Weyl, H. 1944. "David Hilbert and His Mathematical Work." *Bulletin AMS* 50:612–54.
Zeilberger, D. 1994. "Theorems for a Price: Tomorrow's Semi-Rigorous Mathematical Culture." *Notices of the AMS* 40:978–81 (Reprinted *Math Int.* 16(4):11–14).

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The Withering Immortality of Nicolas Bourbaki: A Cultural Connector at the Confluence of Mathematics, Structuralism, and the Oulipo in France

The Argument

The group of mathematicians known as Bourbaki persuasively proclaimed the isolation of its field of research — pure mathematics — from society and science. It may therefore seem paradoxical that links with larger French cultural movements, especially structuralism and potential literature, are easy to establish. Rather than arguing that the latter were a consequence of the former, which they were not, I show that all of these cultural movements, including the Bourbakist endeavor, emerged together, each strengthening the public appeal of the others through constant, albeit often superficial, interaction. This codependency is partly responsible for their success and moreover accounts for their simultaneous fall from favor, which, however, can clearly be seen as also stemming from different internal problems. To understand this dynamics, I argue that Bourbaki's role can best be captured by using the notion of *cultural connector*, which I introduce here.

Il a nécessairement vieilli, votre fictif
mathématicien, il doit avoir pris du retard.
Eh bien! non, Bourbaki n'a pas vieilli
parce qu'il ne peut pas *vieillir.*

Raymond Queneau (1962)

Truly, as Queneau claimed, the prolific mathematician Nicolas Bourbaki could not grow old for the good reason that he never existed. That is, the man never did. As a symbol, on the other hand, he was, for more than 30 years, powerful enough to serve many different purposes across disciplines.[1] By looking at the various

[1] Throughout, I use customary male pronouns to refer to Bourbaki. In doing so, I am aware of the danger of reinforcing a myth — the myth of a collective author speaking with a single, authoritative

roles he played in several types of discourse, his rising impact among some mathematicians, structuralists, and writers alike, and then his declining authority, we can study the way in which different cultural streams mingled at a node called "Bourbaki." We will be able to put some flesh on the cliché that sometimes, somehow, ideas are "in the air."

Nothing more than a nom de plume chosen by a group of French mathematicians, Bourbaki nonetheless — or rather for precisely this reason — authored, from 1939 on, one of the most ambitious mathematical treatises of the twentieth century: *Les Eléments de mathématique*.[2] Raymond Queneau wrote that Bourbaki would always keep abreast of his time — he would never grow old — since a persistent rumor stated that at age fifty a "collaborator" of Bourbaki (as it was customary to call members of the group) would relinquish his veto over any of Bourbaki's publications, a right that would forever remain the prerogative of younger generations. In this sense, Bourbaki indeed enjoyed the rare privilege of eternal youth.[3]

But while the mythic Bourbaki could not grow old, the myth of Bourbaki has. To reconstitute the "thought" of a group of people is probably a futile affair. This hardly means, however, that it is impossible to recover what Bourbaki stood for, within the mathematical community and outside of it. For almost everyone, he served as a symbol of a strict method of axiomatization, with which, he himself claimed, he could build up the whole of mathematics on the sole basis of a few fundamental "mother structures" and their combinations. He thereby wished to purify mathematics of any reliance on the external world. This made the usefulness of mathematics in understanding natural phenomena hard to comprehend; it followed from a mathematical order inherent in nature, rather than from the fact that, historically, mathematics had often been constructed with specific purposes in mind.

Sometime in the course of the 1970s, however, this vision, which had become dominant among mathematicians and which Bourbaki stood for, unraveled. Let me emphasize that it was Bourbaki's *vision* that then faded away, not his goal of founding mathematics on the notion of structure, which, as we shall see, had already begun to face serious challenges as early as the 1950s (Corry 1996). Domains of mathematics bloomed and boomed without the aid of the axiomatic method — e.g., the theories of catastrophes, chaos, and fractals. Although the elaboration of a satisfactory set of axioms for these mathematical theories proved challenging, if not impossible (e.g., Arnol'd [1981] 1992), mathematicians managed

voice. But since this article deals with the actual effects of this mythic image rather than the "true" history behind it, this odd usage seemed appropriate. For Bourbaki's myth, see for example Halmos 1957; for its functions with respect to Bourbaki's image, see below.

[2] Histories of Bourbaki are to be found in Beaulieu 1993, 1994; Cartan 1980; Chouchan 1995; Corry 1996; Fang 1970. However, the most comprehensive history, Beaulieu 1989, is still unpublished.

[3] The historian Liliane Beaulieu, who has worked the most extensively on Bourbaki, told me that she had never come across any written trace of this rule and that in any case it was breached many times.

to dispense with it and still produce significant results enthusiastically embraced by their community. In addition, these recent theories were developed not out of motives purely internal to mathematics but rather in constant interaction with other fields of science, such as biology, physics, economics, or even structural linguistics. All of this was obviously and distinctively anti-Bourbakian.

Surveying the mathematics of the 1970s, Christian Houzel, soon to be elected president of the Société mathématique de France, revealed to the public that "the age of Bourbaki and fundamental structures is over." While the previous period was one that had witnessed the development of powerful new theoretical tools of great generality, he noted, the 1970s were characterized rather by a tendency to revive an old interest in more concrete problems. Houzel did not venture an explanation for this reversal. "I cannot say," he simply wrote, "to what extent this [tendency] is conditioned by the internal dynamics of the development of mathematics, or by ideological currents like the degradation of science's superior image in public opinion and scientists' questioning of the social status of their practice" (Houzel 1979, 508–9).[4]

Houzel wisely avoided addressing a dilemma familiar to the cultural historian. The cultural history of science strives to understand the subtle connections between science and society. But in order to present a compelling argument, it is necessary to go beyond metaphors and analogies. However appealing some connections may appear, how can we assess whether enough evidence has been presented? Just how many astonishing coincidences will suffice for a story to be plausible? This often remains problematic. While some historians of science have recently been able to articulate such connections convincingly by focusing on social units naturally well circumscribed, the story of Bourbaki as a cultural icon in postwar France requires a much more diffuse framework, a Protean notion of cultural connection (see Biagioli 1993; Daston, 1988; Galison, 1994; Schaffer 1995; Smith and Wise 1989; Wise 1989–90).

