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# The Functional Role of Structures in Bourbaki

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## 1. Introduction

From antiquity to the 19th century and even up to now, the following two theses are among the most debated in the philosophy of mathematics:

a) According to the Aristotelian tradition, mathematical objects such as numbers, quantities, and figures are entities belonging to different kinds.
b) Mathematical objects are extralinguistic entities that exist, independently of our representations, in an abstract world. They are conceived by analogy with the physical world and designated by singular terms of a mathematical language.

The Aristotelian thesis and that of ontological "Platonism" were countered by nominalism and early tendencies of algebraic formalization; but they became even more problematic when mathematicians, such as Niels Abel, thought of relations before their relata or when they, as Hermann Hankel (1867) pointed out, posited that mathematics is a pure theory of forms whose purpose is not that of treating quantities or combinations of numbers (see Bourbaki 1968, 317). In the 1930s, Bourbaki finally defended the view that mathematics does not deal with traditional mathematical objects at all, but that objectivity is solely based on the stipulation of structures and their development in a hierarchy.

In the history of 20th-century mathematical structuralism, the figure of Bourbaki is prominent; sometimes he is even identified with the philosophical doctrine of structuralism. However, the Bourbaki group consisted of pure mathematicians—among them the greatest of their generation—most of whom had a conflicted relationship to philosophy. This chapter proposes to explore this tension, following the current philosophical interest in scientific practice. The problem with properly assessing Bourbaki's importance is that he was at the same time the collective author of a monumental and long-lasting treatise (in a golden age of more than 30 years) and a pleiad of individual geniuses (including

Gerhard Heinzmann and Jean Petitot, *The Functional Role of Structures* in *Bourbaki* In: *The Prehistory of Mathematical Structuralism*. Edited by: Erich H. Reck and Georg Schiemer, Oxford University Press (2020). © Oxford University Press.
DOI:10.1093/oso/9780190641221.003.0008

five Fields medalists: Schwartz, Serre, Grothendieck, Connes, Yoccoz). The former had to faithfully conform to initial editorial choices, while the latter were at the cutting edge of innovation and creativity.[1] So it makes no sense to think that Bourbaki was not aware of mathematical advances, since his members were among the main agents in these advances.

Quite often interpreters focus only on Bourbaki's formal definition of structure, so as then to dismiss it. Our approach will be quite different. Our thesis is that the use of the concept of structure in Bourbaki is not so much logical and, in a philosophical sense, foundational as pragmatic and functional—"functional" not in the mathematical sense, but in a sense analogous to the relationship between structure and function in biology. We will illustrate the functional role of structures in Bourbaki's work, starting with Hilbert's axiomatics, which was developed to perfection by the Bourbaki group, and going up to category theory, thus to a higher level of structuralism, a path that Bourbaki initiated without yet actually engaging in it.[2]

## 2. Bourbaki, the *Éléments*, and the *Séminaires*

Nicolas Bourbaki was the pseudonym of a "collective mathematician,"[3] formed in 1934–35 by a group of young French mathematicians who graduated (with the exception of Mandelbrojt) from the École Normale Supérieure in Paris, who did research abroad, primarily in Germany (but also in Denmark, Italy, Hungary, Sweden, Switzerland, and the United States), and who taught mostly in Strasbourg, Nancy, and Clermont-Ferrand.[4] This group included Henri Cartan,

[1] Other mythical examples of such groups in French history include, in the middle of the 16th century, *La Pléiade*, which completely transformed the norms of poetic language (Du Bellay, Ronsard, Jodelle, Belleau, de Baïf, Peletier du Mans, de Tyard, etc.); and in the second part of the 18th century, the *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Diderot, d'Alembert, de Jaucourt, d'Holbach, Dumarsais, Quesnay, Turgot, etc., related to Montesquieu, Voltaire, Buffon, Mably, Condillac, Helvétius, etc.).

[2] For a philosophical discussion of the role of Bourbaki's concept of structure in the interpretation of category theory, see Krömer (2007).

[3] See Chevalley and Guedj (1985). The name of the French general Bourbaki, defeated in the French-German war of 1870–71, was part of the anti-militarist folklore at the École Normale Superieure long before the group chose it (Beaulieu 1989, 278ff).

[4] For a detailed bibliography and rich archive, see the site http://archives-bourbaki.ahp-numerique.fr of the Archives Henri-Poincaré, compiled by Liliane Beaulieu (supplemented by Ch. Eckes and G. Ricotier) and the site http://sites.mathdoc.fr/archives-bourbaki/feuilleter.php of the Association des Collaborateurs de Nicolas Bourbaki. An introduction to the corresponding history is Maurice Mashaal's *Bourbaki: A Secret Society of Mathematicians* (2006). Another interesting historical source is Amir Aczel's *The Artist and the Mathematician: The Story of Nicolas Bourbaki, the Genius Mathematician Who Never Existed* (2006). The two books were reviewed in 2007 by Michael Atiyah in the *Notices of the AMS*. At a theoretical level, a well-known reference is the 1992 essay by Leo Corry, "Nicolas Bourbaki and the Theory of Mathematical Structure."

Claude Chevalley, Jean Delsarte, Jean Dieudonné, Szolem Mandelbrojt, René de Possel, and André Weil, to which must be added Paul Dubreil and Jean Leray (who both, however, withdrew after some meetings), Jean Coulomb, and Charles Ehresmann (1935–36).[5] Their goal was "to define for 25 years the syllabus for the certificate in differential and integral calculus by writing, collectively, a treatise on analysis. Of course, this treatise will be as modern as possible" (Beaulieu 1989, 28). This was a revolt against the dominant French mathematics of the 1930s.

The initial group had no money nor any official administrative structure. Each draft of a chapter of their famous multi-form treatise, *Éléments de Mathématique* (the expression "Mathématique" in the singular emphasizes the unity of mathematics), was discussed largely in the group and had to be accepted unanimously by those present at the regular Bourbaki meetings.[6] Elected by consensus, after age 50 a Bourbakist had to leave the group, whose list of members was an (open) secret. Among the most prominent later Bourbakists were Hyman Bass, Armand Borel, Pierre Cartier, Alain Connes, Michel Demazure, Jacques Dixmier, Samuel Eilenberg, Roger Godement, André Gramain, Alexander Grothendieck, Jean-Louis Koszul, Serge Lang, Pierre Samuel, Laurent Schwartz, Jean-Pierre Serre, John Tate, Bernard Teissier, Jean-Louis Verdier, and Jean-Christophe Yoccoz, all also pursuing their own individual work.

The creation in 1962, by Grothendieck, of the group of algebraic geometry at the "Institut des Hautes Études Scientifiques," a "European Princeton Institute of Advanced Studies" (Bolondi 2009, 701) located in the Paris suburb Bures-sur-Yvette, was at the same time a continuation and an improvement of the Bourbaki perspective in mathematics in France and around the world. In a certain sense, the monumental *Éléments de Géométrie Algébrique* by Grothendieck and Dieudonné (1960–67) can be considered as a systematization of the same type as the *Éléments de Mathématique*: driven by the desire to optimize the framework of demonstration of great theorems and to attack major conjectures, especially the Weil conjectures. Indeed, its language was no longer that of classes of structures in a universe of set theory but that of full-fledged category theory.[7] But, as communicated by Jean-Pierre Ferrier, the project was a resurgence of the Bourbaki project, equally ambitious and original, greatly renewing mathematical

[5] Beaulieu (1989, 12–13). Beaulieu's dissertation is the most extensive description of the origin and the first 10 years of activity of the group. It is the sourcebook of all biographically oriented studies on Bourbaki (for works on Bourbaki see Beaulieu 2013). See also Weil (1992) for Weil's memories.

