The Structuralist Mathematical Style : Bourbaki as a Case Study

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Abstract

In this paper, we look at Bourbaki's work as a case study for the notion of mathematical style. We argue that indeed Bourbaki exemplifies a mathematical style, namely the structuralist style.

1 Introduction

In his article in the *Stanford Encyclopedia of Mathematics* on mathematical style, Paolo Mancosu presents the challenge of developing an "epistemology of (mathematical) style":

> Are the stylistic elements present in mathematical discourse devoid of cognitive value and so only part of the coloring of mathematical discourse or can they be seen as more intimately related to its cognitive content? Mancosu (2017)

There is no doubt that there are stylistic elements in the *presentation* of mathematics. After all, writing and talking about mathematics is not purely a matter of manipulating formal symbols organized in a unique manner. It is another issue to determine whether there are stylistic features in *mathematics*. Asking the question brings us immediately to the *practice* of mathematics and all its aspects. One has to define a concept. One has to state a theorem. One has to prove a theorem. One has to construct a counter example. One has to find a

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method to compute a formula. Etc. More often than one might think, in most cases, there is no unique path to the solution to a given problem. Then, one has to write. One has to talk. And there are myriad presentations possible. There are many different ways to introduce and justify a definition, motivate and contextualize a theorem, write up a proof and even organize a computation. Of course the plurality of presentations is not unique to mathematics. Anyone who has to present and prepare some material is faced with similar challenges. Is there an element of style that would be intrinsic to mathematics? Or, at least, a style that would bring an epistemological dimension that cannot be dissociated from the style? Are there styles of definitions, styles that contain an inherent epistemological component? This is how I understand Mancosu's challenge. Thus, if it is taken up, its resolution has at least two parts. First, identify what constitutes the stylistic elements in mathematical knowledge, as opposed to methods, approaches, etc., or merely "color". Second, show that these stylistic elements have cognitive value and, again, are not merely "part of the coloring".

It seems a priori easy to identify what the "coloring" of mathematical discourse might be: it should be some kind of ornament that accompanies a discourse, but that does not essentially contribute to its cognitive content. The terminology itself brings us back to the arts, any art, be it music, painting, sculpture, dance, acting, literature, etc. This is the traditional association. If mathematical style is merely part of the coloring, it would be akin to literary style, even a special case of the latter, it would refer to a specific way of writing a mathematical presentation, dictated by esthetic choices that do not have an impact on the epistemological content of the mathematics itself. It may make a mathematical text clearer, more fun, more powerful, more enjoyable or what have you, but if we are in the realm of coloring, then it does not convey a specific epistemic content, it does not contribute to its justification. It could be completely removed and the mathematics would be in principle just as clear, just as right, just as justified. Underlying this conception of mathematical knowledge is the idea that the truths of mathematics are organized in an essentially unique logical network and that to know mathematics is to know this web of logical relations. Whatever is added to this network would be ornemental, for instance the use of pictures, of certain types of notations and symbols and, of course, the presence of texts that are not directly part of the logical deductions. However, as Mancosu points out himself, it is easy to find claims in the philosophical literature that there *are* mathematical styles, be they individual styles, national styles or epistemic styles. I refer the reader to the list he provides in Mancosu (2017).

This paper is an attempt to face Mancosu's challenge head on[1] by examining the case of Nicolas Bourbaki, the well-known collective of French mathemati-

[1]As I will make clear in the second section, I am not the first one to do so. One could go back to Gilles Gaston Granger's work on the notion in Granger (1968), also discussed in Mancosu's article. I will not use nor refer to Granger's work here, for it would take us too far from our main objective. I will, however, follow the steps of David Rabouin in Rabouin (2017).

cians that initiated an ambitious and influential undertaking in 1934 and that led to the publications of 28 volumes covering a large spectrum of modern mathematics — from set theory to algebraic topology[2]. The project was initially meant to provide a modern treatise on analysis, but quickly became something much larger, since Bourbaki decided to start from scratch and organize the material from an abstract standpoint. Bourbaki's work was extremely influential and contributed to the development of contemporary mathematics in many different ways. It had, in the 1960s, its supporters and detractors. Although the collective still officially exists and still organizes an important seminar held in Paris, what we will focus on in this paper is the work done by the first two generations of Bourbaki, namely the founding fathers and those who joined the collective after WWII. Our main claim is that *this* Bourbaki is a generic case of an *epistemic* mathematical style. We also claim that this style is a direct consequence of a very specific conception of mathematics, its nature, organization and articulation, namely the structuralist style. Given these goals, the plan of our paper is straightforward. We will first propose a general definition of mathematical style. Then we will take a close look at Bourbaki's mathematics. We will then step back and try to explain what we mean by the structuralist style.

2 The Notion of Mathematical Style

As a first approximation, I submit that a mathematical style is a *systematic way of doing* mathematics which is then represented in its presentations. More precisely, it is a global and systematic pattern of choices that are made to define concepts, prove or disprove theorems, solve problems, compute formulas. Note that we are within a set of goal-oriented activities. For this approximation not to be a platitude, I have to put some flesh around the bones. By systematic, I mean that the way of doing is repeated, thus is identifiable and used more than once. A style, be it mathematical or otherwise, cannot be a fluke, a singular manifestation of a behavior. It has to be a way of behaving, of doing, of making that is a variation or a series of variations around an identifiable pattern, even though the latter might be hard to define. But even for this to be possible, I claim that the following conditions have to be satisfied. In order to have a mathematical style, there has to be:

1. A "standard", a way of doing mathematics against which the alternative style is contrasted; most of the time, this standard is implicit and is not recognized as such by the practitioners;

2. A combination of patterns of behavior that deviate significantly from the standard;

[2] It is hard to determine the exact number of volumes, particularly in the original publications, since some were published in parts and then in complete volumes. The best source of information about the origins of the collective and its early work is still found in Beaulieu (1990). See also Beaulieu (1994). Other important sources are Corry (1992, 1996, 2001, 2009). Corry's work is extremely valuable and stimulating.

3. A systematic and voluntary use of these patterns; these patterns have to be used and sought in all possible cases. They are not adopted as a mere option that can be discarded at will and they are not practiced without the practitioner being willingly aware of them; it has to be implemented as a conscious value in and for itself.

Some comments and clarifications are required. The first condition, namely the existence of an implicit (or explicit) standard seems to me to be inescapable. A style, to be identified as a style, has to be distinctive in one way or another and for this to be possible, there has to be something that it is distinguished from.

A style, to be recognized as a style, has to deviate significantly from a (implicit or explicit) standard. It is, of course, difficult to qualify in general what 'significantly' means precisely. Within a given practice, there are variations. These variations by themselves do not yield nor do they constitute a style, but they might the precursor to a style. The expression 'combination of patterns of behavior' refers to ways of doing that are guided or systematic, that follow a pattern. Of course, it is not a method nor a combination of methods in the sense of an algorithm or algorithms. For a style is fluid, changing within a certain range or space of variations, but also rigid enough so that it can be recognized as such.

Last but not least, for a style to be a style, the patterns of behavior underlying it have to be consciously adopted and applied in all possible cases, even those that might seem outside the original scope of these behaviors. One subtle point has to be made about the voluntary aspect. It is not that the person who is adopting these patterns is aware that she is adopting a style — for she might not think of it in these terms —, but she has to be aware that the patterns of behavior she is adopting *are* deviant from the standards of the community. It is entirely possible that she is simply adopting the patterns of behavior that seem to her to be the best or most effective given her goal, what she knows and what she can do. In other words, sometimes a style comes naturally and is not seen as being the result of a conscious effort to behave the way that person behaves. But in the eyes of others, it is definitely a style.

Notice that a style is intrinsically historical. It appears at some point and can, and usually does, disappear at another point. But it has to last sufficiently long so that it can be identified as such. It can also become the new standard for a given community and thus lose its status as a style if the new generations are taught to do mathematics by adopting these behavioral patterns.

We propose a more precise definition of mathematical style. Let us fix a few conventions. First, by an agent α, we refer to the author of a piece of mathematics, be it an individual, a group of individuals, a collective, etc. Second, by a cultural context γ, we refer to the accepted norms, implicit or explicit, in a given community, that dictate how a certain activity has to be performed or is usually performed. Needless to say, there are specialized cultural contexts, e.g. homological algebra, descriptive set theory, etc., as well as more global cultural contexts, number theory, algebra, analysis, even mathematics as a whole.

Third, by patterns of definitions δ, we refer to ways of using a language, spoken or written — we use the term 'language' in a broad sense, including diagrammatic, visual, symbolic, etc., conventions —, even introducing a new language and using it in a certain manner. Fourth, by patterns of inference ι, we refer to ways of arguing based on the choice of linguistic and/or symbolic devices made, that is the choice of δ. Contrast and compare, for instance, the way mathematicians can now define the product of two sets X and Y. In the language of set theory, one defines the Cartesian product in the usual fashion, that is as a set containing the ordered pairs (x, y), with $x \in X$ and $y \in Y$. In the language of category theory, a product of two sets is defined as being an object P together with two morphisms p_X and p_Y satisfying the usual universal property. These choices then determine to a certain extent the patterns of inference one can use and will use, even though there is still room for variations in the patterns of inferences employed within both contexts. We can now give our definition.

We say that a corpus of mathematics μ, embodied in books, papers, talks, etc., produced by an agent α exhibits or has an *epistemic style* σ in the cultural context γ if and only if σ is a systematic way of solving problems that rests upon:

i. specific and systematic patterns δ of definitions that differ significantly from the standards of γ;

ii. specific and systematic patterns ι of inference that differ significantly from the standards of γ;

iii. combinations κ of components of δ and ι in the solution of problems, the organization of concepts, results and relations between the parts of μ that differ significantly from the standards of γ.

