary importance. Dickson's, for instance, became after its publication the most advanced algebra text available in the United States and it was not until 1941 that a new one, better adapted to recent developments in algebra and closer to the spirit of *Moderne Algebra*, was published there: *A Survey of Modern Algebra* by Garrett Birkhoff (1911-1996) and Saunders Mac Lane (1909-2005) (Birkhoff and Mac Lane 1941).

The main idea embodied in van der Waerden's book–the structural conception of algebra–became highly influential for the members of Bourbaki. Before receiving his doctorate in Paris under the supervision of Hadamard in 1928, Weil visited Göttingen where he came under direct contact with Noether and some of her collaborators. This visit left a significant imprint in the young mathematician which reverberated through the centrality later accorded to modern algebraic approaches as a unifying perspective in the *Éléments*. Of course, also Bourbaki's volume on *Algebra* (hereafter *A*) is closely modeled in many respects after *Moderne Algebra*. But the pervasive influence of the book is much broader than that, as I will argue below. Before seeing that, however, it is convenient to comment on the way that a second pillar of the treatise, topology, was discussed and eventually turned into Book III of the *Éléments*, with some very important parts of the discipline not being included in it.

Topology

The background to the writing of Bourbaki's book on *General Topology* (hereafter *GT*) was different to that of *A*, if only because of the different historical processes of developments of the two disciplines involved. Indeed, whereas the recent development in algebra had led in 1930 to a new conception of the aims, tools and scope of the discipline, embodied in van der Waerden's book, in the case of topology we can speak of the actual consolidation of an essentially new discipline over just four preceding decades. Although some of the basic, relevant ideas can be traced back to Riemann and even earlier to Leibniz, it was not until the late nineteenth century that Jules Henri Poincaré (1854-1912) published a series of six memoirs on 'Analysis Situs', where the basic ideas of homology and the fundamental group were systematically treated for the first time. Also after 1900, the gradual consolidation of set theory into an autonomous, well-defined discipline with its own set of open problems and tools concomitantly led, in the hands of Arthur Schoenflies (1853-1928) and Luitzen E.J. Brouwer (1881-1966), to the emancipation of point-set theory as a framework for the study of questions related to simple closed curves and dimension (see Ferreirós 1999). The most general of the early conceptual frameworks for the subject was provided by Felix Hausdorff (1868-1942) (see Purkert 2002). And yet, most authors still considered these questions as part of a general theory of sets.

Several books appeared between 1906 and 1930 that presented various aspects of the newly emerging discipline of topology in more or less systematic and more or less elementary ways (for instance, (Young & Young 1906), (Hausdorff 1914), (Fréchet 1928), (Veblen 1922), (Lefschetz 1930), (Moore 1932), (Kuratowski 1933)). Still, if two textbooks in this domain can be compared to van der Waerden's, in terms of what the latter did for algebra, these were–each in its own way–*Lehrbuch der Topologie* by Herbert Seifert (1907-1996) and William Threlfall (1888-1949) (Seifert & Threlfall 1934) and *Topologie* by Pavel S. Alexandroff (1896-1982) and Heinz Hopf (1894-1971) (Alexandroff & Hopf 1935). These four mathematicians spent study periods in Göttingen where they were deeply influenced by the local mathematical culture and, above all and in a very direct way, by Emmy Noether. In both books, this influence is clearly manifest alongside that of Brouwer. Also: Weil met Alexandroff in 1927 at Göttingen and became directly acquainted with his ideas.

When the young participants of the Séminaire Julia devoted the year 1935-36 to the study of topology, they had used precisely these two books. Alexandroff and Hopf's was the reference book of choice for Weil when he presented the application of homological invariants to the characterization of classes of representations, intersection numbers and topological degrees. Also de Possel used this text when he talked about fixed-point theorems. Seifert and Threlfall's text was used in the discussion of coverings and the fundamental group. The clarity and comprehensiveness with which the two books presented the elements of homology theory had the effect of preempting the need for reference to either Poincaré's texts or any of the other books published prior to the two (except for Lefschetz's who was also mentioned in discussions) (see Herreman 2004).

The first two chapters of *GT* were published in 1940 following almost four years of the usual procedure of drafting and criticism. They dealt, respectively, with 'Topological Structures' and 'Uniform Structures'. While the two textbooks mentioned above obviously influenced the approach followed in the two first chapters of *GT*, a much more general perspective dominated the presentation, and it was largely based on ideas independently developed by Cartan and Weil. *GT* was meant to provide the conceptual basis needed for discussing convergence and continuity in real and complex analysis. The group's thus attempted to define this conceptual basis within the most general framework possible, avoiding whenever possible the need to rely on the traditional, most immediately intuitive concepts such as sequences and their limits. This effort helped understanding, among others, the centrality of compactness in general topology (Mac Lane 1987; 166, Mac Lane 1988, 337). It also yielded a thorough analysis of the various alternative ways to define general topological spaces and their central characteristic concepts: open and closed sets, neighborhoods, uniform spaces. Indeed, in his own research on topological groups Weil showed that the metric plays only a secondary role in defining the main topological concepts. He thus developed an alternative approach based on uniform spaces as the adequate conceptual framework for this purpose (Weil 1937; Weil 1978, 538). Moreover, an important by-product of Bourbaki's discussions was the introduction of filters and ultrafilters as a basis for defining convergence while avoiding reliance on countable sequences. Bourbaki, however, rather than including these latter concepts in the treatise, encouraged Cartan to publish them under his own name, while elaborating on their relation to topological concepts (Cartan 1937a, 1937b).[9]