[6] The first publication of the *Éléments* was released in 1939 (Bourbaki 1939). An interesting document on Bourbaki's birth is the first issue of the *Journal de Bourbaki* handwritten by Jean Delsarte on November 15, 1935. Composed with a touch of humor, it refers to the creation of the group at the "congress" held at Besse-en-Chandesse in July and presents a first division of labor between Cartan, Delsarte, Dieudonné, Chevalley, Mandelbrojt, de Possel, and Weil (http://sites.mathdoc.fr/archives-bourbaki/PDF/deljb_001.pdf).

[7] On Grothendieck and the shift to category theory, see McLarty (2008), as well as the contribution, also by Colin McLarty, on Saunders Mac Lane and category theory in this volume.

thinking, as Bourbaki did in his time, and overcoming many difficulties raised by Bourbaki's initial choices.

In this essay we do not only report on the *Éléments* and its content. Bourbaki is a collective author, but, again, also a pleiad of unique individual masterminds who took up the most difficult mathematical challenges. This is confirmed by its encyclopedic *Séminaire*, which was unparalleled and continues until today. Started in 1948, it reached its 1,118th talk in June 2016. Almost all great mathematical results have been presented in it. As creative mathematicians, the members of Bourbaki were not only interested in the context of justification but also, and even more, in the context of discovery. Their conception of structures must be understood in this light. In particular, they were all working on very complex conceptual proofs of "big problems," and for them there existed a *complementarity* between general relevant structures and specific hard problems. One could say that this complementarity found its material expression in the complementarity of the *Éléments* and the *Séminaire*: the function of the *Éléments* was to offer to working mathematicians an extremely wide toolbox of axiomatized devices (structures), to be used as conceptual apparatuses in complex proofs, while the function of the *Séminaire* was to inform, in preview, about mathematical progress, thus being a preferred place to host creation.

Many controversial aspects of Bourbaki are well known, e.g., its overly formalist and algebraic setting or its lack of interest in logic. The first has been strongly criticized from the start by some great mathematicians who refused to be members of Bourbaki, while belonging to the same generation of the École Normale Supérieure as its founders. This is the case, e.g., for René Thom (1970), who accused Bourbaki of destroying geometric intuition, or for Roger Apéry, a constructivist mathematician inspired by the French constructivist school of Poincaré, Borel, Lebesgue, Fréchet, and Denjoy and opposed to Hilbertian formalism and axiomatics. The second aspect has been denounced, e.g., by Adrien Mathias in his 1992 paper "The Ignorance of Bourbaki," which analyzes the inadequate reflections of Bourbaki on foundational issues in set theory. For Matthias, Bourbaki's *Set Theory* "appeared to be the work of someone who had read *Grundzüge der Mathematik* by Hilbert and Ackermann,[8] and *Leçons sur les nombres transfinis* by Sierpinski, both published in 1928, but nothing since."[9] A lot of things have also been written about the folklore of Bourbaki, his legend, his dictatorial power, his dramatic impact on education with the introduction of "modern mathematics" in schools (see again Thom 1970). Our purpose

[8] It seems that Mathias means to refer to *Grundzüge der theoretischen Logik*.

[9] Mathias (1992, 5). Sometimes the ignorance seems to be intentional and polemical: thus Dieudonné says explicitly that his neglect of Gödel's result concerning a consistency proof for formal systems is not a consequence of ignorance, but of a "philosophical" position (see Heinzmann 2018).

here is quite different. We will try to explain the *functionality* of Bourbaki's structuralism.

## 3. Traditional Mathematical Objects versus Structures

Bourbaki inaugurated an axiomatic-structural point of view that could seemingly work without the need of metamathematics in Hilbert's sense. Indeed, given that metamathematics is "finitist" and contentual, it would be an exception to the slogan that mathematics is only about formal structures. The hypothetical-deductive foundations of Bourbaki were explicitly designed to be neutral with respect to philosophical foundations. However, it can engaged with along the lines of the philosophical interest in scientific practices that has been renewed recently: foundations as structural systematization.

The "working mathematician"[10] Henri Cartan, one of the founders of Bourbaki, wrote in 1943: "The mathematician does not need a metaphysical definition; he must only know the precise rules to which are subject the use he has in mind. . . . But who decides upon the rules?"[11] This may sound Wittgensteinian, but is not so in reality. According to Cartan, mathematical reasoning in a given area intuitively obeys certain rules at first; and if difficulties arise, the use is adapted, etc. Consequently, a mathematical reality is created through practice. What is the criterion for the practice and for the rules that result? In a historical notice on set theory, Bourbaki writes:

> [It was] recognized that the "nature" of mathematical objects is ultimately of secondary importance, and that it matters little, for example, whether a result is presented as a theorem of a "pure" geometry or as a theorem of algebra *via* analytical [Cartesian] geometry. In other words, the essence of mathematics . . . appeared as the study of *relations* between objects which do not of themselves intrude on our consciousness, but are known to us by means of *some* of their properties, namely those which serve as the axiom at the basis of their theory. (Bourbaki 1968, 316–317)

Bourbaki considered "the problem of the nature of beings" or of "mathematical objects" as deriving from a "naive point of view," "half-philosophical, half-mathematical" (Bourbaki 1948, 40). Indeed, it would be naive to presuppose that we can have a well-defined mathematical object at all, i.e., that it can be identified

[10] An expression used by Bourbaki (1949).
[11] Cartan (1943), transl. by Gerhard Heinzmann.

completely by specifying a property that characterizes it. It is only apparently well-defined according to the traditional theory of definition.[12]

Bourbaki henceforth abandoned the philosophical problem of object-individuation in favor of a premise that seems to have the same meaning today: the unity of mathematics (Houzel 2002, 3). The tool to achieve this unity was Hilbert's axiomatic method: it provides clarity and rigor in the register of reasoning (see Dieudonné 1939, 232b) by using a systematization of mathematical theories (Bourbaki 1948, 37). It allows one to obtain all kinds of axiom systems; not for all of classical mathematics, however, but only those domains that correspond to the hierarchy of structures classified as "simple," "complex," and "mixed." Indeed, to define a simple structure, we take a set "of elements whose nature is not specified," provide it with certain relationships, and formulate the axioms that satisfy them. And we define the structure as *algebraic* "if the relationships are the laws of composition," as *topological* "if the relations concern the intuitive concepts of neighborhood, limit and continuity," and as an *order structure* if the relations are of that type.

## 4. The Unity of Mathematics: Structures and Entangled Problems

Let us focus now on how structures were used by Bourbaki, in a process of clarification and unification, to further the discovery of new and unexpected results—as common to several systems of objects of very different origins, as indicative of deep and fruitful analogies between theories far removed from each other, and as a powerful heuristic for proofs. As two examples, Cartan's filters and Weil's uniform structures are among the greatest inventions of Bourbaki. The first illuminates the analogy between the convergence of sequences and that of functions, while the second illuminates the analogy between a metric and a family of pseudometrics. A third example produced directly a new result: the so-called *Banach-Alaoglu* compactness theorem (for the weak topology) of the dual unit ball of a normed space, which is also due, in the form that we know today, to

[12] H. Cartan gives the following example: "According to Lebesgue, the quantity

$$\lim_{m\to\infty}\left[\lim_{n\to\infty}\cos(\mathrm{m}!\pi C)^{2n}\right]$$

is a well-defined number, when C is a well-defined real number, for example, Euler's constant. However, this quantity is equal to 0 if C is irrational, 1 if C is rational; and we are still today ignorant whether Euler's constant is rational or irrational. Thus, if C is Euler's constant, we obtain a well-defined number, but we do not know if it is equal to 0 or to 1" (Cartan 1943, 5; transl. Gerhard Heinzmann).