This gives a general definition of a mathematical style, but it does not provide the features of a *particular* mathematical style. To get the latter, one has to define δ, ι, their combinations κ and specify how they deviate from the standards of γ.

Let me immediately illustrate this definition by a concrete example, which I hope will be useful. I claim that members of the contemporary community of logicians, mathematicians and computer scientists who are developing and using homotopy type theory could end up practicing a new style of mathematics in the foregoing sense. I cannot, of course, describe homotopy type theory in such a short paper[3]. I will sketch the main elements that I believe can justify my claim.

First, homotopy type theory can be used systematically to do mathematics, to solve mathematical problems. It differs as such significantly from the stan-

[3]See Collective (2013) for a presentation of the theory and how mathematics is developed within it. Of course, homotopy type theory is also presented as a new foundational framework. As a consequence, it is taken to be global and systematic, which are two elements that are crucial to our approach. I underline again that the 'global' is always relative to a community. It could be all of homological algebra, or all of algebraic geometry, but not all of mathematics, for instance.

dards used, implicitly or explicitly, by the contemporary community of mathematicians. The language of homotopy type theory is not based on the standard universe of sets or a variant thereof. Classical mathematical entities are defined by using new means of definitions and theorems and computations are obtained by novel inferential and computational patterns. Classical constructions and concepts, e.g. sets, the homotopy groups, the Hopf fibration, Eilenberg-Mac Lane spaces, etc., are defined in novel ways and proofs of theorems have to go through new paths (no pun intended). See Licata and Finster (2014); Rijke and Spitters (2015); Buchholtz et al. (2018) for some examples.

I want to emphasize that we need not have such a well-defined, formal framework to characterize a singular mathematical style. In fact, as it develops, the mathematical practice based on homotopy type theory might become a mixture of purely formal, computational mathematics, checked by computers, and informal expositions containing the main mathematical ideas involved in the computations. However, the latter does not constitute its style. Its style resides in the patterns of definitions, patterns of inferences and their combinations in the solutions of mathematical problems. It is not tied up to specific axioms, e.g. the univalence axiom, of homotopy type theory, but rather basic methodological features built into it. Thus, some of the technical, formal aspects of homotopy type theory might be modified, even abandoned, and the style could still be present. The style is not attached to the specific (univalent) foundational framework presented and explored, but rather to the language, the manners of defining, proving and calculating that can be kept apart from the specific formal framework.

To make sure that our example does not mislead the reader in thinking that our definition of mathematical style applies only to formalized mathematics and formalized theories, let me fall back on a recent analysis of the notion of mathematical style proposed by David Rabouin. Even though Rabouin does not give a general definition of the notion of mathematical style in his paper Rabouin (2017), his approach is close to ours in many respects and has, in fact, inspired ours[4].

Based on Chevalley's paper on mathematical style, Rabouin identifies a mathematical style with a way of writing that inflects mathematical thought. After pointing out that Chevalley does not give a definition of mathematical style, Rabouin presents Chevalley's position thus: "...he [Chevalley] merely states that one can identify general tendencies in ways of writing mathematics..."(Rabouin, 2017, 142), and then quotes Chevalley saying that there are "revolutions that inflect writing, and thus thought."(Rabouin, 2017, 145) There are other elements that are implicitly included in Rabouin's analysis. Two features have to be underlined, for they are directly tied to our analysis. The first component has to do with patterns of inferences, which he mentioned in an example:

> When Poincaré used the ϵ-style, it was not because he shared a

[4]There are also parallels with Kvasz (2008), but we will not expand on this particular point here.

> certain conception with Weierstrass of what the objects (...) involved in this manipulation were and about the good (in this case 'rigorous') delineation of the theories, *but because this way of writing allowed some powerful inferences that were not possible in the previous style.* (Rabouin, 2017, 148)[my emphasis]

Finally, another relevant element comes up when he discusses the Cartesian style:

> Both Descartes's and Fermat's methods rely on a kind of inferential black box coupled with geometrical reasoning. This allows us to give a more precise characterization of the Cartesian style (at least for one important aspect): its core is not the use of algebra in and of itself (which existed long before Descartes and Fermat) but *the coupling of specific kinds of computational inferences with geometrical ones*. In this sense, one can say that the Cartesian style of geometry, even if it did not suddenly disappear, took a dramatic turn around 1750 with the first formulations, which, as later emphasized by Joseph-Louis Lagrange, were free from any diagrammatic inferences — Leonhard Euler (1748) can be considered a starting point here. (Rabouin, 2017, 154)[my emphasis]

It is not only the computational inferences but also the geometrical inferences that we want to underline here, which we include in the patterns of inferences contained as an intrinsic part of the language or the writing.

Rabouin gives also the examples of Leibniz's style of (transcendental) geometry, set theory as a language (as opposed to formalized set theory) and the Euclidean style of geometry[5] as examples of his notion of mathematical style.

As emphasized by Rabouin, a style can be adopted for a variety of reasons, even incompatible reasons, and these reasons are not necessarily philosophical. For some, it might be associated with a specific ontology. To others, it might be seen as a consequence of a chosen epistemology. It is even conceivable that some see in it an ideological or political component. Finally, it might simply be more effective than another way of solving certain problems. The main point here is that the style is not *defined* by only a common ontology or a common epistemology, etc.

I will now try to show that Bourbaki is an exemplar of the notion of a mathematical style[6].

3 Bourbaki's Style

Bourbaki is particularly interesting when looked at from the point of view of the notion of mathematical style. The fact is, we could use the expression "Bourbaki's style" in three different senses.

[5] At least as interpreted by Ken Manders in his Manders (2008).

[6] I am using the term 'exemplar' in a sense similar to Kuhn's usage in his postscript of the second edition of Kuhn (1970).

1. Bourbaki had a unique method of work; it was a collaborative effort unlike any other before and, as far as I know, ever since[7]. This in itself deserves to be called “Bourbaki’s style of work”.

2. Bourbaki developed a unique, terse way of presenting mathematics which even became known as “Bourbaki’s style”. We can therefore talk about “Bourbaki’s presentation style”.

3. Finally, and most importantly for our project, Bourbaki’s modes of development of mathematics itself, centered on a certain notion of structure and of how to do mathematics in a structuralist fashion. It is of course at this level that our characterization of the notion of mathematical style ought to apply to Bourbaki. Thus, there is “Bourbaki’s structuralist style”.

These three senses of styles are not independent. The third, namely the mathematical style as such, emerged in part from the first, Bourbaki’s method of work. The second, the writing style, is a direct consequence of the third and the first components. We will look at these three senses in turn. But before we do so, we have to provide a minimum amount of information about Bourbaki, for it is an essential part of the context.

3.1 Bourbaki: a very short description of the group and the project

Bourbaki was famous among mathematicians, and intellectuals in general, from the 1960s until the beginning of this century approximately. The new generation of philosophers, logicians and mathematicians have very little knowledge of who they are, what they did and why it was important, and thus it seems appropriate to give a short presentation of the group[8].

André Weil (1906-1998), Henri Cartan (1904-2008), Claude Chevalley (1909-1984), Jean Delsarte (1903-1968), Jean Dieudonné (1906-1992), René de Possel (1905-1974), a group of young and ambitious mathematicians, all former students from the *École Normale Supérieure* in Paris, an élite school, met for the first time in December 1934 to discuss the idea of writing together a modern textbook in analysis. Except for Claude Chevalley, the youngest member of the group, they were all university professors who found that they did not have at their disposal a decent textbook to work with and Weil convinced them that the best solution was simply to write one. They certainly did not know then that they had just set in motion a unique collaborative enterprise that would not only last well after their withdrawal from the group, but that would also

[7]The closest I can find nowadays are The Stack Project in algebraic geometry, the nLab in higher dimensional category theory and Gowers’s Polymath Project. But they all differ in one way or another from Bourbaki’s work. See `https://stacks.math.columbia.edu/about` for the Stack Project, `https://ncatlab.org/nlab/show/HomePage` for the nLab and `https://en.wikipedia.org/wiki/Polymath_Project` for the Polymath project.

[8]There is nothing original in this section. The interested reader can consult Beaulieu (1990, 1994); Corry (2004, 2009); Houzel (2004); Mashaal (2000) for more.

have a deep impact on the face and development of mathematics in the 20th century.

They were well aware that mathematics was changing and that Hilbert and his school were promoting the axiomatic method in mathematics. Many of them had visited Göttingen, Berlin, Hamburg, Frankfurt, Munich, Rome, Stockholm, Zurich, Copenhagen, Princeton (to mention but the most important places) during their graduate studies or afterwards. They had all read Van der Waerden's *Moderne Algebra* and it had a great impact on them.

The first "extensive" meeting took place in the summer of 1935[9]. The composition of the group changed somewhat in the meantime and would change again in the following fall. We will not follow the exact composition of the group through time. Suffice it to say that Weil, Cartan, Chevalley, Delsarte and Dieudonné formed the core of the group for the first 20 years or so. Charles Ehresmann (1905-1979) joined the group in the fall of 1935 and left in 1947. After WWII, Laurent Schwartz (1915-2002), Pierre Samuel (1921-2009), Roger Godement (1921-2016), Jean-Louis Koszul (1921-2018), Armand Borel (1923-2003), Jean-Pierre Serre (1926-), Alexandre Grothendieck (1928-2014) and Pierre Cartier (1932-) joined the collective at some point. Samuel Eilenberg (1913-1998), one of the fathers of category theory, became a member in 1950. All the members were creative mathematicians who all had respected individual careers. All of them nonetheless said that being members of Bourbaki and working together had a deep influence on their individual work[10].