Paralleling the trajectory of structuralism, Bourbaki's rise and decline in postwar France provides, I believe, a perfect case by which to exhibit the possibilities and limits of the cultural history of science. Here there are indeed clear indications that the mathematicians' attitudes coincided with broad social, cultural, and intellectual movements, which seems to suggest that a reconciliation between the culture of Bourbakian mathematics and larger currents might be possible. I mean to achieve this by looking at Bourbaki as a *cultural connector*.

Cultural connectors, as I define them here, are more or less explicit references allowing actors to connect different spheres of culture in order to strengthen the meaning of their work. Cultural connectors carry whole sets of meanings and practices which may flow more or less happily. They have two aspects that need special emphasis. First, cultural connectors act at a variety of levels. People

[4] All translations are mine, except when I quote from a published translation. See also Houzel 1985 and Arvonny 1980.

establish connections through personal contacts, either furtive or winding up in intense collaborations; by their citations, which are innocuous metaphors or essential concepts for thought or legitimacy; by borrowing, translating, adopting, and adapting whole bodies of knowledge for a new setting; and finally, by associating different cultural spheres in the context of a third discourse — a philosophical enterprise or the media. All these levels are important. Second, although the diversity of levels at which cultural connectors are employed is bound to make the connection seem superficial, cultural connectors acquire their strength through constant reinforcement, and they link durably the spheres of culture they connect. A single instance of connection is not enough; it must be picked on, expanded on, argued for and against, etc. The connection becomes so widespread that a historical account can usefully be given not only for the plugging in of the cultural connector but also for its rupture, obviously taken not as an event but as a process.

Power relations and institutional dynamics however are, up to a degree, a blind spot of the following historical analysis in terms of cultural connectors. Microsociological science studies have revealed over and over the crucial relevance of these relations, but also their extreme diversity, which makes them hard to include in a study drawing attention to a widespread, diffuse cultural connection that extends beyond closed, well-circumscribed sociological circles. In my view this is a price we may have to pay in order to achieve a more global understanding of the cultural dynamics of science in contemporary society.[5]

Bourbaki, therefore, acted as a cultural connector — not because the members of the group were especially active outside of mathematics itself, but because his very name had come to serve as a shortcut indicating a certain attitude toward science. By invoking Bourbaki, authors signaled that they espoused some of his views — or more precisely, as I will not always state explicitly, views commonly attributed to him. How this state of affairs came to be, how it varied and evolved, and within which limits, is the topic of this paper. In what follows, I tell the story of the origins of the connection, its period of hegemony, and its decline. In the last part I also mention cultural connectors replacing Bourbaki at the interface of mathematics and the philosophy of science. I emphasize the intersection of three arenas in which the name of Bourbaki often appeared: mathematics; the structuralist and postmodernist discourses; and so-called potential literature, always focusing on the points of contact between cultures.[6]

This story will perhaps strike some readers as being inwardly French. This impression is partly due to the editorial choices I made in order to restrict the scope of the paper. But also, and most importantly, it reflects the actors' intellectual

[5] David C. Brock and M. Norton Wise recently argued that postmodernism could be seen as a cultural connector between "postmodern quantum mechanics" and contemporary culture (Wise, in preparation).

[6] In addition, Bourbaki's name has often been invoked by reformers of mathematical education, and their adversaries, both in France and in the United States — an issue that I scarcely address here, but that is part of the story of the cultural connector Bourbaki.

attitude. External developments in science and philosophy were often integrated into the French intellectual scene only after some accommodation, and the yardstick by which they were measured always remained at home. It was one of Bourbaki's great achievements to adapt older mathematical approaches to the French context and to succeed in diffusing them to larger audiences. With this paper, I hope to provide a firm historical basis for placing the evolution of mathematics, and of its image, in a larger cultural context. By doing so, I will both review the culture of mathematicians in postwar France and put culture back into the cultural history of mathematics.

Part I: Origins

Structuralisms: Lévi-Strauss and Bourbaki

In the aftermath of the Liberation, in 1944, France experienced a period of bubbling intellectual activity. Most prominently, the existentialists held a philosophy of *engagement*, well adapted to their troubled times but disjointed from mainstream scientific pursuits, with the possible exception of psychoanalysis. In particular, it had no use whatsoever for mathematics. Although existentialism nearly monopolized intellectual debates in the immediate postwar era, foundations were meanwhile being laid down for the next generation. Among the important works that came out in 1948–49, besides Braudel's *La Méditerranée* and de Beauvoir's *The Second Sex*, were two more that nicely exemplified the new directions soon to be followed. Both had their genesis during the exceptional circumstances of World War II. One, Claude Lévi-Strauss's *Elementary Structures of Kinship* ([1949] 1969) is quite well known. This book is widely considered as the "act of foundation" of postwar French structuralism, an approach to the human sciences extremely influential in shaping cultural and intellectual discourses in France for the ensuing decades.[7]

The other work to which I want to draw attention is a special issue of *Les Cahiers du Sud* published in March 1948. Edited by the mathematician François Le Lionnais, it proposed to delineate the "Great Currents of Mathematical Thought" (Le Lionnais [1948, 1962] 1971). Dating from 1939, the idea for this collection was impeded by the problems of wartime communication, which intensified its French focus, and was further delayed by the internment in 1944 of Le Lionnais in a German camp. Although not as famous as Lévi-Strauss's work, it included a seminal programmatic statement by Bourbaki ([1948] 1971).[8] There he succinctly articulated, in general terms, his overall approach to a unified science of mathematics. During the 1950s and 1960s, Bourbaki's approach would be at least

[7] Quoted from the chronology established by Simonin and Clastres (1989, 79), which has been very useful for this paper.