Bourbaki.[13] Everything in it is owed to the clarification by means of weak topologies, which revived a problem that could not be correctly formulated until then.

This functional aspect of structures, on which Bourbaki continually dwelt, is governed by the principle of the unity of mathematics, that is to say, by the very strong ability to translate pieces of one mathematical theory into another theory. Besides the deductive "vertical" dimension internal to every theory, taking into account the relevant structures can reveal a host of "horizontal" connections between different theories.[14] The resulting "horizontal" navigation between different theories involves (at least) two processes. On the one hand, there are analogies, intuitive at first, between structures of the same type in different areas, i.e., structures whose clarification and systematization often lead to new discoveries. On the other hand, there is the encounter of different structures within the same "crossroads" area, which allow for the unification of theories. We need both to tackle the complex proofs of intricate problems. (We will come back to this issue later.)

Let us clarify the importance of the unity of mathematics according Bourbaki further. In terms of category theory, many connections between theories correspond to the existence of functors and natural transformations of functors between categories (for example, between topological spaces and groups in algebraic topology); but many others are not simply functorial. In fact, conceptually complex proofs are very uneven, with rough and rugged multi-theoretical routes in a sort of "Himalayan chain" whose peaks seem inaccessible. They cannot be understood without the thesis of the unity of mathematics, because they are in some sense *holistic*. This holistic aspect of complex proofs has always been emphasized by Bourbaki. Thus, in his *Panorama des Mathématiques pures: le choix bourbachique* (1977, xii), Jean Dieudonné classifies theorems into six classes:

1. "Dead-born problems [les problèmes mort-nés]": particular problems for which a certain theoretical approach has failed.
2. "Problems without posterity [les problèmes sans postérité]": problems whose resolution did not generate any other problems.
3. "Problems bringing forth a method [les problèmes qui engendrent une méthode]": e.g., analytic number theory or finite group theory.

[13] Leon Alaoglu proved his generalization of Banach's 1932 theorem in 1940, but Jean Dieudonné claimed that it was already announced in Bourbaki in 1938. The point is controversial.

[14] Cf. Cavaillès' terminology of the "thematic" and "horizontal" construction of concepts (Cavaillès 1947, 27).

4. "Problems clustering around a general, fertile, and vibrant theory [les problèmes qui s'ordonnent autour d'une théorie générale, féconde et vivante]": e.g., Lie group theory or algebraic topology.
5. "Declining theories [les théories en voie d'étiolement]": e.g., the theory of invariants.
6. "Theories on the way to dilution [les théories en voie de délayage]": problems that try to modify the axioms of already known rich theories.

It is the third and fourth classes that deserve special attention in our context, since they manifest the enigmatic unity of mathematics. A typical example given by Dieudonné of difficult key results involving this unity, by intertwining several very heterogeneous theories, is that of *modular forms*:

> The theory of automorphic and modular forms has become an extraordinary crossroads where the most varied theories are reacting to each other: analytic geometry, algebraic geometry, homological algebra, non-commutative harmonic analysis, and number theory. (Dieudonné 1977, 87)

The notion of "crossroads" (*carrefour*) is crucial: "big problems" are problems where many structures of different type interact and became *entangled*. The systematization of structures in the *Éléments* can be thought of as a "disentanglement."

A spectacular confirmation of Dieudonné's claim has been the proof by Andrew Wiles and Richard Taylor, in 1993–95, of the Shimura-Taniyama-Weil conjecture (implying Fermat's Last Theorem via a theorem of Ribet). This proof uses modular forms in a central way, and it is the prototype of a complex proof whose deductive parts are widely *scattered* in the global unity of the mathematical universe.[15] Its holistic status has been emphasized by many specialists. For example, Israel Kleiner writes:

> Behold the simplicity of the question and the complexity of the answer! The problem belongs to number theory—a question about positive integers. But what area does the proof come from? It is unlikely one could give a satisfactory answer, for the proof brings together many important areas—a characteristic of recent mathematics. (2000, 33)

15 For a summary of the proof, see Petitot (1993).

Similarly, Barry Mazur writes:

> The conjecture of Shimura-Taniyama-Weil is a profoundly unifying conjecture—its very statement hints that we may have to look to diverse mathematical fields for insights or tools that might leads to its resolution. (1991, 594)

To use the complementarity in physics between observed phenomena and measuring apparatuses as a metaphor, we could put it this way: For the Bourbakists, "big problems" and hard conjectures (the distribution of primes, linked to the zeroes of the zeta function and the Riemann hypothesis, etc.) were treated as key mathematical "given" phenomena that had to be looked at using appropriate formal "apparatuses"; and axiomatized structures are precisely such devices. Thus mathematics is *at the same time holistic and modular*.[16] Structures are modular, but key phenomena are holistic, since they have to be "observed" by using many completely different "apparatuses." The "scattered" character of complex proofs is due to this holistic/modular complementarity.

This complementarity illuminates some aspects of the axiomatic method that Bourbaki inherited from Hilbert: (i) the fact that axioms can be freely chosen and are prescriptive principles, as opposed to being descriptive of objects (in the physical metaphor, to treat structures as objects would be a confusion between objects and apparatuses); (ii) the fact that many genetically different mathematical objects can be analyzed using the same structures; (iii) the fact that, in order to avoid an irrelevant axiomatic game, relevant "interesting" structures must be discovered through a reflexive process from the practice.

## 5. René Thom and Bourbaki

It is interesting to return here to the evaluation of the *Éléments* by René Thom, a colleague of the Bourbakists first at the École Normale Supérieure (he was a PhD student of Henri Cartan, together with Jean-Pierre Serre) and, after 1963, at the Institut des Hautes Études Scientifiques. A good reference is Thom's 1970 paper, "Les mathématiques modernes: une erreur pédagogique et philosophique ?" (translated in 1971 for the *American Scientist*). Thom criticized the idea that axiomatization can be at the same time a tool for systematization *and* for discovery.

16 "Modular" not in the mathematical sense, but in a sense analogous to "modularity" in programming languages or in cognitive science ("modularity of mind").

> During the last few years many such opinions were being put forward about the importance of axiomatization as an instrument both of systematization and of discovery. Instrument of systematization for sure; but whether of discovery, that is a much more doubtful affair. (Bolondi 2009, 705)

And he based his critique of Bourbaki precisely on this point:

> It is characteristic that from the immense effort at systematization by Nicolas Bourbaki (which is not a formalization anyway, since Bourbaki uses a non-formalized meta-language) no new theorem of any importance has resulted. And if researchers in mathematics make reference to Bourbaki, they find food much more often in the exercises—where the author has repelled the concrete material—than in the deductive body of the text. (Thom 1971, 697–698)

Thus for Thom the *Éléments* offers a systematized toolbox of axiomatized structures whose real interest for mathematical practice lies outside of it, in "concrete problems." We agree; but we will see later that, in fact, Bourbaki himself was perfectly aware of this and thought that the purpose of the *Éléments* was to help in the resolution of concrete "big" problems.