Over the next years alternative approaches to questions of continuity and convergence were developed by other mathematicians, based on concepts such as directed systems and nets. The equivalence of the various alternative systems and those of Bourbaki was proved in (1955) by Robert Bartle. Thus, the history of the development of topology, at least from 1935 to 1955, cannot be told without considering in detail the role played in it by both Bourbaki as a group and its individual members.

As the various chapters of the books were repeatedly discussed and tasks assigned and reassigned to members of the group, not only specific issues were considered, but also, time and again, the overall organization of the general picture of analysis that the treatise was meant to present. It is noteworthy that in the various attempted versions of the general table of contents chapters on matters related directly related with traditional core aspects of analysis (such as Elementary Techniques of Infinitesimal Calculus; Integration; Differentials; Calculus of Variations; Analytic Functions), that were suggested but never actually written, gradually disappear in favor of more abstractly oriented, and mainly topological disciplines (such as: Topological Vector Spaces). As the

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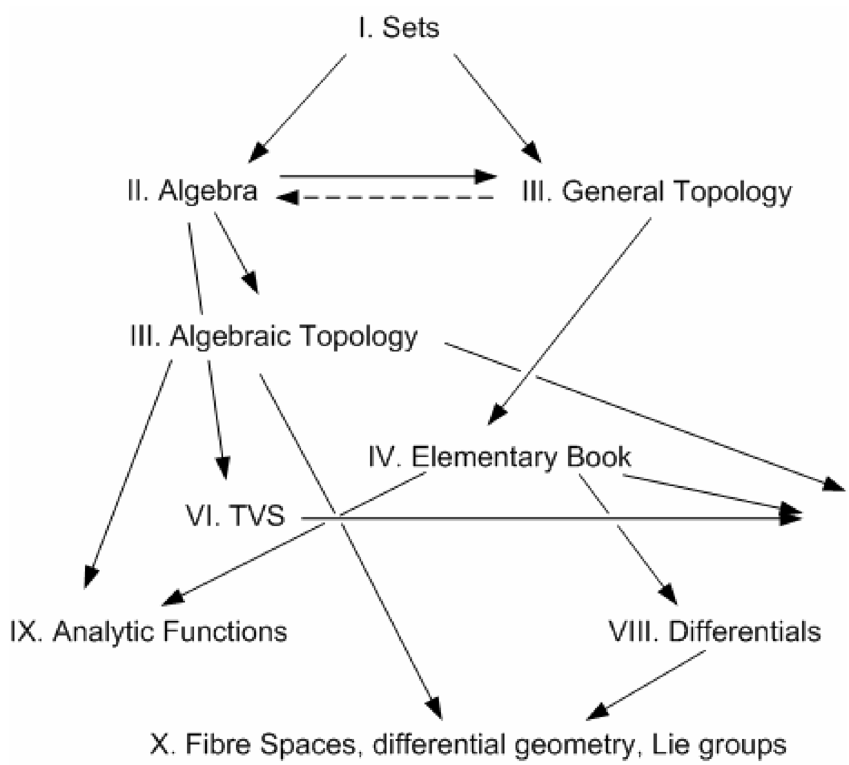
9 For the debates leading to the publication of Bourbaki's book on topology, see (Beaulieu 1990, 39-41).

treatise continued to materialize over the coming decades, one only finds two books the can be said to relate directly to the themes of classical analysis: Functions of a Real Variable and Integration.

Also the brake in the activities of the group brought about by the war had a sensible influence on reformulating much of the overall conception of the treatise (Beaulieu 1994, 249-251). When the group started to reconvene in 1945, the picture of what should be the interconnections among the various parts of the treatise was represented as follows:[10]



Algebraic topology appears here as a fourth book in the series. Debates around the main topics of this field arose since the early meetings of the group. In 1946 Eilenberg joined the group and prepared together with Weil a very detailed report that covered significant portions with great

[10] For additional details see McCleary (2007), who bases his account on, and cites, various relevant issues of *La Tribu*.

authoritativeness. But in the end no such section appeared, and this in itself sheds interesting light on the way that topics were considered, discussed and sometimes discarded from the overall picture presented. Thus for instance, one may notice that the focus of the enormous interest that this discipline aroused in members of the group moved away from the treatise and the debates surrounding it to a new seminar started in Paris by Cartan in 1948-49 after his return from a brief stay at Harvard. This seminar was in a sense the revived version of the Séminaire Julia and it would eventually turn into the famous 'Séminaire Bourbaki'. Given the current expansion and rapid level of development of the algebraic topology, the highly dynamic seminar turned out to be a much more adequate place for the continued discussion of the related ideas than a treatise intended as a textbook that should continue to be relevant for decades to come.