[8] A different English translation of this article was published earlier — Bourbaki 1950.

as diligently followed by mathematicians as was structuralism by social scientists. More strikingly, the appeal of this famous article was based on the powerful metaphor of "mother structures." As we shall see later, Bourbaki's structures were not unrelated to those of Lévi-Strauss.

Both Bourbaki and Lévi-Strauss can therefore be viewed as having founded some sort of structuralism. But what I wish to discuss here is not so much the fact that these books can rightly be considered as sources for important currents of thought, but rather that both represented an intersection of people and ideas that would remain loosely associated until their common effacement in the 1970s. Indeed, Lévi-Strauss's book included an appendix written by André Weil (1949), one of Bourbaki's founders and foremost collaborators. On the other hand, Le Lionnais's book included, in addition to the articles written by Bourbaki, Weil, and Jean Dieudonné (another member of Bourbaki), a contribution by the famous writer Raymond Queneau ([1948] 1971), author of *Zazie dans le métro* (1959). In 1960, Queneau and Le Lionnais cofounded an influential literary group, the Oulipo (Workshop for Potential Literature), which explored the possibility of language in a way directly inspired by Bourbaki. Already in the immediate postwar period, then, the discourses I want to talk about seem to have been involved in some discussion. Let us see how these relationships came into being.

Bourbaki: The Emergence of a Myth

On 10 December 1934 six young French mathematicians gathered in a Parisian café. André Weil had convened them with the goal of writing, collectively, a textbook of analysis, "as modern as possible" (Beaulieu 1989, 147). They were: Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, René de Possel, and Weil; a few months later they formed Bourbaki.[9] The story of this meeting and of the following years, which saw the emergence of Bourbaki, has been related in detail by Liliane Beaulieu (1989, 1993, 1994).

When in 1948, fourteen years and a world war later, the piece called "The Architecture of Mathematics" appeared in Le Lionnais's *Great Currents of Mathematical Thought,* N. Bourbaki was getting to be known for two main reasons: his treatise and his myth. If the reader believed the leaflet spelling out "the directions for the use of this treatise" enclosed with each published booklet, it seemed, then, that Bourbaki had embarked on a gigantic project. On the basis of the notion of *structure,* he would construct the foundation for all mathematics with the help of the axiomatic method. The *Eléments de mathématique* claimed to take up "mathematics at the beginning, and [give] complete proofs" (Bourbaki

[9] Paul Dubreil, Jean Leray, and Szolem Mandelbrojt participated in some of the subsequent meetings. Leaving the group before the summer, Dubreil and Leray were never formal members of Bourbaki. Later, in 1935, they were replaced by Jean Coulomb and Charles Ehresmann (Beaulieu 1989, 12–13).

1939). The modesty of the word *Eléments* in the title was misleading, the parallel with Euclid's *Elements* revealing the extent of Bourbaki's ambition. *Mathématique* on the other hand was, unusually for the French, singular, for this was how he had come to see mathematics as a whole.

So far, eight booklets had been published. All were devoted to aspects of algebra and topology, except for the first one to appear — in 1940, albeit dated 1939 — which presented a digest on "naive" set theory (his word), a more formal treatment being announced. Together they formed the first chapters of the first books of "Part I: The Fundamental Structures of Analysis." These chapters dealt with the bases of analysis in a very general, abstract way. As Beaulieu has documented, this emphasis departed from traditional textbooks (Beaulieu 1989, 171–90). True, Bourbaki promised: "The general principles studied in Part I will find their applications in the following parts" (Bourbaki 1939). But of course nobody knew then what these parts would contain or when they would appear.[10]

There was thus a significant shift between what the treatise was and what it promised to be. This shift had its origin in remnants of the initial goal of Bourbaki's members, which was, as I have said, to write a textbook of analysis. At first the group consulted physicists and applied mathematicians, such as Jean Leray, Jean Coulomb, and Yves Rocard (Beaulieu 1989, 156–61). The future Bourbakis hoped that their book would be useful to students and users of mathematics as well as accomplished mathematicians. For Cartan this even meant, at the first meeting, that algebra should be eliminated from the treatise (ibid., 150). Later that day, however, Delsarte proposed that the treatise start with "an abstract, axiomatic exposition of some essential general notions." A consensus emerged for the idea of a short "abstract packet," provided that it was "reduce[d] to the minimum" (ibid., 151). Beaulieu's account shows that on the contrary this "packet" grew in the following years without an explicit decision to that effect. This was almost the only part of the treatise about which major decisions had been reached before the war dispersed the Bourbakis on both sides of the Atlantic. The later parts were not yet so well conceptualized. It was thus this abstract part that durably left its imprint on the whole project.

In the writing of the "abstract packet," between 1935 and 1938, Bourbaki developed his own style of presentation. If at times some collaborators had proposed texts appealing to intuition and starting with examples, Bourbaki slowly decided to work otherwise. It became customary to present definitions before examples and build general results first, relegating concrete applications to witty exercises. In his own words, Bourbaki constantly proceeded from "the general to the particular" (Bourbaki 1939). As Beaulieu writes, this "was not a sacred principle given a priori. Only after consultations and tryouts was Bourbaki's

[10] The (unpublished) global plan for the treatise, reproduced in Beaulieu 1989, vol. 2, 104, reveals that in 1941 Bourbaki was planning at least three other parts (functional analysis, differential topology, and algebraic analysis).

exposé progressively purified from examples" (ibid., 376). But Bourbaki knew that this mode of presentation would be striking to most readers:

> The choice of this method was imposed by the principal object of this first part, which is to lay the foundations for the rest of the treatise, and even for the whole of mathematics. For this it is indispensable to acquire, to start with, a rather large number of very general notions and principles. Moreover, the necessity of demonstration requires that chapters, books, and parts follow one another in a rigorously set order. The usefulness of some considerations will thus appear to the reader only if he already possesses a rather extended knowledge, and then only if he has the patience of suspending his judgment until he has had the occasion of convincing himself of this [usefulness]. (Bourbaki 1939)

This act of faith, demanded of the reader, was made easier by Bourbaki's myth. The pseudonym, as Beaulieu emphasized, certainly had its function for members of the group (Beaulieu 1989, 297–306).[11] It helped diffuse the tensions of collective writing among eminent mathematicians who, investing time and effort, saw their work severely criticized or rejected offhand, with no hope of immediate professional reward. Indeed, authorship for Bourbaki was a complicated affair. In a letter to Jean Perrin, Szolem Mandelbrojt, then under-secretary of scientific research, explained: "Each chapter, after having been . . . discussed at length, is assigned to one of us; the resulting work is seen by all and is again discussed in detail; it is always redone at least once, and sometimes many times. We thus pursue a truly collective *oeuvre,* which will present a deep character of unity" (Chouchan 1995, 10). In practice, until the 1960s, it often was Jean Dieudonné who wrote the final version (cf. Beaulieu forthcoming b).