In addition, Thom attributed to Bourbaki, and criticized, the idea that structures can be derived from set theory:

> The old Bourbakist hope, to see the mathematical structures emerge naturally from the hierarchy of sets, from their subsets and their combination, is no doubt a chimera. Reasonably, one can hardly escape the impression that important mathematical structures (algebraic structures, topological structures) appear as data fundamentally imposed by the external world, and that their irrational diversity finds its only justification in their reality. (Thom 1971, 699)

Here again, Bourbaki was in fact aware of this point and held, as we already pointed out, that relevant "interesting" structures must be discovered in a reflexive way from the practice and from the search for solutions to given "big" problems. Hence Thom's criticism is justified for a restricted formal conception of structures, but not for a more general approach emphasizing their functional role.

From this perspective, we will now comment further on (i) the restricted formal definition of structures in Bourbaki; and (ii) their functional role in a more general structuralist context. The latter ranged from Hilbert's axiomatic approach to Grothendieck's categorical approach, and it involved discovery and complex proofs.

## 6. Formal Definition of Structures: Set Theory and Category Theory

Initially, the key notion of structure in Bourbaki was supposed to be a noncontroversial concept; but the members of the group did not agree on the importance and priority it should be given. Especially the question of its definition was not conceived by everyone in the same manner. The options were to give either (1) a vague account of how to define a structure, formulated in the metatheory (Bourbaki had done so from the beginning), or (2) an explicit and general definition, to be referred to whenever a new structure is introduced. Liliane Beaulieu's PhD thesis bears witness to the hesitations of the first members of Bourbaki, in the 1930s, with respect to a formal definition of structure.[17] The definition was finally published 20 years later, [18] but was hardly respected or used in the released mathematical corpus, despite the principle to publish only what was unanimously accepted by the group.

Indeed, as pointed out by Leo Corry already (1992, 327), Bourbaki made a very revealing comment in this context. It can be found in the *Fascicule de résultats* (*Summary of Results*) of the treatise *Théorie des Ensembles*,[19] 3rd edition (1958), originally released as the first publication by Bourbaki (1939). In it, an informal definition of "structure" is used—well before the publication of chapter 4

[17] In the first plenary meeting in Besse, on July 1935, one can read in a resolution: "We warn the reader, once and for all, that the operations that will be applied to sets can be axiomatized and justified, provided that they are only carried out on sets we study in a mathematical theory" (Beaulieu 1989, 233). There is also a project, probably discussed during the 1936 plenary meeting of Chançay, entitled "Projet Laïus Scurrile" (the group used the term "scurrile" mostly for "what has to be done without enthusiasm, which leads to nothing, or what has a philosophical content." Thus, we find in the minutes of the Bourbaki meetings or in his writings the expressions "laius scurrile" (Beaulieu 1989, 228, note 37).) In the project description we can read: "The subject of a mathematical theory is a structure organizing a set of elements: the words 'structure', 'set', 'elements' are not likely definable, but constitute the basic concepts for all mathematicians. They take on clearer form once we have had the opportunity to define structures, as will be done from this chapter one. Thanks to a structure, one has the right to say that elements or parts of the set considered in a theory have some relationships between them or possess certain properties: the words 'part', 'relationship', 'property' are likely undefinable too, and are also basic notions. According to our principles, we should state the axioms that satisfy these notions: these axioms are those of set theory, and of any mathematical theory. Given the difficulties, until now not overcome, which stand in the way of the formulation of such axioms, we will assign temporarily to these words the meaning they have in ordinary language, and we will give in what follows general rules governing their use and how to switch from one to another. . . . We say that one has defined a structure on a fundamental set if properties of the (or relationships between the) elements of this set are given, or if one of those can be deduced by a combination of the above operations, and, eventually, by previously given auxiliary fundamental sets" (Beaulieu 1989, 561; transl. Gerhard Heinzmann).

[18] See chapter 4 of *Théorie des Ensembles* (Bourbaki 1957). The *Fascicule de résultats* of this volume had already been published in 1939, i.e., 18 years earlier!

[19] The *Summaries* are in principle attached to every volume of *Éléments de Mathématique*, and their goal is to give a "rough idea" of an entire book either for orientation before reading or for a hurried reader (Bourbaki 1939, vi).

of *Théorie des Ensembles* itself (1957). And in section 8 of the *Fascicule*, devoted to scales of sets and structures (*échelles d'ensembles et structures*), Bourbaki comments in a footnote:

> The reader may have observed that the indications given here are left rather vague; they are not intended to be other than heuristic, and indeed it seems scarcely possible to state general and precise definitions for structures outside of the framework of formal mathematics (see Chapter IV). (Bourbaki 1968, 384)[20]

In the *Fascicule de résultats*, four pages "summarize," or better anticipate, the 69 pages on the notion of structure in the fourth chapter of *Théorie des Ensembles*; and the footnote indicates that Bourbaki put chapter 4, entitled "Structures," in the "framework of formal mathematics," which is developed in chapter 1. This is even clearer in the introduction to chapter 4:

> The purpose of this chapter is to describe once and for all a certain number of formative constructions and proofs (cf. chapter I, §1, no. 3 and §2, no. 2)[21] which arise very frequently in mathematics. (Bourbaki 1968, 259)

Bourbaki will never resort to such formal "structures" in his other books. Indeed, as also noted by Corry, until the publication of chapter 4 in 1957 the only references are to the *Fascicule de résultats*, which gives simply an informal definition:

> Given for example, three *distinct* sets *E*, *F*, *G*, we may form other sets from them by taking their sets of subsets, or by forming the product of one of them by itself, or again by forming the product of two of them taken in a certain order. In this way we obtain *twelve* new sets. If we add these to the three original sets *E*, *F*, *G*, we may repeat the same operations on these fifteen sets, omitting those which give sets already obtained; and so on. In general, any one of the sets obtained by this procedure (according to an explicit scheme) is said to belong to the *scale of sets on E*, *F*, *G as base*. (Bourbaki 1968, 383)

[20] This footnote is the only change from previous editions with respect to section 8 ("Structures"); we therefore quote always the most accessible English edition of 1968.

[21] By "formative constructions and proofs," Bourbaki understands in chapter 1, entitled "Description of Formal Mathematics," the definition of a formula-calculus ("règles d'assemblages")—"terms are assemblings which represent objects, and relations are assemblings which represent assertions which can be made about these objects (20)— together with a formal description of derivations, defined as sequences of relations.

The discussion is rounded off in the following way:

> Thus being given a certain number of elements of sets in a scale, relations between . . .[22] elements of these sets, and mappings of subsets of these sets into others, all comes down in the final analysis to being given a *single element* of one of the sets in the scale.
>
> In general, consider a set *M* in a scale of sets whose base consists, for the sake of example, of three sets *E*, *F*, *G*. Let us give ourselves a certain number of explicitly stated properties of [an][23] element of *M*, and let *T* be the intersection of the subsets of *M* defined by these properties. An element *s* of *T* is said to define a *structure* of the *species T* on *E*, *F*, *G*. The structures of species *T* are therefore characterized by the schema of formation of *M* from *E*, *F*, *G*, and by the properties defining *T*, which are called the axioms of these structures. We give a specific name to all the structures of the same species. Every proposition which is a consequence of the proposition "$s \in T$" (i.e. of the axioms defining *T*) is said to belong to the *theory* of the structures of species *T*. (Bourbaki 1968, 383)

Bourbaki assumed not only to have written the previous chapters to meet these specifications, which remained an outline of the formal content of chapter 4, but was also working on filling them out. In addition, he needed to introduce structures with morphisms to talk about derived structures.