The original plan was simple enough: write a modern textbook on analysis. It became clear that they needed to start with what they called an "abstract packet", which included set theory, general topology and algebra as it was then known. Notice that these three disciplines were being created at the time. Indeed, Bourbaki contributed to their evolution and stabilisation[11]. What was supposed to be merely an introductory chapter rapidly became a large undertaking. Bourbaki first published a fascicle of results on set theory in 1939. It was not, as such, a textbook, for it contained no proofs. They decided to publish it nonetheless, since many of the results on sets were to be used in subsequent volumes. The complete volume on set theory finally came out in two parts, one published in 1954 and the other in 1957. The last one contains the chapter on structures. Notice how long it took them to finally get it published: almost 20 years. The volume on sets and structures has a tortuous history and it went through numerous versions[12]. It might be worth pointing out that the *general* notion of structure was not in Bourbaki's mind in 1935. It showed up for the first time during the meeting held in the summer of 1936, but as an undefined concept. It then went through various presentations and the final, published

[9]In fact, the group had met every two weeks during the winter and the spring of 1935. The summer meeting was an intensive session where they hoped to do more work together.

[10]In a late interview, Henri Cartan declared: "In Bourbaki I learned very much. Almost all I know in mathematics I learned from and with the Bourbaki group."(Jackson, 1999, 785)

[11]Suffice it to say that the axioms of topology in terms of open sets came directly from Bourbaki. So does the notation for the empty set, $\emptyset$, among other things.

[12]It is now possible to consult these versions on line, since the early documents have been digitized and made available on the site http://sites.mathdoc.fr/archives-bourbaki/

version, did not satisfy the group, for reasons that we will clarify later.

Despite the fact that they had to modify the original project in numerous ways and even, at some point, to scale it down, Bourbaki started publishing books as early as 1940. The first volume contained the first chapters of *General Topology*, quickly followed by the first chapters of *Algebra* in 1942. Subsequent chapters on topology and algebra follow in 1947 (topological groups, linear algebra), 1948 (multilinear algebra, real numbers), 1949 (functions of a real variable, functional spaces) and in the 1950s, they basically published a volume a year, up to the theory of integration. It was an intensive undertaking, ambitious and systematic. No single author could have done that. Even for a distinguished group, and especially given their method of work, it is remarkable that they succeeded in doing anything.

3.2 Bourbaki's method of work

Team work is neither easy nor simple[13]. A large amount of trust and respect has to exist between the members for anything to be done. There also has to be an agreement as to what the final goal is, otherwise the group spends countless hours wasting time discussing that goal. In Bourbaki's case, the target was clear at the beginning, but it changed as the work developed. Somehow, the original members agreed on a method of work and it led to the publications mentioned. The method was brutal. Here is how Dieudonné presented it later[14].

> The work method used in Bourbaki is a terribly long and painful one, but is almost imposed by the project itself. In our meetings, held two or three times a year, once we have more or less agreed on the necessity of doing of book or chapter on such and such a subject (...), the job of drafting it is put into the hands of the collaborator who wants to do it. So he writes one version of the proposed chapter or chapters from a rather vague plan. Here, generally, he is free to insert or neglect what he will, completely at his own risk and peril, ... After one or two years, when the work is done, it is brought before the Bourbaki Congress, where it is read aloud, not missing a single page. Each proof is examined, point by point, and criticized pitilessly. One has to see a Bourbaki Congress to realize the virulence of this criticism and how it surpasses by far any outside attack. (...) Once the first version has been torn to pieces – reduced to nothing – we pick a second collaborator to start it all over again. This poor man knows what will happen because although he sets off following new instructions, meanwhile the ideas of the Congress will change and next year *his* version will be torn to bits. A third man will start, and so it will go on. One would think it was an endless process, a

[13]For more on Bourbaki's method of work, the reader can consult the references given in the previous footnotes.

[14]Other original members have provided similar descriptions and later members concurred. See, for instance Guedj (1985) or Cartan (1979).

> continual recurrence, but in fact, we stop for purely human reasons. When we have seen the same chapter come back six, seven, eight, or ten times, everybody is so sick of it that there is a unanimous vote to send it to press. This does not mean that it is perfect, and very often we realize that we were wrong, in spite of all the preliminary precautions, to start out on such and such a course. So we come up with different ideas in successive editions. But certainly the greatest difficulty is in the delivery of the first edition. (Dieudonné, 1970, pp. 141-142).

The result was perhaps not perfect, but very few books are written in that way and go through such a rigorous editing process. Although not faultless, the final result was certainly better is some ways than what it would have been had it been the product of a single individual. Definitions were weighed, proofs were criticized, the organisation of theorems was analyzed, the overall network of concepts and results was evaluated by first-rate mathematicians. The result was something unique. There is one important component of the method that Dieudonné did not underline. As Chevalley later put it: "This allowed our work to submit to a rule of unanimity: anyone had the right to impose a veto. As a general rule, unanimity over a text only appeared at the end of seven or eight successive drafts."(Guedj, 1985, 47) Majority was not enough. If only one member thought that a manuscript was not good enough, it had to be rewritten. Like I said, it was a brutal process.

This mode of collaboration certainly played a role in the redactions of the volumes published over the years. It contributed in an essential way to the construction of the presentation of the material and its organization. For, when one looks at the works of its individual members, it is clear that there are differences between what Bourbaki published and what they published, even when some of the members produced expository material. Chevalley, for instance, is more radical than Bourbaki in some ways. I cannot refrain from quoting a long passage from a review of Chevalley's textbook on algebra, Chevalley (1956), written by Mattuck:

> Chevalley has written a text-book, and his mathematical personality permeates every paragraph. [...] The book is tight, unified, direct, severe; relentlessly and uncompromisingly it pursues its ends: out of the simplest basic notions of algebra to build up with perfect precision the theory of multilinear algebras which have found applications in topology and differential geometry. [...] The unity is monolithic. Gone is the discursive rambling of previous texts. This one marches unswerving and to its own music. [...]
>
> The general approach to the subject matter is that of Bourbaki's first three algebra chapters, but there are significant differences in content and treatment (Chevalley is often more general). *As for the style, Bourbaki emerges from the comparison a warm, compassionate, and somewhat elderly gentleman.* (Mattuck, 1957, 412)[my emphasis]

Mattuck directly refers to Bourbaki's style or presentation and compares it to Chevalley's. There is no doubt that to characterize Bourbaki's style of presentation as "warm, compassionate, and somewhat elderly" was deeply ironical. To most readers at the time, Bourbaki was anything but warm, compassionate and somewhat elderly! Mattuck's description of Chevalley's book as "tight, unified, direct, severe" is precisely what its contemporaries would have claimed of Bourbaki's books. Chevalley was pushing it even further.

Weil, on the other hand, wrote books that are definitely not in Bourbaki's style, at least not in the sense that I am using the term. For instance, in his review of Weil's *Foundations of Algebraic Geometry*, Oscar Zariski underlines the fact that "It is a remarkable feature of the book that — with one exception (Chap. III) — no use is made of the higher methods of modern algebra. The author has made up his mind not to assume or use modern algebra 'beyond the simplest facts about abstract fields and their extensions and the bare rudiments of the theory of ideals'."(Zariski, 1948, 671) Zariski himself claims afterwards that "we may just as well help ourselves to modern algebra to the fullest possible extent", a claim certainly consistent with Bourbaki's style. And then, he goes on, this time talking about Weil's writing itself: "To achieve his objectives Weil wages a campaign of the Satz-Beweis type. Most readers will find it difficult to follow the author through the seemingly endless series of propositions, theorems, lemmas and corollaries (their total must be close to 300)."(Zariski, 1948, 674). Thus, it can be claimed that, although the choice of exposition made by Weil was close to what one finds in Bourbaki — the Satz-Beweis type —, the patterns of definitions and inferences were not. In fact, as we will argue, there is another important aspect of Bourbaki's style that Weil does not quite follow to its natural conclusion in his work.

It is important to note that Bourbaki's volumes are expository. They are not research monographs, even though some of them include some very recent developments at the time of their writing. But I do not believe that the analysis that I propose is limited to these expository works. It can and was adopted by some of Bourbaki's members. I would claim, for instance, that Chevalley and Grothendieck both have produced mathematics that exhibit Bourbaki's style, although in the case of Grothendieck, it is a structuralist style that is a variant or an extension of Bourbaki's. These are empirical claims that will have to be established by looking at their work if my analysis holds any water.

Let us now briefly look at the mode of presentation of the material chosen by Bourbaki. We first want to emphasize one aspect that, although important in the organization and the presentation of the material, does not constitute, in my opinion, an essential aspect of Bourbaki's structuralist style.

3.3 Bourbaki's writings

Every book of Bourbaki's *Éléments de mathématique* comes with a user guide[15]. They all open with a warning "To the Reader". The first paragraph goes like

[15]Once again, I do not claim any originality in this section. But it is essential to untangle different components present in the writings.

this:

> 1. This series of volumes, [...], takes up mathematics at the beginning, and gives complete proofs. In principle, it requires no particular knowledge of mathematics on the reader's part, but only a certain familiarity with mathematical reasoning and a certain capacity for abstract thought. [...] (Bourbaki, 2004, v)

It is no accident that Bourbaki insists right from the beginning on a 'certain capacity for abstract thought.' We will argue that it is in fact a crucial part of Bourbaki's mathematical style. The next paragraph goes into more detail.