From the point of view of his audience, Bourbaki's persona became a powerful guarantee for his authoritative pronouncements. If a group of prominent mathematicians had agreed that these were the basic structures of mathematics then it surely was so. Indeed, while the literature about Bourbaki often emphasized his "polycephalic" nature, it remained discreet about who took part in the writing of his treatise. It was not important to know who these mathematicians were, only that they had achieved a consensus. The myth had the effect of bolstering Bourbaki's scientific authority and hiding arguments among the group. Similarly, one should view the rumor of Bourbaki's collaborators' retirement at age fifty as catering to the widespread belief that one's best mathematical work was accomplished in one's youth.

[11] The pseudonym came from an old student prank of the Ecole normale. In 1923, the freshman class, including Cartan, attended a phony lecture concluding with "Bourbaki's theorem," Bourbaki being the name of a French general in 1870 (Beaulieu 1989, 278–82).

The Architecture of Mathematics

"This text deserves special study," Jacques Roubaud wrote of Bourbaki's "Architecture." "There, Bourbaki quietly handles properly Neanderthaloid philosophical bludgeons contrasting with his usual snaky cautiousness." At the same time Roubaud pointed out just how implicitly Bourbakist was the *Great Currents of Mathematical Thought* as a whole (Roubaud 1997, 114–32; quote is on p. 123). Even if philosophically naive (compared with the sophisticated defense of axiomatics of Chevalley and Dandieu 1932 and Chevalley 1935), and perhaps because of this, the article was widely read. Beaulieu has since discovered that Dieudonné was its principal author and that it apparently was not discussed by the group (Beaulieu 1989, 2: 77). For my purposes, since neither Bourbaki nor any of his collaborators ever retracted it, we can safely take this article to be as it was then perceived — that is, Bourbaki's articulation of his own program.

"Mathematical science is in my opinion an indivisible whole, an organism whose vitality is conditioned upon the connection of its parts," David Hilbert had claimed in his famous lecture before the International Congress of Mathematicians (Hilbert [1900] 1902; see also Reid 1986, 82). However, by 1948, Bourbaki noted, mathematicians were producing thousands of pages of new results every year, which, rather than linking the different branches together, increased the specialization of each subdiscipline. Such universal minds as Poincaré and Hilbert seemed to belong to the past. Now mathematicians only hoped to master their own specialties, each of which possessing its own terminology and methods not necessarily applicable to other fields of mathematics. Worse still, the same terms sometimes meant different things when used in different fields. The question was therefore worth raising: "Is the mathematics of today singular or plural?" (Bourbaki [1948] 1971, 24).[12]

For Bourbaki, as for Hilbert, there was really no choice to be made. The unity of mathematics was taken for granted; and if it was not unified, then the goal was to strive for unity. He believed that

> the internal evolution of mathematics has, in spite of appearances, tightened the unity of the various parts. . . . The essence of this evolution has consisted in a systematization of the relations existing among the various mathematical theories, and is comprised in an approach generally known under the name of the "axiomatic method." (P. 25)

The axiomatization of various branches of mathematics was hardly something new in 1948. But Bourbaki based his use of axiomatics on a new concept that allowed him, so he thought, to extend this method coherently to all of modern mathematics. "The common trait of the various notions designated by [the]

[12] In what follows, I quote from the version published in Le Lionnais 1971 and indicate the page number in parentheses.

generic name" *mathematical structure,* Bourbaki wrote, "is that they apply to sets of elements whose nature is *not specified*" (p. 28). This term had first entered Bourbaki's discussions in 1936, with a sentence close to his later conception: "the object of a mathematical theory is a structure organizing a set of elements" (quoted by Beaulieu 1989, 327). Then considered as not definable, and not formally defined until 1957, the notion of structure always remained problematic for Bourbaki's enterprise, yet at the same time central to his discourse (Corry 1996).

Bourbaki liked to recall that natural numbers were a structured set of elements that had lost all relation to referents. Nobody used different arithmetics to count apples, sheep, francs, or kilos. Above all, Bourbaki's archetypal example was the algebraic structure of *groups,* which had proved fruitful in algebra as well as in physics and chemistry. In his article, he popularized the notion of groups, i.e. sets endowed with a law of composition that satisfied three simple axioms. As we shall see, groups were also used by Weil in his appendix to Lévi-Strauss's book. They thus became the mathematical structure *par excellence* for non-mathematicians.

What Bourbaki did not say was that the three group axioms were not arbitrary. Mathematicians had slowly come up with the group concept, after having been exposed through decades of experience to similar properties in many different cases (Wussing [1969] 1984). With Bourbaki, here following earlier mathematicians, collections of objects that mathematics had studied for centuries were turned into mere instances of groups. For example, the set Z of integers (0, 1, 2, 3, etc., and negative numbers) with respect to addition, real numbers (without 0) with respect to multiplication, rotations in the plane, substitions, etc., were all groups.