Nevertheless, Bourbaki did not wait until this chapter was written, because the expectations were clear. In particular, he had a clear idea of the three main types of structures, i.e., the "mother structures": algebraic structures, topological structures, and order structures. Thus in the introduction to the volume on *Algebra* it is noted:

> In conformity with the general definitions (*Théorie des Ensembles, IV, §1, no. 4* [entitled "Espèces de structures"], being given on a set one or several laws of composition or laws of action defines a structure on *E*; for the structure defined in this way we preserve precisely the name algebraic structures and it is the study of these which constitutes Algebra.
>
> There are several species (*Théorie des Ensembles, IV, §1, no. 4*) of algebraic structures, characterized, on the one hand, by the laws of composition or laws of action which define them and, on the other hand, by the axioms to which these laws are subjected. Of course, these actions have not been chosen arbitrarily, but are just the properties of most of the laws which occur in applications, such

[22] The available translation is "between generic elements," but "generic" is not in the original French text. We skip it because "generic" has a specific mathematical content that does not apply here.
[23] We skip again the word "generic" here.

> as associativity, commutativity, etc. Chapter I is essentially devoted to the exposition of these axioms and the general consequences which follow from them; also there is a more detailed study of the two most important species of algebraic structure: that of group (in which only *one* law of composition occurs), and that of a ring (with *two laws* of composition) of which a *field* structure is a special case. (Bourbaki 1974, xxii)

As we have seen, Bourbaki ranked the structures in a hierarchy at the base of which are the three "mother structures": algebraic structures are characterized by "laws of composition," as van der Waerden had already done[24]; order structures by an order relation; and the topological structures, again, by "an abstract mathematical formulation of the intuitive concepts of neighborhood, limit, and continuity, to which we are led by our idea of space" (Bourbaki 1948, 227). These basic structures are followed by "multiple structures" involving two or more mother structures (e.g., topological algebra), and at the top of the hierarchy are placed the "theories properly particular." The criteria of Bourbaki's hierarchy of structures for each kind of structures are simplicity, generality, and the number of axioms (229).

Actually, it is a contradiction to speak of a hierarchy within a particular structure. At most Bourbaki can compare the species of one kind of structure using the same scale, i.e., the same data for which the axioms set down properties. Thus groups are more general than commutative groups, which require an additional axiom while possessing the same scale. But groups and topological groups cannot be compared; the first are not more general than the latter: they are not defined on the same data (scales), and Bourbaki had to use what is now called the "forgetful" functor to reduce the scale of topological groups to the scale of mere groups. However, topological groups can be treated as mixed structures, i.e., as topological spaces provided in addition with a group structure whose operations are continuous. It is sufficient to consider the huge project of Lie groups here (where one uses the structure of a differentiable manifold in addition). But it is not clear whether, for Bourbaki, mixed structures were also full-fledged sui generis structures, which would be the case from a categorical perspective (the category of topological groups is a specific category). In any case, the categorical formalism necessary to compare species of structure was not yet fully available to Bourbaki. He began with structured sets and isomorphisms, so as then to add the most general relations between structured sets that amount to morphisms. This means in fact placing them in a category, but without using the term. The issue of the relationship between species of structures is not really addressed,

[24] From the beginning, for Bourbaki, *Modern Algebra* by B. L. van der Waerden (1930–31) was a model for the program in analysis, and then for mathematics as a whole (see Beaulieu 1989, 164).

which would mean considering *formally* functors, natural transformations, and categories of categories. In other words, in his pre-categorical framework Bourbaki introduced many categorical objects and constructions: morphisms, sub-objects, quotients, Cartesian products, projective and inductive limits, universal problems, and (implicitly) functorial objects, like the fundamental group $\pi_1(X, x)$ of a (pointed and arc-connected) topological space $(X, x \in X)$, but all in a universe of set theory and without the formal machinery of later category theory. For Bourbakists, categorical notions and operations became relevant and even inescapable in the 1950s (we only have to look at Cartan's seminar from 1948 onward); but for the *Treatise* category theory would have been too important an editorial transformation and, moreover, it was not really a foundational issue.

Why was the discrepancy between the formal definition of structure in chapter 4 of *Théorie des Ensembles* and the actual practice in applications never fixed by Bourbaki? And why was he not more interested in corresponding metamathematical questions (such as the question of consistency)? There is both a historical-mathematical and a systematic-philosophical explanation. The historical-mathematical explanation is that, even before being released, the chapter on structures had already been superseded, since it would have needed to consider categories.[25] Some members of Bourbaki did not agree with it, but Bourbaki could also not revise it for a silly material reason: Everything that had been printed so far would have to be thrown away.[26] Bourbaki confined itself, initially, to print just the *Fascicule de résultats* on the subject; and this is precisely because nothing else was needed for the main books of the *Élements*. Actually, the distance between the rest of the *Éléments* and its formal definition of structures was even greater. It also treated structures accurately defined but not in the formal sense of chapter 4. For example, in chapter 9 of his *General Topology* Bourbaki defines a normed space as a vector space "endowed with the structure defined by a given norm" (Bourbaki 1966, 170, 1st edition 1958), thus as a mixed structure (see the example of topological groups earlier). But, as communicated by Jean-Pierre Ferrier, there is no explicit reference to the formal definition of "structure" here; in fact, it is not explained in chapter 4 (1957) what the structure defined by a given norm is and what exactly "morphisms" between normed spaces could be.

[25] We emphasize: as already noted above, many categorical concepts are used more or less implicitly by Bourbaki. Categories were present between the writing of *Élément des Mathématiques* in 1939 and its publication in 1957. But the framework of *Éléments* is set theory and not category theory, because otherwise it would have meant a complete rewriting. Algebraic topology has been the main source of category-like reflections for Bourbakists, but, strangely enough, they postponed the writing of the volume *Algebraic Topology* (chapters 1–4) until 2016!

[26] A fuller historical account of this debate can be found in Krömer (2006).

But there is also a systematic-philosophical explanation. Namely, in some ways Bourbaki remained closer than his rhetoric suggests to the "geometric structuralism" of Poincaré than to that of Hilbert. According to both Hilbert and Poincaré, geometrical axioms and axiom schemas are not propositions, i.e., true or false, and there are no special ("ontologically" specific) objects that geometry should have to study. Rather, geometry is just a system of relations that can be applied to many kinds of objects. For Poincaré, the metric postulates in geometric systems are "apparent hypotheses" that are neither true nor false, i.e., they are conventions (see Poincaré 1898). For Hilbert, the axioms and axiom schemas in geometric systems are expressions that, again, are neither true nor false. But according to Hilbert, mathematical formalism requires a "finitist" metamathematics in order to demonstrate the consistency of formal mathematical systems. The failure of this program is well known (Gödel) and was known to Bourbaki (cf. note 9). In contrast, for Poincaré it is necessary to explain the hypotheses with respect to an informal standard that involves the unity of mathematics and preexists intuitively in our mind (at a first stage transformation groups, later the qualitative structure of topological spaces); and he takes a structuralist position without disengaging meaning and knowledge completely from ostension.[27] Poincaré's concept of structure is thus not the new Hilbertian one derived from his axiomatization of geometry, but constitutes a development of the traditional idea of geometrical invariances. For Bourbaki too, the mother structures have an informal background. And he also incorporates the metamathematical problems into mathematics, as it were, by adopting an empirical position and by sharing Poincaré's concern for the unity of mathematics. From a philosophical point of view, it is clearly the status of Hilbert's metamathematics (invalidated by Gödel) that makes it distinct from the shared position of Bourbaki and Poincaré.