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 Bourbaki 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 long-standing 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 to this, however, was Chevalley for whom foundational questions were, especially in the early part of his career, a matter of direct interest. In fact, in his student years at the *ENS* Chevalley developed a strong friendship with the early-deceased Jaques Herbrand (1908-1931), and the two shared a strong interest in logic. Only later Chevalley moved to those topics in which he made his more important contributions: class field theory, group theory, algebraic geometry and the theory of Lie algebras (Dieudonné & Tits 1987).

Chevalley was, at any rate, the most active force behind the inclusion of a separate book on set theory as part of the evolving plan for the contents of the *Éléments*. An issue of *La Tribu* in 1949 reported on this matter, and in doing so it pointed to the underlying discussions around one of the main questions that occupied the Bourbaki project from the beginning, namely, 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. As already stated, Bourbaki's book came to be considered as the epitome of extreme formalization in twentieth-century mathematics, whereby topics are presented with no heuristic or didactic concessions to the reader. And yet, there is plenty of evidence that discussions repeatedly arose around the exact way to present many individual topics or theories. This was clearly the case with sets:

> Since the first session, Chevalley raised objections concerning the notion of a formalized text, which threaten to hinder the whole publication. After a night of contrition, Chevalley turned to more conciliatory opinions and it was agreed that there are serious difficulties to it, which he was assigned to mask as unhypocritically as possible in the general introduction. A formalized text is in fact an ideal notion, since one has seldom seen any such a text and in any case Bourbaki has none. One should therefore speak with discretion about those texts in chapter 1 and indicate clearly in the introduction what separates us from them. (*La Tribu*: 13-25 April 1949)

Similar debates continued to appear and the publication of this book was constantly delayed. In the final account, the contents of *ST* turned out to be a compromise between the attempt to fully formalize the topic and explore it in detail, as demanded by Chevalley also in this case, 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 indeed adopted as a universal language underlying all mathematical domains because of its unifying capabilities, which were fully acknowledged only recently:

> ... whereas in the past it was thought that every branch of mathematics depended on its own particular intuitions which provided its concepts and primary truths, nowadays it is known to be possible, logically speaking, to derive practically the whole of mathematics from a single source, the theory of sets. (Bourbaki 1968, 9)

But on the other hand, this very basic theory was not presented in a truly formalized language, as Bourbaki acknowledged that while no mathematician actually works in a fully formalized language, '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)'. Within the entire treatise, only in *ST* one finds explicit statements like this.

Also the question of the consistency of set theory arises here in an interesting way. Bourbaki did not attempt to address the question heads-on as part of the discussion but rather reverted to a strongly empiricist position in connection with it. Thus, in a quite ironical turn, Bourbaki simply stated that a contradiction is not expected to appear in set theory because it has not appeared after so many years of fruitful research (Bourbaki 1968, 13). In one of Bourbaki's earlier publications it was 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).[11]

A summary of results on set theory was published as early as 1939, more properly entitled in the French original *Fascicule de résultats*. The actual chapters of the volume were published only during the 1950s, comprising the following: 1. Description of Formal Mathematics; 2. Theory of Sets; 3. Ordered Sets, Cardinals, Integers; 4. Structures. It was in this fourth chapter that Bourbaki introduced the new concept of *structure*,[12] 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 $E_1,E_2,...,E_n$, 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 $\boldsymbol{B}(E)$. For example, beginning with the sets E, F, G the outcome of one such procedure could be: $\boldsymbol{B}(E)$; $\boldsymbol{B}(E)\times F$; $\boldsymbol{B}(G)$; $\boldsymbol{B}(\boldsymbol{B}(E)\times F)$; $\boldsymbol{B}(\boldsymbol{B}(E)\times F)\times\boldsymbol{B}(G)$ 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 $\boldsymbol{B}((A\times A)\times A)$ and then choose from all the sub-sets 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*. In a similar way, Bourbaki showed in this chapter 4 of *ST* how the general

[11] Imre Lakatos (1978 Vol. 2, 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 & Radice 1976, 175-176).

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

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 to be described below, the most problematic one.

The *Fascicule de résultats* is strikingly different from the chapters themselves. The reader simply finds here 'all the definitions and all the results needed for the remainder of the series'. Whereas the book's stated aim is to show that it is possible to provide a sound, formal basis for mathematics as a whole, can be given, 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 follows:

> 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 2-3 is reduced here to 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' (p. 347. Italics in the original). And a footnote explains further:

> The reader will not fail to observe that the 'naïve' point of view taken here is in direct opposition to the "formalist" point of view taken in chapters I to IV. Of course, this

> contrast is deliberate, and corresponds to the different purposes of this Summary and the rest of the volume.

As for *structures,* the whole formal development is reduced in the *Fascicule* to a very short, intuitive explanation of the concepts in which the main ideas are explained. The only important concept associated with *structure* which is mentioned, is that of isomorphism.