Just as we can always be sure that two apples and two apples make four apples because $2+2=4$, Bourbaki showed that, using the group axioms, it was possible to prove once and for all certain properties that applied to all groups. For example, the theorem that if $xy = xz$, then surely $y = z$ can be proven at once for all groups. Above all, Bourbaki wanted his readers to note that "the *nature* of the elements x, y, z was completely irrelevant in this argument" (p. 28). This is why structures were defined as sets of elements whose nature was arbitrary. In his conception of mathematics Bourbaki noted that the only "mathematical objects" that remained were structures. Three great types of structures existed; they were the mother structures of order, algebra, and topology. A whole hierarchy of increasingly complex structures, which could be built upon this foundation, formed the true architecture of the mathematical edifice, not the old disciplines of algebra, geometry, analysis, number theory, etc. Confidently, Bourbaki proclaimed:

> On these foundations, I state that I can build up the whole of the mathematics of the present day; and if there is anything original in my procedure, it lies solely in the fact that, instead of being content with such a statement, I proceed to prove it in the same way as Diogenes proved the existence of motion; and my proof will become more and more complete as my treatise grows. (Bourbaki 1949, 8)

In practice, such a construction followed the axiomatic method. "In order to define a structure," Bourbaki wrote, "one or several relations involving [the] elements are given; . . . it is then postulated that the given relations satisfy certain conditions . . . which are the *axioms* of the structure envisaged" (pp. 28–29). When the axiomatic basis for a theory was in place, the rest was a game of internal logical deductions. "To study the axiomatic theory of a given structure is to deduce the logical consequences of the axioms of the structure, *while excluding all other hypotheses* about the elements considered (in particular, any hypothesis concerning their special 'nature')" (p. 29). Mathematicians thus had their goals set for them. For the next few centuries, they needed only to study the logical implications of their axioms. They could do away with external inspiration. In summary, Bourbaki's general attitude expressed that "mathematics [was] an autonomous abstract subject, *with no need of any input from the real world,* with its own criteria of depth and beauty, and with an internal compass for guiding future growth" (Lax 1989, 455–56).

Of course, mathematicians were not abstract thinking machines, and Bourbaki acknowledged that intuition had an important role to play in research; not "the intuition of common sense, but rather a sort of divination (prior to all reasoning) of the normal behavior [mathematicians] had a right to expect from the mathematical entities which a long association had rendered familiar to [them] as objects of the real world" (p. 31). This intuition was thus purely internal to the logic of mathematics; it was an immediate knowledge of structures and nothing more.

Bourbaki's axiomatics isolated mathematics from any other field of knowledge. He did away with both the historical reliance on physics and the foundation on logic. In 1948 Weil listed Van der Pol's equation as "one of the few interesting problems which contemporary physics has suggested to mathematics; for the study of nature, which was formerly one of the main sources of great mathematical problems, seems in recent years to have borrowed from us more than it has given us" (Weil [1948] 1971, 332). On the other hand, Bourbaki claimed that mathematics was relatively independent of formal logic at the level of the "working mathematician," because whatever foundational questions remained, his approach being constructive, the sole constraint was that the construction stayed free from contradiction, which was the case up to the present (Bourbaki 1954, Introducton).

Axiomatics freed mathematics from reality, or rather from errors due to the abuse of intuition. Its utility for other sciences therefore remained an open question for Bourbaki. "That there is a close connection between experimental phenomena and mathematical structures seems to be confirmed in a most unexpected manner by the recent discoveries of contemporary physics; but we do not know at all the deep-lying reasons for this, . . . and we may never know them" (p. 35). From his point of view, "mathematics appears on the whole as a reservoir of abstract *forms* — the mathematical structures; and it sometimes happens, without anyone really knowing why, that certain aspects of experimental reality model themselves after certain of these forms" (p. 36). Moreover, this position conveniently made mathematics independent of the moral choices faced by politicians, engineers, and other scientists. "Why have some of the most intricate theories in mathematics become an indispensable tool to the modern physicist, to the engineer, and to the manufacturer of atom-bombs? Fortunately for us, the mathematician does not feel called upon to answer such questions, nor should he be held responsible for such use or misuse of his work" (Bourbaki 1949, 2).

Structures of Kinship

If Bourbaki was so skeptical of other sciences having a role to play in the direction of contemporary mathematical research, how are we to understand André Weil's collaboration with Claude Lévi-Strauss? Weil did, after all, write a mathematical appendix for *The Elementary Structures of Kinship.* In fact, his involvement conformed to Bourbaki's philosophy. Bourbaki welcomed the application of mathematics to other fields of knowledge. The emphasis here must be put on the word "application," which already presumes the nature of the relation between mathematics and science (Lévy-Leblond 1968). Bourbaki felt that mathematics should remain free from external influences. He considered that problems of application were in themselves quite unappealing, since they would not entail the development of new mathematics; but he was happy to witness the use of his mathematical theories by others, including (but rarely) his collaborators.

Perhaps only the special circumstances of World War II, which sent both of them to the Ecole libre des hautes études (Free School for Advanced Studies) in New York City, a university for exiles, made it possible for Lévi-Strauss and Weil to work on a common project. The anthropologist Lévi-Strauss started to work on what would become *Elementary Structures* in 1943. Soon, according to his recollections, he faced problems of Australian kinship so complex that he thought only a mathematician could solve them. He first went to see Jacques Hadamard, an accomplished mathematician in his seventies, who told him that he could not help. Lévi-Strauss then turned to Weil, who worked out a scheme that involved groups. Hadamard's and Weil's reactions nicely encapsulate the views of their respective cohorts about the proper objects of mathematics. While Hadamard said that "mathematicians [knew] only four operations and that marriage was not one of them," Weil countered that there was no need "to define marriage from a mathematical standpoint. *Only relations between marriages are of interest*" (Lévi-Strauss and Eribon [1988] 1991, 52–53, my emphasis; see also Weil [1991] 1992, 185). Whereas for Hadamard marriage was not amenable to mathematical treatment because it was not a mathematical object, Weil could not care less. In Bourbakist thought the nature of objects was irrelevant; only the structure of sets mattered.

Lévi-Strauss could not have agreed more. But he had reached this conclusion by following a different route. He often acknowledged that his notion of structure was imported from linguistics. In New York he befriended the Russian-born

linguist Roman Jakobson, who taught structural phonology (or phonemics) at the Ecole libre (Lévi-Strauss and Eribon [1988] 1991, 41; Lévi-Strauss [1983] 1985, 138–147). The history of structural linguistics has often been told, starting with Saussure at the beginning of the century and culminating with the French structuralist movement of the 1960s, via the interwar Prague Linguistic Circle, to which Jakobson belonged (Benoist [1975] 1978; Dosse 1991–92; Harland 1987; Merquior 1986; Pavel 1989). The Prague linguists started to use the term "structure" around 1929.