From a practical point of view one can ask, finally, whether Bourbaki's "mother structures" are "natural" in the sense of common-sense habits. Here we agree with Piaget's analysis: "No subject, before he has learnt it, has the 'concepts' of what a group, lattice, topological homeomorphism etc. is: and in most cultural milieus, we do not come across such concepts before university or the upper classes of secondary school. Thus, it is not in the domain of reflective thought, considered from the subject's view-point, to ask whether these structures are 'natural' " (Beth and Piaget 1966, 167). In other words, to put such structures at the beginning of the mathematical edifice is not justified by socio-psychological practice, although elements of them can be used to describe parts of both mathematical practice and socio-psychological practice. Hence Bourbaki's mother structures are a sort of mix of normative standards and empirically confirmed tools.

[27] Compare here the contribution on Poincaré, by Janet Folina, in the present volume.

## 7. The Function of Structures: An Example from Weil

It must be emphasized that, maybe not for Bourbaki as a collective author but undoubtedly for Bourbaki as a pleiad of mathematical masterminds, structures and axiomatics were deeply linked with *analogies* and *intuitions*. This is remarkable since these two domains seem completely different, the first belonging to the formal world and the second, in this case, to creative imagination. However, the link is not so surprising if one takes into account that analogies are fundamental for discovering ways of solving "big problems." To explain this point further, let us consider one of the main examples of such problem-solving, namely the way in which André Weil—"primus inter pares" in Bourbaki—tackled the Riemann hypothesis. In his celebrated letter written in jail to his sister Simone (March 26, 1940), he described his procedure in natural language, thus leaving a rare and precious testimony of his way of thinking. Considering it will take us from Dedekind and Weber in the 19th to Alain Connes in the 21st century.[28]

### 7.1 The Initial Analogy by Dedekind-Weber

At the end of the 19th century, Richard Dedekind[29] and Heinrich Weber established a deep analogy between the theory of algebraic numbers and Riemann's theory of algebraic functions on algebraic curves over the field C of complex numbers (compact Riemann surfaces); see in particular their celebrated 1882 paper, "Theorie der algebraischen Funktionen einer Veränderlichen." One of their main ideas was to consider integers $n$ as kinds of "polynomial functions" over the set $P$ of primes, i.e., as "functions" globally defined and having a value and an order at every "point" $p$ of $P$. The "value" is $n$ modulo $p$, and the "order" is the power of $p$ in the decomposition of $n$ into prime factors. If the value at $p$ is not 0, the order is 0, and if the value is 0, the order is at least 1. This is evident because, if we write $n$ in base $p$, we get $n = p^{\text{order}(n)}(a_0 + a_1 p + \cdots + a_k p^k)$ with coefficients $a_k$ between 0 and $p - 1$, $a_0 \neq 0$ For smooth functions on manifolds in the ordinary sense, the values and the orders at the points are local concepts. To find the equivalent of these concepts in the analogy, Dedekind and Weber had to define localization in a purely algebraic manner. This is the origin of the modern (crucial) concepts of *spectrum* and *scheme* in algebraic geometry.

[28] For more details, see Petitot (2017).

[29] Dedekind is one of the founders of axiomatic and structural methods in mathematics: cf. Sieg and Schlimm (2017), and also the contribution on Dedekind, by José Ferreirós and Erich Reck, to the present volume.

In his letter to Simone, Weil describes this analogy very well:

> [Dedekind] discovered that an analogous principle enabled one to establish, by purely algebraic means, the principal results, called "elementary," of the theory of algebraic functions of one variable, which were obtained by Riemann using transcendental [analytic] means. (Weil, [1940] 2005, 338).

He adds:

> At first glance, the analogy seems superficial. . . . [But] Hilbert went further in figuring out these matters. (228)

The simplest elements of the analogy can be summarized in table 1.

## 7.2 Hensel's *p*-adic Numbers

The analogy becomes deeper when we introduce a local/global dialectic. On **C**, we have analytic functions with Taylor expansions in the neighborhood of any point *z*. To extend this fact to arithmetic, it was necessary to find the equivalent of the Taylor expansion of a "function" in the neighborhood of a "point" *p*. For integers, the situation is very simple. In the same way as a polynomial is its own Taylor expansion at every point, an integer is its own "Taylor expansion" (its expansion in base *p*) at every prime *p*. But there are more functions than polynomials, which have different and infinite Taylor series at different points. To find an equivalent in the Dedekind-Weber analogy, one has to consider expansions in base *p* of *infinite* length, i.e., generalized numbers $n = p^{\text{order}(\tilde{n})}(a_0 + a_1p + \ldots + a_kp^k + \ldots)$. Of course, such series are divergent (and therefore have no rigorous meaning) for the standard Archimedean metric on the integers. But they become defined and tractable if one introduces a new, quite strange, metric where the norm of $p^k$ is $1/p^k$ and tends toward 0 when $k$ goes to $\infty$.

This was the great achievement by Hensel with the invention of *p-adic numbers*. And exactly as **R** is the completion of **Q** for the natural Archimedean metric (via limits of equivalent Cauchy sequences), the *p*-adic numbers constitute a field $\mathbf{Q}_p$ of characteristic 0 that is the completion of **Q** for a specific ultrametric, non-Archimedean, *p*-adic metric. In Bourbaki's manifesto, "L'Architecture des mathématiques" (1948) Dieudonné emphasized (with a rather a posteriori conception of history) Hensel's unifying analogy:

> [In an] astounding way, topology invades a region which had been until then the domain *par excellence* of the discrete, of the discontinuous, *viz.* the set of whole numbers. (Bourbaki 1948, 228)

**Table 1** The analogy between prime numbers and points on a Riemann surface

| **Primes** | ⇔ | **Points** |
|---|---|---|
| Integers | ⇔ | Polynomials |
| Divisibility of integers | ⇔ | Divisibility of polynomials |
| Rational numbers (quotients of integers) | ⇔ | Rational functions (quotients of polynomials) |
| Algebraic numbers | ⇔ | Algebraic functions |

## 7.3 Mixing Algebraic and Topological Structures

Of course, with any of its natural metrics **Q** is naturally embedded, as a topological subfield, in its corresponding completions $\mathbf{Q}_p$ and **R** (remember the analogy with polynomials that are their own Taylor expansion at every point). With its induced topology, it is by construction a dense subfield of all its completions; but it must be strongly emphasized that all these topologies on **Q** are completely heterogeneous: as a set endowed with an algebraic structure of a field, **Q** is everywhere the same. Yet as a topological space it is completely different for every metric, since the relations of neighborhood are completely different. We meet here a very good example of mixed structure: a single algebraic system compatible with an infinite number of different metric topologies. And we see how rich the "mixing" of structures of different types can be.