> 2. The method of exposition we have chosen is axiomatic and abstract, and normally proceeds from the general to the particular. This choice has been dictated by the main purpose of the treatise, which is to provide a solid foundation for the whole body of modern mathematics. For this it is indispensable to become familiar with a rather large number of very general ideas and principles. Moreover, the demands of proof impose a rigorously fixed order on the subject matter. It follows that the utility of certain considerations will not be immediately apparent to the reader... (Bourbaki, 2004, v)

Notice that this solid foundation rests on the abstract axiomatic foundation, not explicitly on logic and set theory, although the first volume is indeed on logic and set theory. They certainly play a role and are part of the style, but it is clear that the weight is placed on the abstract axiomatic method which is grounded on them. Logical aspects of the volumes are nonetheless identified immediately. Logic plays two important roles in the enterprise. The first one is global and described in paragraph 4:

> 4. This series is divided into volumes (here called "Books"). The first six Books are numbered and, in general, every statement in the text assumes as known only those results which have already been discussed in the preceding volumes. This rule holds good within each Book, [...]. At the beginning of each of these books (...), the reader will find a precise indication of its logical relationship to the other Books and he will thus be able to satisfy himself of the absence of any vicious circle.

Thus, there is a global logical organization of the whole books. It is systematic and coherent.

The second one is local and shows up in the following paragraph.

> 5. The logical framework of each chapter consists of the *definitions*, the *axioms*, and the *theorems* of the chapter. These are the parts that have mainly to be borne in mind for subsequent use. (Bourbaki, 2004, vi)

This is now a specific mode of presentation, based on a logical framework. And indeed, anyone who has looked at and studied mathematics by reading Bourbaki is struck by the following facts, which make it hard not to fall back on Mattuck's adjectives. The presentation can only be qualified as being extremely dry, severe, austere, unified and terse. There are no images, no informal motivations or descriptions, no explanations of the value of this theorem or that definition. But at the same time, it is clean, elegant, and efficient. As some say that there are no unnecessary notes in Mozart's music, there are no unnecessary definitions, axioms, theorems, lemmas and examples in Bourbaki's mathematics. Another comparison readily comes to mind: Bourbaki's organisation of the material is akin to the plans of the architects of the Bauhaus school and their students.

Here is how Cartan described these components in 1958, at the heyday of their production. Not surprisingly, we find the same elements contained in the note to the reader.

> All the books of part I are arranged from a strictly logical point of view. A concept or result may be used only if it has appeared in a previous chapter of a book. Obviously, one has to pay a high price for such rigor: the resulting presentation tends to become somewhat ponderous. The reader finds its weightiness repellant, and the style is certainly not what one would call inspiring. The mathematical text consists of a series of theorems, axioms, lemmas, etc. This rigorous, precise style stands in sharp contrast to the light and not too precise style of the French tradition at the end of the last century. [...] Today it is apparent that this precise style is finding its way more and more into mathematical literature. Cartan (1979)

Nothing is presupposed. Everything is defined from scratch and thereafter, the proofs all depend on notions and theorems already given and proved. This is an adequate characterization of Bourbaki's expository style. But I argue that it does *not* give us, as such, Bourbaki's mathematical style.

In a different paper, written much earlier, Cartan makes the following remarks about the logical component and the epistemic component of the axiomatic method:

> Now suppose these axioms chosen once and for all. Our mathematical theory must not restrict itself to be a dull compilation of truths, that is of consequences of axioms that we note, for each and every one of them, laboriously the accuracy. For mathematics to be an effective instrument and, also, for us, mathematicians, to be able to take a true interest in it, it must be a living construction: one must clearly see the web of theorems, group the partial theories. In this task, *it is again the axiomatic method that comes to our help*, by giving us the *principle of classification.* [...] Today, more and more we tend to study algebraic structures, topological structures, and ordered structures, etc. [...]

> Thus, not only the axiomatic method, based on pure logic, gives a steadfast seat to our science, but it also allows us to organize it better and to understand it better, it makes it more effective, it substitute general ideas to "computations" that, carried out haphazardly, would most likely lead nowhere, unless done by an exceptional genius. (Cartan, 1943, 11) [my translation and emphasis]

Thus, we have to distinguish the logical dimensions of the axiomatic method from the epistemic dimensions. The epistemic dimensions are built upon the logical ones. A purely logical presentation of mathematics already existed when Bourbaki wrote their books: it was given by Russell and Whitehead *Principia Mathematica*. Granted, it was not based on sets, and in some respects it was a failure, but it certainly was rigorous, austere and precise. Bourbaki and some of its members did publish on the logical foundations of mathematics[16] And Bourbaki did claim that he wanted to derive the whole of mathematics from the axioms of set theory[17]. It is clear that there is a polemical element present in these papers, in particular the first two. Indeed, they present the foundational program in the spirit of Hilbert's answer to Brouwer. It is therefore tempting to reduce Bourbaki's project to its logical development. We believe that this move is, however, far too quick. For one thing, Bourbaki did not want to include logic in their project at first. And Bourbaki always looked at logic as a mere instrument, as providing the proper grammar of mathematics.

3.4 Bourbaki's style

Before we apply our general definition of mathematical style to Bourbaki, we first have to present and discuss Chevalley's article published in 1935 and entitled "Variations of mathematical style", Chevalley (1935), in the *Revue de Métaphysique et de Morale*[18]. Interestingly, while Bourbaki was coming to life, one of its members published a paper in a philosophy journal that discusses precisely the notion of mathematical style[19].

3.4.1 Chevalley on mathematical style

As we have already indicated, when discussing Rabouin's analysis of the notion of mathematical style in the foregoing section of our paper, Chevalley does not

[16]See Dieudonné (1939); Cartan (1943); Bourbaki (1949).

[17]This is explicit in Bourbaki (1949).

[18]The title is *Variation du style mathématique* in French.

[19]Chevalley is a very interesting case. Not only was he a brilliant mathematician, but he was also interested in politics, philosophy and the foundations of mathematics. As he himself revealed later, he was solely responsible for the inclusion of logic in Bourbaki's books. One of his best friends was Jacques Herbrand, a brilliant young logician who unfortunately died in a hiking accident in 1931 at the age of 23. Albert Lautman, the philosopher of mathematics who was killed by the Nazis in 1944, was also one of his good friends. Later in his life, Chevalley joined Grothendieck and founded the movement *Survivre et vivre* in Montreal during the summer of 1970. It might also be worth mentioning that Chevalley studied under Emil Artin in the early 1930s and then with Helmut Hasse, both vigorous developers of what was then called the "axiomatic method".

give a general definition of mathematical style. He identifies three different mathematical styles in his paper: the style based on infinitesimals, the ϵ-style and the axiomatic style. Each one of these is characterized by contrasting it with the preceding style.

Chevalley opens up his paper by saying that he is not interested in the *personal* style of some mathematician, but rather the style of a period, a general tendency that becomes the norm under the influence of certain individuals. To illustrate what he means, he presents the "ϵ-style", a style forged under the influence of Weierstrass.

The ϵ-style itself has a history and became the norm when infinitesimals were seen to lead to difficulties. Thus, the desire to bring rigor into some mathematical demonstrations, in particular those involving infinitesimal quantities, brought about important changes in the practice of mathematics. Not that infinitesimals were not useful; thinking and doing mathematics with infinitesimal quantities was fruitful, even fertile. But their use needed some justification and it opened the door to some anomalies, for instance Weierstrass's discovery of a continuous real function in one variable that is nowhere differentiable, but which can be defined nonetheless by an ordinary looking Fourier development. It appeared that something was wrong somewhere. Similar functions could show up in classical analytic theories without notice. It was by trying to clarify the foundations of these infinitesimal quantities that a new mathematical style emerged. This style, according to Chevalley, can be identified by certain obvious traits.

As its name indicates, the usage, sometimes immoderate according to Chevalley, of various ϵ, with indices, is the most obvious feature of that style. The progressive replacement of equalities by inequalities in proofs, theorems, etc. is the second sign. Notice immediately that the components of the style identified by Chevalley are argumentative strategies, ways of proving that are brought in to make mathematics more rigorous. Although he does not mention what we called 'means of definitions' explicitly, he certainly could have done so.

According to Chevalley, it is precisely this reliance on inequalities that inevitably lead to the limitation of that style and the need to develop a different style.

> Indeed, while equality is a relation that makes sense for arbitrary mathematical beings, inequality can only bear upon objects provided with a certain order, in practice only on real numbers[20]. This therefore leads, in order to embrace the whole of analysis, to the complete reconstruction from real numbers and functions of real numbers. ... One could believe at some point that mathematics would constitute itself in a unitary domain, founded entirely by constructive definitions from the real numbers. ((Chevalley, 1935, p. 379)) [Our translation]

[20]It is interesting to see that Chevalley does not consider abstract ordering structures at that point. He did not know about Birkhoff's or Ore's work at the time. See Corry (2004) for more on the latter. Bourbaki will later on think of order structures as fundamental to mathematics.

He simply states that this unification did not happen. For, some mathematical concepts cannot be constructed from the real numbers, for instance the concept of group. Geometry, although it can be constructed to a certain extent in the ϵ style, becomes somewhat ad hoc or artificial. The nature of points, as n-tuples of real numbers, is not essential to geometry, as Klein had conclusively shown. It is the group of transformations of a geometry that provides the equality of figures inherent to that geometry, not the equality of points. Thus, in some cases, constructive definitions provided by analysis *hide* the real nature of what they were trying to define.