Incidentally, Bourbaki's use of the term possibly stems from the same source. "As for the choice of the word 'structure,' my memory fails me," André Weil admitted in his memoirs, but he ventured this explanation: "At the time, I believe, it had already entered the working vocabulary of linguists, a milieu with which I had maintained ties (in particular with Emile Benveniste)" (Weil [1991] 1992, 114). However, we should remain somewhat doubtful about this late recollection. Bourbaki adopted the term just a few months after the election of the *Front Populaire,* who popularized the phrase "réforme des structures" for its nationalization policy. Furthermore there was a history of using the term in mathematics as well. In the 1880s, Sophus Lie had already talked of the "structure of a group"; and Elie Cartan (Henri's father) wrote a thesis in 1894 entitled *La Structure des groupes continus.* Later, early in the twentieth century, Ore and Glivenko used "structures" for what are now known as lattices (Marchal 1962, 63–67; Bastide 1962, 140–41). As a consequence, a member of Bourbaki could write that one of the goals of contemporary mathematics was "the structural analysis of already known facts" (Chevalley 1935, 384). There is therefore no firm historical reason to assume, on the sole basis of their common name, that the mathematicians' structures and those of the linguists were closely related.

Lévi-Strauss was clearly inspired by the linguists, rather than the mathematicians. "Linguistics occupies a special place among the social sciences," he wrote in 1945. "It is probably the only one which can truly claim to be a science" (Lévi-Strauss [1958] 1963, 31). During his field work in Brazil, he had difficulty denoting some Amerindian languages. Thinking that acquiring the basics of linguistics might help him, he was happy when Alexandre Koyré introduced him to Jakobson. Lévi-Strauss benefited much more than he had anticipated from this encounter. "At the time I was a kind of naive structuralist, a structuralist without knowing it. Jakobson revealed to me the existence of a body of doctrine that had already been formed within a discipline, linguistics, with which I was unacquainted" (Lévi-Strauss and Eribon [1988] 1991, 41). What Jakobson taught in his course on phonology could be directly applied, Lévi-Strauss thought, to anthropology.

In his lectures at the Ecole libre, published in 1976 with a preface by Lévi-Strauss, Jakobson investigated the union between the sound of a spoken word and its meaning or, in Saussurian terms, between *signifier* and *signified.* If previous schools had carefully studied the physiological origins of human phonemes — that is, phonetics — they had substituted "strictly causal questions for questions concerning means and ends" (Jakobson [1976] 1978, 35; Lévi-Strauss's preface is reprinted in Lévi-Strauss [1983] 1985, 138–47). They went back to the origins of the phenomena without having properly described them. They were thus faced, Lévi-Strauss quoted, with a "'stunning multitude of variations,' whereas explanations ought always to aim at the discovery of 'the invariants behind all this variety'" (Lévi-Strauss, in Jakobson [1976] 1978, xii; Lévi-Strauss [1983] 1985, 139).[13] Phonology was the structural analysis of phonemes not in the specific forms in which they appear but with respect to their relations to one another (usually binary oppositions) within a system. Or as Trubetzkoy, another member of the Prague Circle wrote in 1933, "phonology, universalistic by nature, starts with the [linguistic] system as an organic whole, whose *structure* it studies" (quoted by Benveniste 1962, 35). From the late 1920s onward, Jakobson always conceived of this idea of considering objects not for what they were but for how they related to one another, as a general trend pervasive of all aspects of science and culture. In particular, he identified this trend as being constitutive of modern mathematics as well, exemplified by Felix Klein's *Erlanger Programm* (Jakobson [1961] 1971, 632–37). To name this common trend, Jakobson coined the word "structuralism" in 1929.

> Were we to comprise the leading idea of present-day science in its most various manifestations, we could hardly find a more appropriate designation than *structuralism*. Any set of phenomena examined by contemporary science is treated not as a mechanical agglomeration but as a structural whole, and the basic task is to reveal the inner, whether static or developmental, laws of this system. (Quoted by Holenstein [1974] 1976, 1)

Lévi-Strauss wanted to uncover common features among systems of kinship. How make sense of the mind-numbing variety encountered in different cultures? Who was allowed to marry whom? And why was incest, unique among this rich diversity, a universal taboo? From Jakobson's linguistics, Lévi-Strauss learned that "instead of being led astray by a multiplicity of terms, one should consider the simplest and most intelligible relationships uniting them" (Lévi-Strauss [1983] 1985, 139; Jakobson [1976] 1978, xii). Indeed, an important aspect of Jakobson's structuralism was his reductionist focus on the smallest unit of spoken language, the phonemes. Similarly, Lévi-Strauss emphasized elementary structures, determined by the internal dynamic of kinship, rather than the more complex ones depending on economic or political relations. For him, the first and foremost characteristic of a structure was that "*it consists of elements such that any modification of one of them entails a modification of all others*" (Lévi-Strauss [1952] 1958, chap. 15, part 1; my emphasis).[14] With the help of such a structural

[13] Lévi-Strauss is quoting from Jakobson, [1976] 1978, 9.
[14] In this text, Lévi-Strauss considered analogies in mathematics, taken from cybernetics, information theory, and game theory, rather than from Bourbaki.

analysis, he reformulated, and turned around, the question of kinship. Incest was prohibited because of the necessity of alliances between clans, and not the reverse. Lévi-Strauss synthesized many anthropological traditions, managing "to escape from the Scylla of thoughtless empiricism and the Charybdis of factless philosophizing" (Gardner 1981, 40). Moreover, tapping into the status of structural linguistics helped him emphasize the *scientific* nature of his results.