## 7.4 Places and Weil's "Birational" Approach

From this perspective, **Q** appears as what is called a *global field* with an infinite number of incommensurable completions, while $\mathbf{Q}_p$ and **R** are called *local fields*. In this context, **R** is often interpreted as $\mathbf{Q}_\infty$, that is, as the completion of **Q** for an "infinite" prime. This is of course just a manner of speaking. To conceptualize this remarkable geometrical intuition of "points" for finite and "infinite" primes in arithmetic, the specialists have coined the term "place" and speak of finite and infinite places.[30]

[30] The geometrical lexicon of Hensel's analogy can be rigorously justified by using the concept of *scheme* that we have already evoked: (i) finite primes $p$ are the (closed) points of the spectrum Spec(**Z**) of the ring **Z**; (ii) the local rings $\mathbf{Z}_{(p)}$ of rationals without any power of $p$ in the denominator are the fibers of the structural sheaf O of Spec(**Z**); (iii) the finite prime fields $\mathbf{F}_p$ are the residue fields of the fibers of O; (iv) integers $n$ are global sections of O; (v) **Q** is the field of rational functions on

The Dedekind-Weber analogy between arithmetic and geometry goes much further. The spectrum Spec(**Z**) of **Z**, i.e., the space whose (closed) points are the primes $p$, is an affine space and not a projective space. If one wants to extend to arithmetic the analogy with projective (birational) algebraic geometry of compact Riemann surfaces and transfer some of its results (the Riemann-Roch theorem, the Severi-Castelnuovo inequality, etc.), one has to work with *all* places at the same time. Indeed, in projective geometry the point $\infty$ is on a par with the other points. Weil emphasized this insight strongly from the start. Already in his 1938 paper, "Zur algebraischen Theorie des algebraischen Funktionen," he writes that he wants to reformulate Dedekind-Weber in a birationally invariant way. In his letter to Simone, he explains the problem as follows:

> In order to reestablish the analogy [lost by the singular role of $\infty$ in Dedekind-Weber], it is necessary to introduce, into the theory of algebraic numbers, something that corresponds to the point at infinity in the theory of functions. (Weil [1940] 2005, 339)

## 7.5. The Adelic Perspective

Unifying Archimedean and $p$-adic places is the origin of Weil's "adelic" approach. The problem is to consider families of local data indexed by all places together and to look at the possibility of gluing them into global entities. A first simple idea would be to take the elements of the infinite Cartesian product Π of all the completions $\mathbf{Q}_p$ and **R**. This would be a good example of a complex structure constructed as a product of simpler structures; but this idea turns out not to be so interesting. As $\mathbf{Q}_p$ and **R** are fields, Π is a ring; and as $\mathbf{Q}_p$ and **R** are normed fields, Π is a topological ring (it is another example of mixed structures); but its topology is rather pathological in the sense that it is not locally compact, where one says that a topological space is locally compact when every point has compact neighborhoods. We meet here a typical example of a Bourbakian reflection on what can be a relevant "good" structure: it is not the most formally general structure, but the most functionally general structure suitable for a particular purpose.

Spec(**Z**) (i.e., of global sections of the sheaf of fractions of O); and (vi) Spec(**Z**) plus the infinite place $\infty$ is like the "projectivization" of Spec(**Z**). In this context, $\mathbf{Z}_p$ and $\mathbf{Q}_p$ correspond to local restrictions of global sections around the "point" $p$ of Spec(**Z**), analogous to what are called *germs* of sections in classical differential, analytic, or algebraic geometry. (For the Riemann surface **C**, they would correspond, respectively, to holomorphic functions on small disks around a point $z$ and on small *punctured* disks around $z$.)

The lack of local compactness can be fixed using the concept of *adele*, a notion derived from the notion of *idele* introduced by Claude Chevalley in class field theory and coined by Weil (adele = additive idele, and the multiplicative group $\mathbf{I}_{\mathrm{Q}}$ of ideles is recovered as the group $GL_1(\mathbf{A}_{\mathrm{Q}})$). The core idea is to use the "restricted" product $\mathbf{A}_{\mathrm{Q}}$ of the $\mathbf{Q}_p$ and $\mathbf{R}$, where "restricted" means that almost all components, except a finite number, of an adele are $p$-adic *integers*. (Restricted products were already used by Chevalley for the ideles.) $\mathbf{A}_{\mathrm{Q}}$ is a topological subring of Π, which has the fundamental advantage of being locally compact, because the ring $\mathbf{Z}_p$ of $p$-adic integers is compact in the locally compact field $\mathbf{Q}_p$. Of course, the global field **Q** is naturally embedded diagonally in $\mathbf{A}_{\mathrm{Q}}$. (One associates to any rational $r$ the adele $a$ all of whose components are $r$; $a$ is actually an adele since, for all $p$ not dividing its denominator, $r$ is a $p$-adic integer.) Due to the heterogeneity of the topologies induced on **Q** by its different completions, however, **Q** is naturally embedded in $\mathbf{A}_{\mathrm{Q}}$ as a discrete subfield.

## 7.6. Locally Compact Structures

Now, why is being locally compact so important? The pragmatic reason is that the additive structure of $\mathbf{A}_{\mathrm{Q}}$ is an abelian (i.e., commutative) locally compact topological group,[31] and such groups are naturally endowed with Haar measures (generalizing the Lebesgue measure on **R**), which allow integration and harmonic analysis. According to a theorem of Iwasawa,[32] this property belongs to the characterization of **Q** as a global field, the *arithmetic* of **Q** being correlated to the *analysis* of $\mathbf{A}_{\mathrm{Q}}$. As Alain Connes writes, referring to Weil (1967) and Tate (1950) in his "Essay on the Riemann Hypothesis" (2015, 5):

> It opens the door to a whole world which is that of automorphic forms and representations, starting . . . with Tate's thesis ["Fourier Analysis in Number Fields and Hecke's Zeta-Function," 1950] and Weil's book *Basic Number Theory*.

In chapter 9 of *Modern Algebra and the Rise of Mathematical Structures* (2004), Leo Corry discusses the fact that Weil's preference for a theory of integration à la Lebesgue on locally compact groups restrained the development of probability theory à la Kolmogorov. Indeed, the latter uses, e.g., for Brownian motion, measures, and integration theory on non-locally compact groups. In his

[31] Moreover, $\mathbf{A}_{\mathrm{Q}}$ has the deep property of being "self-dual" for Pontryagin duality, i.e., it is isomorphic to the group of its characters.

[32] The fact that the topological ring $\mathbf{A}_{\mathrm{K}}$ of adeles of a field **K** is locally compact, semi-simple (with trivial Jacobson ideal), **K** being cocompact in it, characterizes global fields.

autobiography, Laurent Schwartz testimonies that "Bourbaki stepped away from probability, rejected it, considered it to be unrigorous" (quoted in Corry 2004, 119). We see in this example how the selection of "good" relevant structures can depend heavily upon the "big problems" aimed at: the Riemann hypothesis is not Brownian motion.

## 7.7. The Rosetta Stone

His remarkable conceptual deepening of the Dedekind-Weber analogy enabled Weil to find a strategy for proving the Riemann hypothesis (RH) not for arithmetic, but for an analogous, more geometric world. Indeed, in characteristic 0 the only global fields are finite extensions **K** of **Q** (i.e., algebraic number fields). But there exist a lot of other global fields defined in characteristic $p$. They are the fields **K** of rational functions on algebraic curves over a *finite* field $\mathbf{F}_q = \mathbf{Z}/q\mathbf{Z}$ with $q = p^n$, $p$ prime. It is therefore natural, on the one hand, to try to transfer to these fields questions concerning algebraic number fields: Weil did it for RH. On the other hand, algebraic curves over a finite field must have something to do with algebraic curves over **C**, and it is also very natural to try to translate RH to their case. It is for this *intermediate* third world that Weil succeeded in proving RH. This was one of his greatest achievements. He overcame what he considered to be the main difficulty in the Dedekind-Weber analogy, namely: that the theory of Riemann surfaces is "too rich" and "too far from the theory of numbers," and that "one would be totally blocked if there were not a bridge between the two" (Weil [1940] 2005, 340). Hence his celebrated metaphor of the "Rosetta stone":