Chevalley then states that geometry provided, in fact, the material of what was to become the new style. He attributes the emergence of this way of doing mathematics to Hilbert's work in geometry[21]. One does not construct points, lines, planes, and other geometric objects from more primitive notions, but rather one simply stipulates, by stating axioms, some of their fundamental properties, leaving the nature of the objects completely undetermined. One then proceeds by proving theorems from these axioms and then note that the points of the geometry can be associated to points of real numbers and that the axioms of the theory are true when geometric points and planes are replaced by objects constructed from real numbers[22]. Hilbert's success apparently inspired other mathematicians. Chevalley mentions Lebesgue's integral, which is given by a list of properties and the concept of topological space, in which Weierstass limits are obtained from a purely abstract characterization, as Fréchet has shown. And then, there is algebra. Chevalley points out that one could even claim that the whole movement in fact originates from that source, more precisely from Dedekind's work and teaching of abstract groups. However, in algebra, Chevalley claims that the turning point can be found in Steinitz's work on field theory. Chevalley then claims that "the axiomatization of theories has profoundly changed the style of contemporary mathematical writings"((Chevalley, 1935, p. 381)).

Thus, the hallmark of the new style is the axiomatic method. Chevalley already emphasizes the fact that the axioms are not chosen arbitrarily. Mathematicians start from given, known proofs. One then performs an analysis of these proofs and tries to identify the properties that are strictly necessary to obtain a given result. One looks for the minimal logical requirements and tries to identify the domain of mathematics in which the result can be proved. Once this is done, it is possible to eliminate unnecessary hypotheses. In this way, according to Chevalley, one obtains elegant demonstrations. Chevalley, in 1935, identifies the autonomous domains of mathematics: in algebra, field theory, the theory of abstract groups, ring theory, hypercomplex numbers (now known as algebras); in analysis, measure and integration theory, topology, Riemann surfaces, Hilbert spaces; in geometry, projective and conformal geometries, Riemann spaces, combinatorial topology (renamed algebraic topology soon afterwards). In each case,

[21] Whether this is historically adequate, we will simply ignore. It is a debate among Hilbert scholars that need not concern us here.

[22] In the paper, Chevalley does not talk about interpretations, but by replacing one by another.

we get a specific type of *abstract structure*. Chevalley then claims that these theories combine, yielding, for instance, topological groups, which is seen as a new *abstract structure*. In other cases, some theories turn out to be based on the same axioms, or, in the words of Chevalley, their axioms yield the same structure, as is the case of probability theory and measure theory.

Traditional mathematical objects emerge from the combinations and interactions of some of these abstract structures. Chevalley mentions the system of real numbers: it is a field, a topological space, a topological group, an ordered set, a measured space, etc. The properties of real numbers are either theorems of one of these abstract structures that apply to them, or "properties resulting from the simultaneous validity of many of these theories"((Chevalley, 1935, p. 383)[our translation]). It is worth quoting the closing paragraph of Chevalley's paper:

> It results from all this that contemporary mathematics tries to define mathematical objects in comprehension, that is by their characteristic properties, and not by extension, that is by construction. This aspect is undoubtedly not definitive. But it is hard to predict at this point in which direction it will evolve. Be that as it may, the actual tendency seems far from having exhausted its internal dynamism. The diverse theories that have been separated up until now probably have not attained their definitive form. Many of them will probably be analyzed in terms of superpositions of even more general theories; others will turn out to be equivalent with one another or deriving from a common source. The structural analysis of facts already known is far from being done, not mentioning the analysis of these new facts that manifest themselves once in a while. ((Chevalley, 1935, p. 384))[our translation]

Chevalley is thus well aware that these autonomous domains, as he calls them, might change as mathematics evolves.

The last sentence of the paragraph is, for us, revealing: one has to effectuate a *structural analysis* of facts. Both words are important: one looks for a *structure* and it is obtained via an *analysis*. Both words are philosophically loaded and have a long history. Chevalley was certainly aware of that. Be that as it may, the expression captures perfectly the basis of the new style. The structural analysis leads to the identification of one or more structures and the latter are then explicitly captured by the axiomatic method[23].

In his paper, Chevalley provides a sketch of the 'new point of view' underlying Bourbaki's structuralist standpoint, Bourbaki's style[24]. It is striking to

[23]But it might also be somewhat too short. In his paper *Mathématiques et réalité* published in 1936, Albert Lautman characterizes the work of the Hilbert school as providing 'the synthesis of necessary conditions and not that of the analysis of first notions.'(Lautman, 2006, 49). Lautman is emphasizing the synthetic component inherent to the process of abstraction as embodied in the axiomatic method. He also explicitly refers to Carnap's work and the role of analysis in the latter.

[24]Patras, in his book Patras (2001), takes a similar position with respect to the idea that Bourbaki adopts a certain mathematical style.

see that the paper *The Architecture of Mathematics* and other papers written in the 1940s by various members of the collective essentially repeat and expand on what Chevalley had already said in 1935. They appear to be nothing more nor less than a more precise and updated version of the same ideas.

3.4.2 Bourbaki's epistemic mathematical style

Contemporary mathematicians did not hesitate to talk about Bourbaki's style in such a way that it pointed towards something more than simply the writing style. But no one clearly provided a characterization of the style. Halmos made a direct comparison with music:

> The Bourbaki style and spirit, the qualities that attract friends and repel enemies, are harder to describe. Like the qualities of music, they must be felt rather than understood. Halmos (1957)

There is no doubt that the style of presentation must be felt, but we will nonetheless propose a characterization of Bourbaki's epistemic style by applying our general framework.

First, we have to identify γ, the background culture, more specifically the standards against which Bourbaki's style has to be compared and contrasted. Since their original goal was to write a textbook on analysis, the background is given by the French textbooks of the time, those that they were using themselves[25]. One of the texts used at the time was Émile Goursat's *Cours d'analyse mathématique*[26]. Even a cursory look at Goursat's books indicates that it is an instance of what Chevalley called the ϵ-style, with many epsilons. Needless to say, Goursat does not use the language of sets systematically, the definitions are informal, in the sense that there is no explicit logical apparatus and there are no abstract structures involved either.

Let us move to δ, the patterns of definition. First, Bourbaki decides to use systematically, explicitly and in all cases the language of set theory as expressed in first-order logic. As we have already mentioned, no less than three different papers were written, namely by Jean Dieudonné, Henri Cartan and André Weil, in the late 1930s and 1940s, the latter presented by André Weil at the meeting of the Association of Symbolic Logic, to emphasize the need to provide explicit logical foundations for the working mathematicians. They are clearly not interested in the logical foundations of mathematics for its own sake, nor do they see in the latter as having any real impact on the work of mathematicians[27].

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[25] Needless to say, it would be relevant to do more detailed historical research and look carefully at the textbooks that were in circulation in France in the 1920s and early 1930s. We know that the original members of Bourbaki knew about and were influenced by books published outside of France, e.g. van der Waerden's *Moderne Algebra*, Seifert & Threlfall's *Lehrbuch der Topologie*, Alexandroff & Hopf's *Topologie*, Lefschetz's *Topology*, among others. We rely here on Beaulieu (1990); Corry (2004); Houzel (2004).

[26] Goursat's books can be consulted online at `https://archive.org/details/coursdanalysema00gourgoog/mode/2up`.

[27] Again, Chevalley is the only one who seemed to have taken a genuine interest in foundational studies at the time. He even wrote a report on Gödel's work on the con-

In fact, this choice has to be put in the perspective of the cultural background γ. For no one before Bourbaki had explicitly decided to present concretely in a unified manner, in one language, all the concepts required to do analysis, and, as they thought could be done in the 1930s and 1940s, the whole of mathematics. We will not belabor the idiosyncratic system of axioms of set theory chosen by Bourbaki, for what matters to us is merely the fact that they adopted the language of sets and the formalism of first-order logic in their presentation and practice[28].

It is in this language that the axiomatic method is used to define abstract structures. But we have to be clear as to what is meant here; it is not merely that a mathematician postulates what she likes and derives theorems from there. The axioms that come at the beginning of a presentation are in fact the result of a "structural analysis", to use Chevalley's words, and they are put together, thus synthesized, into a new, autonomous whole. The same idea appears later. In the 1940s, under the name of Bourbaki, Dieudonné wrote:

> Today, we believe, however, that the internal evolution of mathematical science has, in spite of appearance, brought about a closer unity among its different parts, so as to create something like a central nucleus that is more coherent than it has ever been. *The essential aspect of this evolution has been the systematic study of the relations existing between different mathematical theories, and which led to what is generally known as the "axiomatic method"*. (Bourbaki, 1950, 222)[my emphasis]

The function of the axiomatic method is to *abstract* new, original concepts from classical settings, and then to use this to reconstruct and extend these classical results in new directions. The idea is expressed later by Cartan:

> From the beginning, Bourbaki was a decided supporter of the so-called axiomatic method. [...] How does it [the axiomatic method] apply to higher mathematics? A mathematician setting out to construct a proof has in mind well defined mathematical objects which he is investigating at the moment. When he thinks he has found the proof, and begins to test carefully all his conclusions, he realizes that only a very few of the special properties of the objects under consideration have played a role in the proof at all. He thus discovers that he can use the same proof for other objects which have only those properties he had employed previously. Here we can see the simple idea underlying the axiomatic method: instead of declaring which objects are to be investigated, one only has to list those properties

sistency of the continuum hypothesis and I suspect that Gödel's work did influence him in his thinking as to how to give a general metamathematical account of the notion of structure. But this specific point will have to be argued elsewhere. For his report, see `http://sites.mathdoc.fr/archives-bourbaki/PDF/065_iecnr_074.pdf`.

[28]For critical evaluations of the axiomatic system adopted by Bourbaki, see Mathias (1992); Anacona et al. (2014).

> of the objects to be used in the investigation. These properties are then brought to the fore expressed by axioms; whereupon it ceases to be important to explain what the objects *are*, that are to be studied. [...] It is quite remarkable how the systematic application of such a simple idea has shaken mathematics so completely. (Cartan, 1979, 176-177)

This passage emphasizes the standards γ of the time again: when Bourbaki started to work on their project, this so-called axiomatic method was not systematically used in this way. There were important examples of its use in diverse areas, but it was not conceived as a way to reconstruct the whole of mathematics, as a way to introduce mathematical structures in general.