It will be clear from the above, I hope, that there were significant differences between Lévi-Strauss's structural analysis of kinship and Bourbaki's structural view of mathematics, although they surely exhibited common features. Both aimed at unifying their respective disciplines by emphasizing elementary structures. But while Bourbaki imposed systemic structures onto sets of unspecified elements, Lévi-Strauss emphasized the irreducible relations linking elements together. In the appendix of *Elementary Structures*, Lévi-Strauss underscored the distinction between Weil's analysis and his own method. Concerning one of Weil's results, he wrote that he had already reached the same conclusion by following "a *structural* analysis, and the *mathematical* analysis confirms it" (Lévi-Strauss 1949, 285–86). Most significantly in historical terms, "Weil's intuition of the potential of group theory for the analysis of kinship systems . . . turned out to have no influence on the later work of Lévi-Strauss," although he was never immune to a variety of scientific metaphors.[15] Neither were the two methods totally disjointed, as Lévi-Strauss was well aware. "This mathematical demonstration," he commented in 1988, "proceeded from principles akin to those that Jakobson applied in linguistics, since in both cases the focus moves from the terms themselves to the relationships operating between them" (Lévi-Strauss and Eribon [1988] 1991, 53). In conclusion, we can reasonably say that the intersection of Lévi-Strauss, Jakobson, and Weil, in New York in 1943, by cross-breeding anthropology, linguistics, and mathematics, helped make structuralism possible. And although the dialogue between mathematics and structuralism failed to be sustained, this fortuitous encounter was the seed of a lasting cultural connection.

Part II: Hegemony

From the postwar period to the late 1960s, the authority of structuralism in the human sciences and of Bourbakism in mathematics grew, until they achieved dominant positions within their respective domains. Both arguably peaked around 1966 only to begin a parallel decline. How are we to understand this coincidence? Were these two movements, both propagandizing the use of structure, facets of a

[15] Paul Jorion and Claude Assaba's comments (pp. 377–78) following Barbosa de Almeida 1990, an article in which the importance of Lévi-Strauss's use of mathematics is hotly debated. Weil's models were however developed by White (1963) and Courrège ([1965] 1971) — himself an ardent Bourbakist, it should be noted, according to the romanced portrait in Roubaud 1997, 73–81. See Lévi-Strauss 1954 for his view of the usefulness of mathematical methods; and for a critique, see Régnier 1971, 1974.

larger trend? Did one depend on the other for its success? Or did they speak to one another? I offer here an account of the contacts between the two kinds of structuralism, which highlights the actual effects of a mostly failed discussion. Finally, by focusing on a literary group, the Oulipo, I show the impact that it could have on the outside.

Bourbaki's Reign

Bourbaki did not need structuralism to establish his hegemony over his discipline. He added new booklets to his *Eléments de mathématique,* more than thirty-five of which had been published by the end of the 1970s. As early as 1951 several of the earlier volumes had been revised and republished. In 1958 Russian translations started to appear. In 1966 the first volumes on *General Topology* were translated into English. Meanwhile, Bourbaki's programmatic article "The Architecture of Mathematics" was translated into English, Portuguese, Russian, German, and Japanese (Beaulieu 1989).

But Bourbaki was more than just another successful author. His vision permeated all of mathematics. Some of his collaborators or students were regularly among the winners of the Fields medal, the highest distinction for mathematicians: Laurent Schwartz in 1950, Jean-Pierre Serre in 1954, René Thom (never himself a Bourbaki, but a student of Cartan and Ehresmann) in 1958. In 1966, at the International Congress of Mathematicians held in Moscow, Henri Cartan was elected president of the International Mathematical Union for the next four years. Three of the four Fields-medal winners were introduced by Cartan, Thom, and Dieudonné, and the latter lavishly praised his colleague Alexander Grothendieck when he presented him with the fourth medal in sixteen years awarded to a French mathematician. When one unnamed mathematician "remembered the Bourbaki influence on two other [1966] Fields-prize winners, M. Atiyah and S. Smale, he could not help concluding that the Moscow Congress was indeed dominated by Bourbaki" (quoted by Fang 1970, 58).[16]

If Bourbaki shaped mathematics internationally, this was even truer in France. After World War II, the Bourbakis had become established mathematicians. Henri Cartan became the statesman of French mathematics. Teaching at the *Ecole normale supérieure* from 1940 on, he bred an entire generation of French mathematicians, to whom he would strongly suggest studying Bourbakist mathematics (Andler 1994, 371–80). Jean Dieudonné's voice was heard by a large audience of mathematicians. From 1948 on, they had their own *séminaire Bourbaki,* which became a most prestigious outlet for research and a pageant for job seekers. As a symbol of this rise to prominence, four of Bourbaki's founders received a substantial prize (200,000 francs) from the Academy of Sciences in 1966 (Beaulieu 1989, 160). Surely enough, the Academy would, after the usual lag, fill up with Bourbakis. By 1976, they would occupy three of the six seats of the Mathematical Section (Cartan, Mandelbrojt, and Schwartz), one more Bourbaki (Dieudonné) having been elected as nonresident member and two others (Chevalley and Serre) as correspondents.

[16] Retrospectively, it appears highly problematic to include Thom, Atiyah, and Smale in Bourbaki's sphere of influence, but their styles and topics certainly were, at the time, close to his own. For a view of Bourbaki's international dominance, see Arnol'd 1995.

Mostly, from the forties onward, Bourbaki's dynamic nature oriented ambitious students toward his topics of predilection: algebraic geometry above all, but also number theory, group theory, and algebraic and differential topology, a hierarchy best exemplified by Dieudonné's *Panorama* (1977). Reforms of the higher mathematical curriculum were partly inspired by Bourbaki's treatise and its message. Moreover, Bourbaki's logical rigor, his conspicuous modernity, the proclaimed exhaustiveness of his enterprise, and the absolute certainty of the results he exposed in his treatise, all exerted a powerful appeal for the younger generation of the cold war. Many young men who studied mathematics at the university in the 1940s and 1950s have testified to the subtle blend of pressure and appeal that Bourbaki exerted on the younger generation (see Grothendieck 1985; Serres and Latour [1992] 1995, 10–11; Roubaud 1997; Schwartz 1997; and the account of the mathematical education of Gérard Debreu in Weintraub and Mirowski 1994).