> My work consists in deciphering a trilingual text; of each of the three columns I have only disparate fragments; I have some ideas about each of the three languages: but I know as well there are great differences in meaning from one column to another, for which nothing has prepared me in advance. In the several years I have worked at it, I have found little pieces of the dictionary. (Weil [1940] 2005, 340)

## 7.8. The Riemann Hypothesis: From Hasse to Weil, Grothendieck, Deligne, and Connes

Before Weil, Emil Artin and Friedrich Karl Schmidt had already transferred the Riemann-Dirichlet-Dedekind zeta and $L$-functions from the arithmetic side to the side of algebraic curves over $\mathbf{F}_q$. In this new context, Helmut Hasse proved RH for *elliptic* curves. Then Weil proved it for all algebraic curves over finite fields using mixed technical tools, such as divisors, the Riemann-Roch theorem for the curves and their squares, intersection theory, the Severi-Castelnuovo inequality coming from the classical geometric side (characteristic 0), and crucially, Frobenius maps coming from characteristic *p* (see Cartier 1993). It is well known that the attempts to generalize to higher dimensions Weil's proof of RH for curves over finite fields led him to his celebrated conjectures; and to find a strategy for proving them has been at the origin of the monumental program of Grothendieck (schemes, sites, topoi, etale cohomology, etc.), culminating in 1973 with Deligne's proof.

But the original Riemann hypothesis remained, and still remains, unsolved. A few years ago, Alain Connes proposed a new strategy, consisting in constructing a new geometric framework for arithmetics in which Weil's proof in the intermediary case of curves over finite fields could be transferred by analogy. His fundamental discovery is that a way forward could be to work in a new "new world," namely the strange world of "tropical algebraic geometry in characteristic 1." In his 2015 essay he explains that the strategy is

> to find a geometric framework for the Riemann zeta function itself, in which the Hasse-Weil formula, the geometric interpretation of the explicit formulas, the Frobenius correspondences, the divisors, principal divisors, Riemann-Roch problem on the curve and the square of the curve all make sense. (Connes 2015, 8)

## 8. Conclusion: Structures and Mathematical Discovery

From Weil to Grothendieck and Deligne, and from Grothendieck to Connes, we see how crucial and permanent the long-term functional role of structural analogies as a method of discovery is. As Weil strongly stressed from the outset in his letter to Simone:

> *If one follows it in all of its consequences*, the theory alone permits us to reestablish the analogy at many points where it once seemed defective: it even permits us to discover in the number field simple and elementary facts which however were not yet seen. (Weil [1940] 2005, 339)

Thus, a structural clarification of an analogy yields more understanding and allows to go further.

Indeed, structures enable us to imagine strategies for solving hard problems. It is amusing to see how Weil used a lot of military metaphors—"find an opening for an attack (please excuse the metaphor)," "open a breach which would permit one to enter this fort (please excuse the straining of the metaphor)," "it is necessary to inspect the available artillery and the means of tunneling under the fort (please excuse, etc.)"—when explaining to his sister that finding a proof is actually a strategy. He added:

> It is hard for you to appreciate that modern mathematics has become so extensive and so complex that it is essential, if mathematics is to stay as a whole and not become a pile of little bits of research, to provide a unification, which absorbs in some simple and general theories all the common substrata of the diverse branches of the science, suppressing what is not so useful and necessary, and leaving intact what is truly the specific detail of each big problem. This is the good one can achieve with axiomatics (and this is no small achievement). This is what Bourbaki is up to. It will not have escaped you (to take up the military metaphor again) that there is within all of this great problems of strategy. (Weil [1940] 2005, 341)

This illustrates that Bourbaki's structures concern much more than mere "simple and general" abstractions. They have a functional role, a strategic and creative function, namely "leaving intact what is truly the specific detail of each big problem."

This pragmatic functionality of structures is really the key point for our purposes. Bourbaki was a group of creative mathematicians, not of philosophers. The true philosophical meaning of their structuralist approach is rooted deeply in their practice and must be extracted from there. To evaluate it, it is not sufficient to criticize their more or less clever or educated philosophical claims. The fundamental relation between, on the one hand, their holistic and "organic" conception of the unity of mathematics and, on the other hand, their thesis that some analogies and crossroads can be creative and lead to essential discoveries is a leitmotiv for Bourbaki since the 1948 manifesto, "L'Architecture des mathématiques." The continued insistence on the "immensity" of mathematics and on its "organic" unity; the claim that "to integrate the whole of mathematics into a coherent whole" (222) is not a philosophical question, as it was for Plato, Descartes, Leibniz, or "logistics"; the constant criticism of the reduction of mathematics to a tower of Babel juxtaposing separated "corners"—these are not vanities of philosophically ignorant mathematicians. They have a very precise technical function: to construct complex proofs navigating in this holistic, conceptually coherent world.

Hence: "The 'structures' are tools for the mathematician"; "each structure carries with it its own language"; and to discover a structure in a concrete problem "illuminates with a new light the mathematical landscape" (Bourbaki

1948, 227) (compare again the example of the locally compact adelic ring). Leo Corry has formulated this key point well:

> In the *L'Architecture des mathématiques* manifesto, Dieudonné also echoed Hilbert's belief in the unity of mathematics, based both on its unified methodology and in the discovery of striking analogies between apparently far-removed mathematical disciplines. (Corry 2004, 304)

And indeed, Dieudonné claimed:

> Where the superficial observer sees only two, or several, quite distinct theories, lending one another "unexpected support" through the intervention of mathematical genius, the axiomatic method teaches us to look for the deep-lying reasons for such a discovery. (Bourbaki 1948, 230)

Structures are guides for intuition and allow to overcome "the natural difficulty of the mind to admit, in dealing with a concrete problem, that a form of intuition, which is not suggested directly by the given elements, . . . can turn out to be equally fruitful" (Bourbaki 1948, 230). Thus for Bourbaki "more than ever does intuition dominate in the genesis of discovery" (228). And intuition is guided by structures.

After his 1948 manifesto, Bourbaki deepened this vision considerably. The structural systematization made by the *Éléments* allowed clarification of many difficulties, opened up good prospects, and led to fruitful angles of attack, which helped to solve difficult and entangled problems. In the combination of, on the one hand, systematizing and clarifying formal operations in the context of justification and, on the other hand, implementing proof strategies in the context of discovery rests, in our opinion, Bourbaki's main contribution. Thus the philosophical scope of Bourbaki's concept of structure goes far beyond its formal presentation in *Théorie des Ensembles*. Its coherence has to be found not in foundational issues, but in the extraordinary corpus of technical results the Bourbakists produced and inspired. To understand Bourbaki's "philosophy," one has to take seriously, and discuss philosophically, the statements, reflections, and testimonials concerning how they thought about the operational practice involving structures for the creative imagination in pure mathematics. Very few philosophers have addressed these issues.[33]

[33] A remarkable exception was Albert Lautman; cf. Heinzmann (2019) and Petitot (1987). Compare also Zalamea (2012) and Chevalley (1987).

## Acknowledgments

Some parts of this chapter are the result of collective work discussed in seminars, symposia, and a forum of the ANR/DFG project *Mathematics: Objectivity by Representation*. We are especially grateful to Jean-Pierre Ferrier, Alain Genestier, Erich Reck, Georg Schiemer, and David Stump for their many precious suggestions and numerous linguistic corrections, which enabled us to improve the text.

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