We are clearly dealing with a special type of axiomatic method which is now part of a new set of patterns of definition. The axioms are merely a contingent vehicle to talk about the concept of an *abstract* mathematical *structure*. The first step of the axiomatic method is to excavate the essential working components in diverse mathematical situations and extract or abstract the properties, operations, relations, etc. that are then expressed in the axioms. The latter provide a structure, an object of study in itself. Structures are related to one another in ways that classical mathematical fields were not, that is, by the properties, operations, relations that are abstracted out. It thus leads to a complete reorganization of mathematics and a completely different understanding of mathematical concepts.

> Bourbaki's decision to use the axiomatic method throughout brought with it the necessity of a new arrangement of mathematics' various branches. It proved impossible to retain the classical division into analysis, differential calculus, geometry, algebra, number theory, etc. Its place was taken by the concept of *structure*, which allowed definition of the concept of isomorphism and with it the classification of the fundamental disciplines within mathematics. (Cartan, 1979, 177)

This last sentence by Cartan captures an essential part of Bourbaki's epistemic style: "the concept of structure... allowed definition of the concept of isomorphism and with it the classification of the fundamental disciplines within mathematics." Thus, Bourbaki's patterns of definition of structures include intrinsically the notion of isomorphism. The latter is built in, it is part of the axioms, thus the definitions and, it will be part of the inference patterns, as we will see. Alas, Bourbaki's formal characterization of the notion of mathematical structure is often seen as a failure. We strongly believe that to discard it completely is a mistake; there is no need to throw the baby out with the bathwater.

3.4.3 Bourbaki's definition of abstract mathematical structures and isomorphisms

The importance of including the notion of isomorphism in the very definition of structures was understood early by Bourbaki. Here is how Chevalley expressed it in an unpublished version of the introductory chapter on sets:

> There are finally cases where the content of thought refers almost uniquely to the formal aspect of the notion considered. This is how, when a mathematician thinks of the content of the idea that he has of isomorphic mathematical beings, he will note, we believe, that he thinks less of the complete similarity of two objects as things than the following: any theorem concerning one of these objects can be translated into a theorem concerning the other. (Chevalley, Chevalley, 26) [my translation]

Chevalley expresses a very important shift in this quote, a shift that will be included in the final version of Bourbaki's technical definition of species of structure. We move from the idea of isomorphic mathematical beings in terms of similar objects to the claim that they are objects that satisfy the same theorems of a theory, or, from a proof-theoretical point of view, that the same theorems can be proved about these isomorphic beings. Thus, the idea is to define structures with the notion of isomorphism built in, so that if a specific theorem about one of these structures is proved, then any structure isomorphic to it will satisfy the very same theorem. Moreover, the only theorems such a theory ought to be able to prove are precisely those that are invariant under isomorphism. Thus, the pattern of definition includes a pattern of inference. This is the key component of the structuralist style.

Bourbaki's published technical definition of a "species of structure" is indisputably clumsy and was recognized as such. Moreover, and as we will briefly indicate later, when the final version was finally accepted by Bourbaki, they were very well aware that their definition could not accommodate categories and functors, and after many different attempts by different members, even Eilenberg, one of the creators of category theory, they simply gave up and published their latest attempt, which could only cover set-based structures.

I will not sketch Bourbaki's technical definition. I will rather offer a reconstruction of Bourbaki's notion of species of structure[29]. There are two reasons for presenting the reconstruction rather than Bourbaki's published version. First, we will use a more standard and transparent presentation. Second, it will be clear that Bourbaki's definition, which is really a different way of introducing the same ideas, is fully metamathematical. Indeed, in their final published version, when the reader finally gets to the definition of a species of structure, he or she reads "*A species of structures* in $\mathfrak{T}$ is a text Σ formed of..."(Bourbaki, 2004, 262) Look at it again: a species of structures is a *text*. How should one interpret this sentence? Is Bourbaki adopting a formalist stance? Notice that it is consistent with Chevalley's position with respect to mathematical style: it is a way of writing. It is nonetheless clear that Bourbaki's formal set-up has a natural interpretation in a universe of sets. It is a text with a canonical interpretation. More specifically, a species of structures has to be given by a formulas in a language, and when interpreted, it is a set together with relations,

[29]We thank Michael Makkai for this reconstruction.

etc. But what this clearly indicates is that Bourbaki is firmly, when he writes this, in *metamathematics* and not in mathematics[30]. This is methodologically very important, for it translates concretely the idea contained in Chevalley's foregoing quote. It is only in a metamathematical framework that one can state in full generality the requirement that isomorphic structures satisfy the same theorems. Moreover, one needs a fully general notion of isomorphism, something that did not exist when Bourbaki started to define species of structures, and this point has to be taken as an additional deviation from γ. Now, to the reconstruction.

We work in first order logic. Let $\vec{X} = X_1, ..., X_n$, a finite list of *basic set variables*[31] and $\vec{B} = B_1, ..., B_m$ another sequence of *parameters*. The latter are necessary to cover cases like vector spaces over a field k, modules over a ring R, etc.

Definition 1. An echelon construction on the set variables $X_1, ..., X_n$ and parameters $B_1, ..., B_m$, is a collection S of terms defined inductively as follows:

1. Each of $X_1, ..., X_n, B_1, ..., B_m$ is in S;
2. If A_1 and A_2 are in S, so is $A_1 \times A_2$;
3. If A is in S, so is $\mathcal{P}(A)$[32].

This is a standard inductive definition which gives us terms, that is denoting expressions, constructed in a systematic fashion.

Thus, an echelon construction S gives us the basic terms that are given or have to be constructed for the structure of a given kind to be defined. Let us denote an element of an echelon construction S by s_i and we will call such an element a *sort*. We can now introduce the notions of a similarity type, which was not in Bourbaki but is standard in logic.

Let S be an echelon construction and $\overrightarrow{S} = s_1, ..., s_p$ a sequence of chosen elements of S. These are now called *specified sorts*.

Definition 2. A signature $\mathcal{L} = \mathcal{L}(\overrightarrow{X}, \overrightarrow{B}, \overrightarrow{S}, \overrightarrow{R})$ (or similarity type) is given by:

1. A list $\overrightarrow{X} = X_1, ..., X_n$ of (basic set-)variables;
2. A list $\overrightarrow{B} = B_1, ..., B_m$ of parameters;
3. A list of specified sorts $\overrightarrow{S} = s_1, ..., s_p$, each $s_i \in S$;

[30] Granted, there is a clear shift in the section on structures. Bourbaki undisputably starts in a metamathematical framework, but as the section develops and tries to incorporate concepts that clearly belong to category theory, it morphs into a mathematical mode. It is a case of conceptual schizophrenia.

[31] We follow Bourbaki for the time being and talk about sets. They really are simply formal variables that will stand for sets. As variables, they are distinct.

[32] Some readers might be struck by the fact that we seem to be moving towards a type theory. Indeed, in many early versions of the theory of species of structures, Bourbaki does work with types. He progressively abandons the type theoretical terminology in favor of a purely set theoretical.

4. A list of relation symbols $\overrightarrow{R} = R_1, ..., R_p$, each R_j specified as a (sorted) relational symbol $R_j \subset s_j$, more precisely the arity of R_j is $R_j \subset s_{i_{j,1}} \times s_{i_{j,2}} \times \cdots \times s_{i_{j,k_j}}$[33].

This is all purely formal. We are just setting up the syntactic framework that allows us to talk about structures. In fact, we are now in a position to specify what a structure for a given signature $\mathcal{L}(\overrightarrow{X}, \overrightarrow{B}, \overrightarrow{S}, \overrightarrow{R})$ is.

Definition 3. An $\mathcal{L}$-structure M is given by the following data:

1. A tuple $\overrightarrow{X}^M = X_1^M, ..., X_n^M$ of (not necessarily distinct) sets, the basic sets;
2. A tuple $\overrightarrow{B}^M = B_1^M, ..., B_m^M$ of sets, the parameter sets;
3. A tuple $\overrightarrow{S}^M = s_1^M, ..., s_p^M$ of derived sets; each of these is understood as the set-interpretation of the corresponding echelon term; with the given sets $X_1^M, ..., X_n^M, B_1^M, ..., B_m^M$ plugged in for the variables $X_1, ..., X_n, B_1, ..., B_m$ respectively;
4. Actual relations $R_1^M, ..., R_p^M$ with R_j^M a relation of the type

$$R_j^M \subset s^M_{i_{j,1}} \times s^M_{i_{j,2}} \times \cdots \times s^M_{i_{j,k_j}};$$

$$R_j^M \subset s_j^M.$$

Now, the parameter sets, although arbitrary are fixed for a given structure. We will make that explicit in the notation.

Let us fix $\overrightarrow{\underline{B}} = \underline{B}_1, ..., \underline{B}_m$, the parameter sets. Notice the change in the notation here: we denote an actual, fixed set by an underline $\underline{B}$. We define an $\mathcal{L}_{\overrightarrow{\underline{B}}}$*-structure* to be an $\mathcal{L}(\overrightarrow{X}, \overrightarrow{B}, \overrightarrow{S}, \overrightarrow{R})$-structure M where $B_i^M = \underline{B}_i$ for $1 \leq i \leq n$.

So far, we haven't done anything extraordinary or difficult. We have given a simple type of $\mathcal{L}$-signature and $\mathcal{L}$-structure. The only original element comes from the echelon construction underlying both definitions. We hasten to add that this notion of $\mathcal{L}$-structure is *not* (yet) the notion we are driving at. We still have to impose a restriction on the latter to get to the notion of a *Bourbaki species of structure*. But for that, we need to define isomorphism and isomorphism transfer for $\mathcal{L}_{\overrightarrow{\underline{B}}}$-structures.