As already mentioned the *Fascicule* first appeared in 1939, whereas the first editions of each of the four chapters appeared (in French) only between 1954 and 1957. This interval saw many important developments in mathematics and, in particular, the emergence of category theory about which more is said below. As a consequence of these developments, some of the ideas that perhaps looked very promising at the time of presenting the *fascicule* as a blueprint for a conception to be fully elaborated later on, soon became obsolete. Thus, *ST*, and especially its chapter on *structures*, became one of the less interesting of the entire collection. As a textbook for the discipline, it received little attention and, contrary to what was the case in other mathematical disciplines, very few of the concepts and notations introduced by Bourbaki were widely adopted. As Paul Halmos critically 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) [13]

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

Two Meanings of "Structure"

As work on the various volumes of the treatise developed throughout the years, an implicit, but definitely pervasive idea increasingly came to underlie the overall approach of the project. This was the conception of mathematics at large as a systematic, elaborate hierarchy of structures. This was, essentially, an extension of the idea that had initially emerged in the more circumscribed realm of algebra from van der Waerden's textbook. Van der Waerden had undertaken the 'structural' investigation, from a unified point of view, of several concepts that were defined in similar, abstract terms (groups, rings, ideals, modules, fields, hypercomplex systems) while asking similar kinds of questions about each of them and using similar kinds of tools to investigate them. Now, in Bourbaki's texts, different mathematical branches such as algebra, topology, functional analysis, started to appear as individual materializations of one and the same underlying, general idea, i.e.,

[13] For a detailed review of Chapters 1-2 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).

the idea of a mathematical structure. The meaning of this is, briefly said, that Bourbaki attempted to present a unified an 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 distant mathematical domains.

In 1950 Dieudonné published, signing with the name of Bourbaki, an article that came to be identified as the group's manifesto, "The Architecture of Mathematics". Faced with the unprecedented growth and diversification of knowledge in the discipline over the preceding decades, Dieudonné raised once again the well-known question of the unity of mathematics. Mathematics is a strongly unified branch of knowledge in spite of appearances, he claimed, and now it is clear that the basis of this unity is the use of the axiomatic method as the work of David Hilbert had clearly revealed starting from the beginning of the century.[14] 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... Farther 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, 228-29)

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

Thus, the idea that van der Waerden applied successfully and consistently, but only implicitly so–namely the centrality of the hierarchy of structures–became now explicit and constitutive for Bourbaki. At the same time, as was seen above, an elaborate attempt was made in one of the chapter of the treatise to present a formal definition of a certain notion of *structure*, and this definition was somehow meant to provide a solid conceptual foundation on which the whole edifice of mathematics as presented in the *Éléments* could supposedly be built. Thus, two different meanings of the term mathematical 'structure' appeared in the Bourbakian discourse on mathematics, and 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, or 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 (as indicated below in some detail) was not even really used by Bourbaki in its own treatise.

As already stated, the main interest of most members of the group was in the various disciplines covered in the treatise and 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 also remarkable that in retrospect, 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 shared by many mathematicians and systematically pursued in the *Éléments*, that was so central to much of twentieth century mathematics. Thus, for instance, in Weil's autobiography we can read that:

> In establishing the tasks to be undertaken by Bourbaki, significant progress was made with the adoption of the concept of structure, and of the related notion of isomorphism. Retrospectively these two concepts seem ordinary and rather short on mathematical content, unless the notions of morphism and category theory are added. At the time of our early work these notions cast light upon subjects which were still shrouded in confusion: even the meaning of the term 'isomorphism' varied from one theory to another. That there were simple structures of group, topological space, etc., and then also more complex structures, from rings to fields, had not to my knowledge been said by anyone before Bourbaki, and it was something that needed to be said. (Weil 1992, 114)[15]

While here Weil left the meaning of the term "structure" ambiguous enough as to be understood in either way, Dieudonné (1979, 9) went as far as stating in one place that the evolution of mathematics for more than 200 years was a process whereby 'the study of specific mathematical objects has been replaced more and more by the study of mathematical *structures*', while adding in a footnote that the term 'mathematical structure' in this quote is to be taken *in the specific technical sense defined by Bourbaki in the fourth chapter of the first book of the Eléments*!

Bourbaki's theory of *structures* never received any real attention on the side of working mathematicians, and this includes Bourbaki's members when they were involved in their own research. But even when we look at the specific way in which the concept of *structure* was used as a general underlying idea for the various parts of mathematics considered in the treatise, all we see is that in the opening chapters of the books on specific branches, e.g., algebra and topology, some sections were devoted to show how the specific branch could, in principle, be formally connected

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[15] A similar statement appears in (Weil 1978, 537).

with the general concept of *structure*. This connection, however, was a rather feeble one and amounted to no much more than a formal exercise that was forgotten after the first few pages of the book before going further to present the theory in question. Thus, for instance, while vector spaces are presented in *A* as a special case of groups and, therefore, all the results proven for groups hold for vector spaces as well, this hierarchical relation is not presented at all 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 proven that ***Z***-modules and commutative groups are 'equivalent' *structures,* to take but two concepts. The *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 the subsequent theories in the hierarchy of algebraic structures, the connection with *structures* is only scarcely mentioned, if at all. Ironically, the need for a stronger unification framework was indeed felt in later sections. Such was the case, for instance, in Chapter 3 where three types of algebras defined over a given commutative ring are successively discussed: tensor-, symmetric- and exterior- algebras. Although a separate treatment is accorded to each type of algebra, this treatment nearly repeats itself in its details three times, one after the other. Thus Bourbaki defines each kind of algebra and then discusses, for each case separately: 'functorial properties', 'extension of the ring of scalars', 'direct limits', 'Free modules', 'direct sums', etc. (Bourbaki 1973, 484-522). This is worth mentioning not only because a unified presentation of the three could have been more economic and direct but especially because all the above mentioned issues lend themselves naturally to a categorical treatment and this possibility is not even mentioned here. 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.

As for *GT*, this 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 in no sense introduced in terms of the *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 in any other place 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 remaining books of Bourbaki's treatise rely mainly on concepts taken from *A* and *GT* and the concept of *structure* is totally absent from them. The most interesting one to look at, in this connection is *Commutative Algebra* (*CA*) consisting of seven chapters whose first editions appeared between 1961 and 1965. In this book the limitations of *structures* as a generalizing framework are interestingly manifest and, in fact, they are explicitly acknowledged in the discussion on 'flat modules'. This is a concept which is better understood in terms of concepts taken from homological algebra, a mathematical discipline which was not dealt with in the treatise until 1980. While it is often the case that when formally introducing concepts in a book of the treatise, Bourbaki illustrates those concepts by referring to an example which had not been yet introduced in that specific book, if the example is not a logical requisite for a full understanding of the concept itself and it appears in another place of the treatise, Bourbaki presents the example between asterisks and gives the corresponding cross-reference. In the case of flat modules, a whole section was included 'for the benefit of the readers conversant with homological algebra', in which Bourbaki showed 'how the theory of flat modules is related to that of the Tor functors' (Bourbaki 1972, 37). The concept of functor and the particular case of the Tor functor are not developed in the treatise, but Bourbaki thought it important to present the parallels between the two approaches: Bourbaki's own approach and the functorial approach to homological algebra. In order to do this, Bourbaki freely used concepts and notations foreign to the treatise. This is one of the few instances in the treatise where, instead of sticking to the usual notation between asterisks, Bourbaki gave a reference to a book or article outside it, which the reader could consult until the expected publication of a forthcoming volume of the treatise where categories, and in particular, Abelian categories would be treated.

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 of the entire discipline, that turned out to be very influential. On the other hand, it comprised a formal 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 in fact it was not even the only such attempt in which members of the group were involved.[16] Thus, in order to understand the full historical and mathematical context of the theory of *structures* and its role within the *Éléments*, it is necessary to discuss now the inevitable conflict created by the rise of its most serious competitor, the theory of categories.

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

The Categorical Imperative and its Demise

In the early 1940s, Eilenberg and Mac Lane, collaborating from their respective institutions at Columbia and Chicago, introduced the concepts of category and functor. Eilenberg would become an active Bourbaki member by the end of that decade. Mac Lane sporadically participated in meetings of the group throughout the years. These concepts and the general perspective they furnished gradually became a widely adopted unifying tool and language for mathematical disciplines that pursued a structural spirit similar to Bourbaki's. Groundbreaking, early instances thereof appear in the works of two younger-generation Bourbaki members, Grothendieck and Serre, who in the early 1950s used categories as the basic tools for their own original research in fields like homological algebra and algebraic geometry.[17] 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 serve as an underlying support for the unifying, structure-oriented perspective that the group had been striving after since its inception. Indeed, the idea was variously discussed at Bourbaki congresses but in the end it never materialized. In this section I discuss this very interesting chapter in the history of the ambitious attempt to write the ultimate mathematical textbook.[18]

If the categorical language were to be adopted by Bourbaki as a possibly unifying language for the various mathematical domains discussed in the *Éléments*, this would imply, in the first place, the need to reformulate considerable parts of the already existing chapters in order to make them fit the

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[17] (Krömer 2007, 117-190).

[18] In (Corry 1992) I called attention to the inherent tension between *structures* and categories, and published for the first time some illuminating, related documents (mainly, issues of *La Tribu*), some of which are also included here. More recently, Ralf Krömer (2006) has further developed this important point and has added significant insights to it, using heretofore unpublished material, part of which I am quoting hereinbelow.

new approach. A particularly obvious nuisance that would require focused attention in this regard concerned the chapter on *structures* in *ST*. As already mentioned, this entire chapter had a rather ad-hoc flavor to it, to begin with, and in any case it did not represent a main focus of interest for most members of the group. This signified in itself a meaningful obstacle to any attempt to incorporate categories into the treatise. It turns out, however, that additional obstacles came from other directions such as diverging views about the intrinsic value of the categorical approach in general. Weil, for one thing, explicitly and actively opposed the introduction of categories in any way into the *Éléments*. It is pertinent to show some of the existing evidence in this regard.