Isomorphism and transport of structure We start with two n-tuples of basic sets $\overrightarrow{X}^1 = X_1^1, ..., X_n^1$ and $\overrightarrow{X}^2 = X_1^2, ..., X_n^2$. We assume we are given an echelon construction S and an element s of S. We now fix the following notation. The *interpretations* of $s_{\overrightarrow{X}^1}$ and $s_{\overrightarrow{X}^2}$ of s is given inductively as follows:

1. If s is X_i, then $s_{\overrightarrow{X}^1}$ is X_i^1 and $s_{\overrightarrow{X}^2}$ is X_i^2;

[33] Needless to say, functions can be introduced as special kind of relations, as usual.

2. If $s = B_i$, then $s_{\overrightarrow{X}^j} = \underline{B}_i$ for both $j = 1, 2$;
3. If $s = s_1 \times s_2$, then $s_{\overrightarrow{X}^j} = (s_1)_{\overrightarrow{X}^j} \times (s_2)_{\overrightarrow{X}^j}$ for $j = 1, 2$;
4. If $s = \mathcal{P}(s')$, then $s_{\overrightarrow{X}^j} = \mathcal{P}((s')_{\overrightarrow{X}^j})$ for $j = 1, 2$.

The foregoing is straightforward bookkeeping and is merely an exercice in notation and substitution.

Assume that we are given a tuple $\overrightarrow{\phi} = \langle \phi_1, ..., \phi_n \rangle$ of bijections $\phi_i : X_i^1 \to X_i^2$, for $i = 1, ..., n$. The parameters B_j's are not part of the bijection tuple.

The bijection-tuple $\overrightarrow{\phi}$ induces bijections, for every s in S

$$\phi_s : s_{\overrightarrow{X}^1} \to s_{\overrightarrow{X}^2},$$

in the obvious way, where we use the identity maps $1_{\underline{B}_i} : \underline{B}_i \to \underline{B}_i$.

We can now explain how to transfer an $\mathcal{L}$-structure M to an $\mathcal{L}$-structure N.

Let M be an $\mathcal{L}_{\underline{\vec{B}}}$-structure with the basic sets $\overrightarrow{X}^1 = X_1^1, ..., X_n^1$ interpreted as $X_1^M, ..., X_n^M$, respectively and, similarly, let N be an $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structure with the basic sets $\overrightarrow{X}^2 = X_1^2, ..., X_n^2$ interpreted as $X_1^N, ..., X_n^N$, respectively. We use the bijections $\phi_i : X_i^M \to X_i^N$ to transfer the $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structure M to the $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structure N as follows[34].

Definition 4. 1. For each of the sorts $s_1, ..., s_p$,

$$s_j^N = (s_j)_{\overrightarrow{X}^2};$$

2. For each of the relation symbols $R_1, ..., R_p$, $R_j \subset s_j$ with arity $R_j \subset s_{i_{j,1}} \times s_{i_{j,2}} \times \cdots \times s_{i_{j,k_j}}$, we have the interpretation

$$R_j^M \subset s_{i_{j,1}}^M \times s_{i_{j,2}}^M \times \cdots \times s_{i_{j,k_j}}^M,$$

$$R_j^M \subset s_j^M$$

together with the bijective mappings

$$\phi_{s_{i_{j,1}}} \times \cdots \times \phi_{s_{i_{j,k_j}}} : s_{i_{j,1}}^{\overrightarrow{X}^1} \times \cdots \times s_{i_{j,k_j}}^{\overrightarrow{X}^1} \to s_{i_{j,1}}^{\overrightarrow{X}^2} \times \cdots \times s_{i_{j,k_j}}^{\overrightarrow{X}^2}$$

$$\phi_{s_j} : (s_j)_{\overrightarrow{X}^1} \to (s_i)_{\overrightarrow{X}^2}.$$

We define R_j^N as the image of $R_j^M \subset s_{i_{j,1}}^M \times s_{i_{j,2}}^M \times \cdots \times s_{i_{j,k_j}}^M$, $R_j^M \subset s_j^M$ under the foregoing bijective mapping. Thus, R_j^N necessarily satisfies

$$R_j^N \subset s_j^N = (s_j)_{\overrightarrow{X}^2}; \tag{1}$$

$$R_j^N \subset (s_{i_{j,1}})^N \times \cdots \times (s_{i_{j,k_j}})^N, \text{that is} \tag{2}$$

$$R_j^N \subset s_{i_{j,1}}^{\overrightarrow{X}^2} \times \cdots \times s_{i_{j,k_j}}^{\overrightarrow{X}^2}. \tag{3}$$

[34] These are Bourbaki's 'transportable relations'.

This definition can be captured by the following diagram:

$$\begin{tikzcd}
(s_j)_{\overrightarrow{X}^1} \arrow[r, "\phi_{s_j}"] & (s_j)_{\overrightarrow{X}^2} \\
R_j^M \arrow[u, hook] \arrow[r, dashed, "\simeq"] & R_j^N \arrow[u, hook]
\end{tikzcd}$$

where the dotted arrow signifies that it is induced by the given data to make the diagram commute.

We thus obtain an *isomorphism* $\widehat{\overrightarrow{\phi}} : M \xrightarrow{\simeq} N$ that is completely determined by the given bijection-tuple $\overrightarrow{\phi}$ on the basic sets $\phi_i : X_i^M \xrightarrow{\simeq} X_i^N$, $i = 1, ..., n$, that *preserves* the relations $R_1, ..., R_p$.

Now let us be given a set-theoretic formula

$$\Phi(\overrightarrow{X}, \overrightarrow{R}, \overrightarrow{B})$$

with the same free variables as before and no more. We *assume* that the formula Φ implies, that is contains as conjuncts, the specifications that

$$R_j \subset s_j \tag{4}$$

$$R_j \subset s_{i_{j,1}} \times s_{i_{j,2}} \times \cdots \times s_{i_{j,k_j}}. \tag{5}$$

We have a standard formula $\mathrm{Iso}(\overrightarrow{\phi}; \overrightarrow{X}^1, \overrightarrow{R}^1; \overrightarrow{X}^2, \overrightarrow{R}^2; \overrightarrow{B})$ with the distinct free variables as shown that expresses that $\overrightarrow{\phi}$ *is* an isomorphism of the $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structures M and N

$$\vec{\phi} : M \xrightarrow{\simeq} N$$

where M is given by $\overrightarrow{X}^1$ and $\overrightarrow{R}^1$ and N by $\overrightarrow{X}^2$ and $\overrightarrow{R}^2$. We can now formulate *Bourbaki's condition of isomorphism invariance*:

In the adopted set-theory, it is *provable* that

$$\vdash \Phi(\overrightarrow{X}^1, \overrightarrow{R}^1, \overrightarrow{B}^1) \wedge \mathrm{Iso}(\overrightarrow{\phi}; \overrightarrow{X}^1, \overrightarrow{R}^1; \overrightarrow{X}^2, \overrightarrow{R}^2; \overrightarrow{B}) \Longrightarrow \Phi(\overrightarrow{X}^2, \overrightarrow{R}^2, \overrightarrow{B}^2).$$

This is, of course, the key component of the whole construction and will be part of the notion of species of structures.

Bourbaki's definition of species of structures is now at hand.

Definition 5. A *Bourbaki species of structures* is given by the $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structures whose relations satisfy the condition of isomorphism invariance.

The crucial element to notice is that the notion of isomorphism is *systematically built into* the *definition* of species of structures. It is defined for all $\mathcal{L}_{\underline{\overrightarrow{B}}}$-structures before the structure is required to satisfy any condition, any axiom. This is now a norm for all concepts defined with the axiomatic method: one has to make sure that the concept is invariant under the proper notion of isomorphism on the technical sense given above.

We now have one of the main components we were after. Bourbaki's meta-mathematical analysis of the notion of abstract structure automatically yields a crucial component of Bourbaki's mathematical style. Bourbaki is using the axiomatic method as a mode of definition, but he adds an essential ingredient to it, namely the condition of isomorphism invariance. This is not part of the standard axiomatic method. It was not intrinsic to Hilbert's axiomatic method, nor was it clear that it ought to be built into *all* of mathematics. The notion of abstract structure *comes with* the notion of invariance under isomorphism. Again, this is an important deviation from the standards γ.

This has a direct impact on the patterns of inference ι that are part of Bourbaki's style. Of course, as we have noted, the presentation style is of the form *Satz-Beweis* throughout. The logical structure of the proofs and the logical organisation of the volumes are all explicit. This is all well and good, and indeed is a part of ι. But there is more, and this additional element has to do with the specifically structuralist component of the style. Although we are in a set-based universe, the species of structures possess all and only the properties they have *as structures*. The patterns of reasonings are therefore constrained to these and only to these. One could therefore say that the reasonings are, in fact, *structure*-based. All the steps, *all* the reasonings have to be done up to isomorphism.

There is an additional aspect to the style that follows from the analysis-synthesis method, i.e. the abstract axiomatic method, and this is the use of a certain type of maximality principle. When one analyses a proof and determines the necessary and sufficient components to get the proof, one thus synthesizes the most abstract structure in which the proof is obtainable — relative to the given language and context. One is therefore naturally lead to axiomatize the most general abstract concept. This is an additional epistemic feature of Bourbaki's style, at least from 1935 until the late 1940s, and that has to be included in the δ. The patterns of definition have a direct impact on the patterns of inference and the interactions κ between δ and ι. Thus, Bourbaki introduces, in Bourbaki (1950), the so-called "mother-structures" and their combinations. The specific mother-structures — algebraic, topological, order —, although perhaps intriguing and thought provoking, are epistemically speaking, only secondary. It is the reasoning modes that matter here and it is these that explain the organization of mathematics that emerges from the structuralist standpoint.