It was already suggested that some of the topics discussed in *CA* were presented in a manner for which the categorical formulation would be the most natural one, but without explicitly using such a formulation. This was also the case with other topics on which Bourbaki had already published by 1950 or would soon publish. A cursory examination of issues of *La Tribu* during the fifties uncovers recurring attempts to write chapters on homological algebra and categories for the *Eléments* and the discussions that ensued around them. Beginning as early as 1951, Eilenberg was commissioned several times to prepare drafts to be discussed. Eilenberg not only created the theory of categories together with Mac Lane: in the 1950s he collaborated in the publication of the first two books in which this language was systematically used to present, in an illuminating and innovative way, elaborate mathematical disciplines that emerged and were initially developed in completely different terms: algebraic topology (Eilenberg & Steenrod 1952) and homological algebra (Cartan & Eilenberg 1956). When it came to Bourbaki, however, he immediately realized the serious difficulties to be expected in attempting to do something similar in the context of the Bourbaki treatise because the latter had already introduced *structures*. In an undated, unpublished text possibly written around that time, he said this very explicitly:

> Here are some remarks concerning the possibility of including categories and functors in Bourbaki. … 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.[19]

Over the next few years, the younger-generation Bourbaki members increasingly adopted the categorical language for their own research, and accordingly they repeatedly attempted to introduce it to the *Éléments*. It is important to recall that by this time, *structures* had only been announced in the *Fascicle* of 1939 but the respective 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.

Various issues of *La Tribu* in the early 1950s bear witness to this kind of attempts. The participants in a congress of 1956, for instance, reported on 'a violent attack of the functorial virus'. *La Tribu* reproduces details of the discussion around *structures* and categories:

> Chap. IV (Structures) A paper by Cartier shows that Samuel's results on inductive limits are particular cases of ultra-general trinkets on the commutation of universal problems. These trinkets are well-stated only within the framework of categories and functors. Cartier

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

> proposes a metamathematical method to introduce the latter without modifying our logical system. But this system is vomited [sic] since it turns its back on the extensional point of view ... It was therefore decided that it will be better to enlarge the system in order to make room for the categories. … In order ... not to delay the publication of a chapter on which we have worked much, it is decided ... to send chapter IV to press without modifying the inductive limits, and introducing the slight modifications concerning strict solutions of universal problems.
>
> Regarding categories and functors, we are finally convinced of their importance. Therefore: Chap. V (Categories and functors): To begin with, Grothendieck will write down a kind of Summary of Results in a naive style, so that Bourbaki may realize what can be usefully done with it. Later on, it will be formalized. (*La Tribu*: 4 June – 7 July, 1956)

This train of events reached a high point around the publication of Grothendieck's famous Tohoku article (Grothendieck 1957), a central milestone in the history of category theory. Grothendieck innovatively applied here cohomological methods (full couched in the categorical language) to algebraic geometry, thus opening the road for thoroughgoing developments that would continue to engage for decades the efforts of mathematicians working in this field. The issues of *La Tribu* and the Serre-Grothendieck correspondence (Colmez & Serre (eds), 2001) during this time provide clear evidence that Grothendieck had conceived the text of his famous article as a possible contribution to the Bourbaki treatise. Grothendieck's functorial ideas were well received by most of the younger generation in the group and also by Dieudonné, but the continued opposition of others, especially Weil, did not allow for their actual adoption in the *Éléments*.

Evidence of Weil's skeptical views about categories dates back to as early as 1950. In a letter to Chevalley, dated 15 October 1950, that was distributed among the members of Bourbaki as an appendix to of the issues of *La Tribu*, Weil wrote:

> I have just received chapters 2 and 3 of Set theory ... Should the word "function" be reserved for mappings sending a set to the "universe", as you have done (in which case, with your axioms, the values of the function constitute themselves a set properly understood)? Or is it perhaps convenient to name "function" anything to which we attach a functional symbol, e.g., $P(E)$, $A\times B$, $A\circ B$ (tens. prod.) etc.? Obviously, "function" in the second sense will not be a mathematical object, but rather a metamathematical expression. This is undoubtedly the reason why there are people (without giving names ...) who use the word "functor." Should we accept this term? It seems that a word is needed for this notion.... Regarding the theory of structures, your chapter does illuminate the issue. However, we can hardly avoid going much farther than you have, and find out whether or not it is possible to give some generality to the notions of induced structure, product structure, homomorphism. As you know, my honorable colleague Mac Lane claims that every notion of structure necessarily implies a notion of homomorphism, which consists in indicating for each data constituting the structure, those which behave covariantly and those which behave contravariantly ... What do you think can be gained from this kind of considerations?