The organization of the first four chapters of Bourbaki's *General Topology* illustrates Bourbaki's epistemic style. Chapter One deals with the structure of topological spaces. Filters and ultrafilters are used to deal with the notion of convergence. These two latter notions are purely structural and are not defined with respect to certain numbers and their properties. In the second chapter, the notion of uniform structure is defined and basically replaces the notion of metric space. Chapter Three moves to topological groups, the generic example of a genuinely new structure emerging from the interaction of two abstract structures, and the notion of uniform structure plays an important role in the presentation. We then move to topological rings and their completions. Once these structures and their properties have been studied, Bourbaki finally introduces the real numbers as a topological group which is the completion of the additive group of rationals. They then extend the field structure of the rationals to the reals. Thus, the real line is a combination of a topological, an algebraic and an order structure.

We are now in position to say more specifically how Weil's way of doing mathematics diverged from Bourbaki's style. As we have seen in the foregoing section, Weil did not always start from the most general abstract structure and move down to the more specific context he was interested in. Indeed, instead of adopting a maximality principle with respect to abstract structures, one could claim that Weil adopted a minimality principle instead. Indeed, as Zariski had noticed in his review, Weil restricted himself to the 'simplest facts about abstract fields and their extensions and the bare rudiments of the theory of ideals'. In contrast, Bourbaki uses modern algebra and abstract structures in general 'to the fullest possible extent'. Furthermore, in his approach to the foundations of algebraic geometry, Weil did not take into account the idea of working with structures that are invariant under isomorphism. In fact, Weil was recalcitrant towards the idea of automatically attaching a type of morphism to a species of structures. Indeed, in his (Corry, 1996, 380), Corry quotes a letter from Weil to Chevalley:

> 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?

Weil, interestedly, was also opposed to categories in general and, perhaps, just for this reason.

Categories and species of structures Of course, Bourbaki species of structures are based on sets even though a species of structures does not automatically come with a set-theoretic notion of morphism. Indeed, Bourbaki explicitly rejects this possibility in the final version of the chapter on structures : "A given species of structures therefore *does not imply* a well-defined notion of morphisms."(Bourbaki, 2004, 272). Bourbaki did not find a way to incorporate categories in their definitions and, with hindsight, many members came to the conclusion that Bourbaki's analysis came short[35].

Of course, one of the main problems was that some categories cannot be sets. And if one allows for the existence of classes, then problematically there are some operations on categories, e.g. functor categories and functors between those, that are not legitimate. But there is more, and it is important to understand this point. When Bourbaki was thinking about these problems, category

[35]For instance, see Dieudonné (1970); Cartier (1998) for their evaluation of the situation. See Corry (1996, 2004); Krömer (2006, 2007) for a more general analysis. Unfortunately, we still don't have access to all the documents of that period which would allow us to better understand how and why Bourbaki failed to include categories in their enterprise.

theory had not attained its full maturity. In particular, the proper notion of isomorphism for categories had still not been identified properly. Indeed, it appeared in press for the first time in Grothendieck's Grothendieck (1957), and even then it was not properly defined. Thus, to cover categories properly, and in particular, to cover categories in a structuralist fashion, in Bourbaki's style, required a change in the metamathematical analysis and the metamathematical framework. The fact is, categories are more abstract than set-based structures and in that framework, category-based structures have to be defined up to equivalence, not up to isomorphism. Two frameworks deal explicitly with these levels of abstraction and so can be readily employed for reconstructing the structuralist style, namely Makkai's FOLDS, as in Makkai (1998), and Homotopy type theory with the univalent axiom, as in Collective (2013).

Thus, we claim that even for categories Bourbaki's structuralist style is entirely clear and legitimate. The main components of Bourbaki's style are a direct consequence of their metamathematical analysis of abstract mathematical structures and, in a sense, the style provides a set of *norms* that guide mathematicians both globally, with the overall organization of mathematics, and locally, with the patterns of definition and patterns of proof.

3.4.4 Doing mathematics up to isomorphism: Bourbaki's legacy

Nowadays, pure mathematics *is* done up to isomorphism. Bourbaki's style has become the norm, the standard. It is not questioned. It is a new norm. Students of pure mathematics are taught mathematics that way. We simply do not explicitly see it as their method[36]; we do not have to. The previous styles could have prevailed; likewise, the Bourbaki style could disappear[37].

Every field is based on a structure or a combination of structures. Theorems are proved by establishing properties of structures and relations between structures. One gets to classical results by combining and specifying various structures. The whole organization of mathematics is turned upside down. The whole ontology of mathematics is revised[38]. Numbers, geometric figures, etc., are now elements of structures, more or less abstract. The (conceptual) foundations of mathematics — in contrast with the logical foundations of mathematics — are now made up of monoids, groups, rings, modules, fields, vector spaces, topological spaces, measure spaces, partial orders, etc. By specifying properties

[36]The status of the alleged proof of the ABC conjecture by Mochizuki rests on a subtle discussion regarding isomorphisms and identities, the abstract and the concrete! See `http://www.kurims.kyoto-u.ac.jp/~motizuki/SS2018-08.pdf`, section 2.

[37]It is certainly evolving. Grothendieck and his school have contributed to this change. The structuralist style nowadays includes categories, functors and working up to equivalence of categories. The introduction of higher dimensional categories makes the style even more abstract than it was.

[38]We use the term 'ontology' in its traditional philosophical sense. We could also use it in its modern, engineering sense of classificatory principle. It is quite interesting to see the evolution of the organization of the field and compare, say how mathematical disciplines were organized around 1920 and in the 1960s. In the sense of a classification of disciplines, the mathematical ontology has radically changed with Bourbaki's work.

of those, one gets more structures, and their combinations give rise to genuinely new structures.

When one does mathematics *à la* Bourbaki: one identifies the appropriate *abstract* structures involved in a given context; one looks at the theorems about these structures that are relevant to the given problem; one applies these theorems appropriately and solves the problems by using all and only the abstract properties needed. It is highly abstract. It is elegant. It is clean. It is rigorous. But it is awfully hard, for one needs to learn all about these abstract structures and know how and when to use them. Sometimes, it seems unnecessary, uselessly abstract. Does one *need* to use a theorem about locally compact abelian groups to prove Plancherel's theorem about Fourier transforms of certain functions on the real line? Of course not. Plancherel certainly did not prove his theorem by using the structure of locally compact abelian groups. Proceeding that way is sometimes seen as a form of intellectual terrorism or a form of elitism. To some, it is repelling. But it *can* be done this way and there are cognitive benefits to doing so.

4 The Structuralist Style

Bourbaki's style is an instance of what might be dubbed the 'structuralist style.' Using our definition of mathematical style, we submit that the structuralist style is based on these interrelated components.

1. Patterns of definition for abstract structures. Bourbaki naturally used the axiomatic method. He was well aware that the term 'axiom' does not refer to its usual epistemological sense. One merely needs a systematic procedure to list properties, relations and how they are connected with one another. Sketches could be used and in the context of higher dimensional categories, might very well be used. We assume that the structures are abstract simply because they have been abstracted from previously given mathematical contexts[39].

2. These patterns of definition have to include criteria of identity for the abstract structures. Bourbaki helped clarify the general notion of isomorphism for species of structures. At the time, it was the natural criterion of identity to define and use. Nowadays, we know that we need homotopy equivalence, categorical equivalence and higher dimension equivalences. Mathematics is done up to a certain type of isomorphism, the latter being derived from the abstract structures one is working with.

3. An appropriate logical framework is needed to codify the inference patterns inherent in these abstract structures. In a sense, first-order logic was designed specifically to tackle set-based abstract structures. First-order logic, set theory and Bourbaki's structuralism co-evolved from the 1910s

[39]Thus, in this sense, being abstract is a relative property and is not opposed absolutely to being concrete.

until the 1950s. It allowed Bourbaki not only to specify what a structure was, but more importantly what is meant to do mathematics 'up to isomorphism'. Bourbaki required that the properties and relations used to define structures be 'transportable', which is to say that they are invariant under isomorphism. More precisely, Bourbaki required that any property P (and relation) present in the axioms of a species of structure $\mathcal{S}$, satisfy the following structuralist principle: for all X of type $\mathcal{S}$, if $P(X)$ and $X \simeq Y$, then $P(Y)$, where the relation $X \simeq Y$ is the appropriate notion of isomorphism for this species of structure. Nowadays, depending on one's needs and goal, one could use Makkai's FOLDS or homotopy type theory. The main point is that these logical frameworks also satisfy the structuralist principle. These two might also be the first steps towards a different system that still has yet to be defined, but that would be designed as to satisfy the structuralist principle.

4. A systematic framework to combine and compare these abstract structures. Again, the axiomatic method *together with* the notion of isomorphism played that part in Bourbaki's case. It quickly turned out to be inadequate, for the language of categories and functors was more effective and systematic, although more abstract.

Many philosophers of mathematics have claimed that Bourbaki's structuralism had nothing to do with what the philosophers call 'mathematical structuralism.' We have, in a companion paper Marquis (2020), argued that those philosophers have misunderstood Bourbaki's structuralism. Bourbaki is unfortunately responsible in part for this state of affairs. We will not rehearse our arguments here. One of the reasons given is that Bourbaki's technical notion of (species of) structure was basically flawed and so was mathematically useless. We disagree with this evaluation, our current claim is clear: Bourbaki exemplifies a mathematical style. And anyone interested in the epistemology of mathematical practice should pay attention to its implications for how we reason in mathematics.

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