The chapter on *structures* came out in 1957, and it did not contain the slightest, explicit reference to categorical ideas. The incompatibility of both approaches and the work already invested were the main reason 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 IV of *ST* should replace the old one, 'unusable in all respects'.[20]

It is important to delineate more precisely the internal historical context within which this discussion was taking place. In the mid-1950s younger members (Serre, Grothendieck, Cartier, Borel, and others) had started to join the group. Naturally, and partly because of the influence of Bourbaki, the mathematical scene was by then in many ways different to that faced more than a quarter of the century earlier by the founding fathers. At the same time, the age of self-imposed mandatory retirement at fifty had arrived for the latter (but was not always strictly fulfilled). 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, from those of each other. Towards the end of the decade, the first six books of the *Éléments* had essentially been completed, thus covering much of what the group had come to consider as the hard core of the project. Now that the time had come for dealing with more advanced and specialized topics, and that the younger members wanted to have a say on the overall direction of the project, the possibility of universal participation in each topic and the original view that the writing of specific chapters should not be assigned to "specialists" was seriously reconsidered. Basic questions about the entire enterprise arose anew, thus eliciting conflicting views and sometimes personal tensions. The debate around the adoption of categories was part of this situation as was, in particular, the opposition between Grothendieck and Weil, two strongly opinionated mathematicians and difficult persons to deal with.

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[20] For the relevant sources and quotations, see (Krömer 2006, 144).

Indeed, Weil was a very dominant character whose mathematical prestige and overall intellectual personality, coupled with his authority as one of the leading forces in the creation and promotion of the Bourbaki project, bestowed upon him an undisputed, unique position within the group. The retirement of some prominent members along the years has been commonly attributed to conflicts or tension with Weil. That was certainly the case with de Possel, to whom Evelyn, Weil's wife since 1939, had been previously married. Tensions with Weil probably led to the Leray's and Lang's leaving the group. Weil had been the first to suggest that members should retire form active participation in the group at the age of 50, but ironically, on arriving at that age by 1956 he gave very little signs of wanting to diminish his influence on how the project would continue to develop.

Grothendieck, in turn, was a highly unconventional personality even by the standards of this bunch of rather unconventional individuals. He was born in Germany, and escaped the war to France. He remained an alien citizen, which created some difficulties to finding a position in his new country. In 1959, the IHES was created in Bures-sur-Yvette and Grothendieck got a research position. He spent twelve years in that institution, creating and teaching his revolutionary ideas. This were collected mainly in *EGA* (*Éléments de Géométrie Algébrique*) written with Dieudonné and in *SGA* (*Séminaire de Géométrie Algébrique*) written with his students. 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, similar to *EGA* but which much broader aims in mind. Not all members, however, approved of this. Many years later, 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) and initially circulated only within closed circles,[21] he referred to his special status within the group, while pointing to the underlying tension with Weil:

> … 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. … Sometimes I felt a tacit, stronger reservation in Cartan. During several years, I must have given him the impression of someone inclined to superficial and gratuitous generalization. … 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 *rébuffade*, or even a shadow of condescension, except once or twice from the part of Weil (evidently a very different case!). Cartan never departed from a constant kindness towards myself, imbued with cordiality as well as with a touch of humor so typical of him, which for me remains always associated with his personality. (*R&S* I, 142-143)

From Grothendieck's correspondence with Serre in 1956, it is quite evident that both mathematicians definitely disliked Weil's style, although they surely recognized the importance and originality of his ideas for their own concerns (Colmez & Serre 2001, 49-53). Writing

[21] Two useful websites containing digital versions of *Récoltes et semailles*, and additional material related with Grothendieck are found at http://math.jussieu.fr/~leila/grothendieckcircle/index.php and http://kolmogorov.unex.es/~navarro/res. All subsequent quotations of Grothendieck are taken from Part I, of *R&S*, 'Fatuité et renouvellement', and the pages refer to the original version of the text.

retrospectively about this period of time, Grothendieck did not fail to put matters in the right proportion, as he stressed the positive balance that he attributed to the project and to Weil's role. He thus wrote:

> Gradually, Serre came to exert on the group an influence comparable to that of Weil. During the time I was part of Bourbaki, this never gave rise to situations of rivalry between the two men, and I am not aware of any enmity that arose between them later on. From the vantage point of twenty years later, Bourbaki, such as I knew it during the fifties, continues to appear to me as an example of remarkable success at the level of the quality of the relations in a group constituted around a common project. This quality of the group appears to be of a unique essence even more unusual than that of the books they produced. … If I did not stay in the group, this has nothing to do with conflicts or with any degradation of the quality about which I spoke. Rather, it was because of personal tasks attracted me more strongly at the time, and to which I wished to devote the totality of my energy. This departure, moreover, cast no shadow either over my relations with the groups or over my relations with any of its members. (*R&S* I, 46)

As it happened, however, Grothendieck quitted the group around 1958-59 while some of the members, above all Serre, Schwartz and Dieudonné, continued to be close friends and collaborators. Later on, in 1970, he completely retired from public scientific life, as he discovered that IHES was partly funded by the military.

Schwartz, who directed Grothendieck's dissertation, had a simple explanation why the latter remained only few years in the group: 'he lacked humor and had difficulty accepting Bourbaki's criticism which his customarily rather virulent' (Schwartz 2001, 284). There is every reason to accept this explanation, and yet, there is also clear evidence that the non-adoption of category theory and Weil's attitude towards this question and towards Grothendieck were a main reason for the latter's decision to quit. Such evidence is found in a text by an anonymous author (possibly Lang) that was appended to one of the issues of *La Tribu* in the early 1960s. Under the title *Ad majorem fonctori gloriam*, it commented on the departure of Grothendieck, describing it as clear indications of a decline in the originally innovative spirit of the entire Bourbaki enterprise. This text supports the view that it was Weil, above all, who was behind these developments:

> 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 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 will be a great pity.[22]

The consequences of the debate around categories and *structures* continued to be felt for many years to come, and it is interestingly manifest in Bourbaki's book on Homological Algebra,

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[22] Quoted in (Krömer 2006, 152-153).

published in 1980 as a chapter 10 of *A*. The conceptual framework provided by categories had become the standard one for treating homological concepts ever since the publication Cartan and Eilenberg's famous textbook of 1952. In Bourbaki's presentation, however, these concepts are defined within the narrower framework of modules, since category theory had not been developed in the treatise. Using this language here would go against the most basic principles that had guided the enterprise since it inception. Thus, whereas Bourbaki's treatment of a field like general topology had embodied in the 1940s a truly innovative approach that many others were to follow, this would hardly be 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 last volume of the by now truly classical treatise.

Concluding Remarks

The Bourbaki project reached its high-point of success and influence during the 1960s. Many reasons converged to determine its gradual decline in the years to come, as the initial impetus that characterized the project in its initial years could not be maintained indefinitely. The role of Dieudonné as a catalyzing force could hardly be matched after his retirement. Some new chapters were proposed which never materialized, including on topics such as analysis of several complex variables, homotopy theory, spectral theory of operators, and symplectic geometry. That was also the case with plans to rewrite the first six books. The new books that did appear by 1980 included a summary on 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 also found itself involved in a legal dispute with its publisher, which absorbed a great amount of energies.

Partly because of the very success of the project and the impact it had on so many quarters of mathematics, the need for its continued development became a much less pressing matter. On the other hand, the name Bourbaki also started to elicit negative reactions and for many it represented a style that should be avoided, rather than emulated. It is interesting to consider in this regard how this 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 this in his memoirs:

> It was no doubt during the 1960s, that the 'tone' within Bourbaki slipped towards an increasingly pronounced elitism, of which I certainly was also responsible and which, for this very reason, I would not notice. 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 narrowminded dogmatism, of a cult of 'canonical' form at the expense of concrete understanding, of hermetism, of castrating anti-spontaneity and so on! Moreover, it was not only with the average mathematician [*dans le 'marais'*] that Bourbaki has a low reputation: during the sixties, and perhaps even earlier, I got occasional echoes from mathematicians with a different turn of spirit, who were allergic to the 'Bourbaki style'. (*R&S* I, 49)

Grothendieck also disapproved of the attitude of some of his colleagues (possibly he referred mainly to Weil), to disparage fields of interests and approaches that differed from the typical Bourbakian ones:

> I did not have the impression that this 'allergy' to the Bourbaki style would undermine the communication between these mathematicians and myself or other members or

> sympathizers of Bourbaki, as could have been the case if the spirit of the group had been parochial or characteristic of an elite within an elite. Beyond style and fads, there was among the members of the group a vivid sense for mathematical substance, whatever its origin. 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' [*'emmerdeurs'*]. Since this concerned matters hardly known to me at the time, I tended to accept such appraisals on face value, for I was impressed by such off-hand assurance–until the day when I discovered that such and such 'bullshitter' were persons endowed with a deep and original mind who had not had the luck of pleasing my brilliant friend. It seems to me that for certain members of Bourbaki, an attitude of modesty (or at least of reservation) towards the works of others, when one ignores that work or imperfectly understands it, was eroded from the outset, while that 'mathematical instinct' which makes you sense that there is rich substance or solid work, still exists without needing to have gained reputation or renown. From echoes that reach me here and there, it seems to me that both, modesty and instinct, have become rare these days in what was once my mathematical milieu. (*R&S* I, 148)

Of course, one must always keep in mind that these texts were written from a position of total retirement and deep hostility towards, not just individual members of Bourbaki, but the scientific community in its entirety.

Bourbaki's *Éléments de mathématique* became a most influential and widely used, classical textbook of twentieth-century mathematics. Generations of students learnt their algebra or their topology from the relevant volumes of the treatise. More than that, it was a highly useful work of reference. Also the Bourbakian choice was itself an influential factor affecting the way that mathematical careers were built in various places around the world. Some readers may have been aware in various ways of the connection between the distinctive mathematical style embodied in the text and the *sui generis*, 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 exactly they led to the final product. With all likelihood, no one outside the inner circle was aware of the tension and conflicts surrounding the *structures* vs. categories question discussed above. But the truly curious point is that for all of its success and impact at various levels, the *Éléments* did not become a textbook of choice for the study of *mathematical analysis* in the sense originally intended by the founding members. Goursat's *Cours d'analyse mathématique* was surely superseded, both in France and elsewhere, by more updated textbooks soon after Bourbaki started its activities and in accordance to their original motivation. But students around the world who took their 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, and never did so with the text that the 'Comité de rédaction du traité d'analyse' had in mind in their early meetings of 1935.

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