Bourbaki's dominance notwithstanding, there was room for other approaches to develop, even in France. But the following two examples show that this could be arduous. In 1958–59, when some Paris mathematicians feared that the French probabilistic tradition (Borel, Fréchet, Lévy) might be interrupted, they had to invite a French *émigré,* Michel Loève, to "sow the good seed" (Choquet 1996, 13). Among his students was Paul-André Meyer who, with Jacques Neveu, would later build a French school of probability theory, a topic neglected by Bourbaki. A second example is the conference on "Forced Vibrations in Non-Linear Systems," organized by the CNRS (National Center for Scientific Research), and held in Marseilles in 1964. In his introduction the editor noted that problems concerning nonlinear systems were traditionally assigned a place within mechanics. But since new progress in functional analysis was especially exciting to him, this led the study of nonlinear systems to sit on the border of physics and mathematics — an "uncomfortable situation, certainly in France" (Vogel 1965, 11–12). Only in the late 1960s could a French school of applied mathematics develop under the leadership of Jacques-Louis Lions (Dahan Dalmedico 1995; 1996). Meanwhile, as mathematicians looked for ways to get around Bourbaki's dominance over their field, his name had begun being invoked again in the human sciences.

The Rise of Structuralism: The First Interdisciplinary Conferences

At first, Lévi-Strauss's *Elementary Structures* was well received even among existentialists (Beauvoir 1949). But the rise to prominence of a structural approach on the French intellectual scene was slower than in mathematics. Only in the late 1950s did its impact begin to be widely felt, and not before 1966–68 did structuralism definitively replace existentialism at the zenith of French philosophy. Only then did Claude Lévi-Strauss, Roland Barthes, Jacques Lacan, and Michel Foucault take Sartre's place; "those were the names of the 'new masters' [*nouveaux maîtres*]" (Simonin and Clastres 1989, 228), among whom Louis Althusser should definitely be counted. Each of them had developed new methods, borrowing from structural linguistics, which they used, respectively, in anthropology, literary and cultural criticism, psychoanalysis, history, and Marxist philosophy. None of them, however, not even Lacan, who sprinkled his language with mathematical metaphors, felt the need to base his method on Bourbaki's structures. In 1951, Lacan, Lévi-Strauss, and Benveniste had started to meet, together with the mathematician Georges-Théodule Guilbaud, in order to work on structures and find links between the human sciences and mathematics, without seemingly emphasizing Bourbaki's structural approach (Rosenstiehl 1996; Roudinesco 1993, 469). This effort, however, would not be widely felt until later.

From the late 1950s to the late 1960s, structuralism happened: it became a social phenomenon that extended much beyond four or five great "masters." Philosophers, humanists, and social scientists embraced the notion of structure as a fundamental tool for their disciplinary activities and for bridging across different sciences. It was then that Bourbaki massively appeared in the literature dealing with structure. He helped a newer generation — one that was not necessarily younger in age but that followed into the steps of the "masters" — to grasp structuralist scholarship as a coherent whole. Seeking to define "structure," they found Bourbaki useful. Not only did he provide a definition, either formalized in mathematical jargon or simple enough to be used casually, he could also help gather scientific prestige for structuralism.

Following the publication of Lévi-Strauss's *Structural Anthropology* ([1958] 1963), two interdisciplinary conferences were held in 1959 with the explicit aim of mapping out the meaning of "structure." Interestingly, notions of mathematical structures, and especially Bourbaki's, figured prominently at both meetings. On 10–12 January, a symposium on the "Meanings and Uses of the Term Structure in the Human and Social Sciences," sponsored by Unesco and held in Paris, met with the goal of preparing an entry for the *Dictionnaire terminologique des sciences sociales*. The stars of the conference were Lévi-Strauss, Benveniste, and Merleau-Ponty. Later that year, another conference was held, from 25 July to 3 August, at Cerisy-la-Salle, where Swiss psychologist Jean Piaget was the driving force. This conference focused on the dual theme of "Genesis and Structure."[17]

Although both conferences were sponsored in part by the Sixth Section of the Ecole pratique des hautes études, few people participated in both. But to the

[17] The proceedings of these two conferences were subsequently published — Bastide 1962; Gandillac et al. 1965.

sociologist Lucien Goldmann, who did, the approaches of the two conferences differed enough to warrant a distinction, which I shall adopt here, between two sorts of structuralism (Goldmann 1962, 1965). On the one hand there was the more standard "nongenetic structuralism," identified with Lévi-Strauss's, which postulated the existence of permanent and universal structures and relinquished all attempts at explaining them. On the other hand Piaget's "genetic structuralism" strove to explain both the structures and their genesis. If at the Paris symposium the matter of genesis spurred "passionate discussions," the consensus clearly went against an absent Piaget: "The concept of structure appears as a 'synchronic' concept" (Bastide 1962, 17). The lesson taken away from the meeting at Cerisy-la-Salle was the exact opposite: "Genesis without structure would be blind, and structure without genesis would remain empty" (Gandillac et al. 1965, 347; see also Desanti 1965, 153).

Contrary to the two conferences above, which offered clear visions of what structuralism should be, a third meeting had been held in Paris three years earlier, on 18–27 April, 1956, whose theme already was: "Notion of Structure and Structure of Knowledge." Organized by the Centre international de Synthèse, under the auspices of the old Sorbonne, this conference might be characterized, I suggest, as a *nonstructuralist* (and rather unsuccessful) attempt at synthesizing knowledge with the help of the notion of structure. By studying the role played by this notion in several disciplines, the organizers of the *Synthèse* week hoped to exhibit an "*isomorphism* between different sectors of knowledge" and deal with "the problem of the structure of the synthesis of the sciences." Because it raised more questions than it provided answers, this "week" remained distinctly nonstructuralist: "no solution has been found; the structure of knowledge has not been defined." Lévi-Strauss's name was only once mentioned in passing; none of the other usual names (Jakobson, Lacan, Barthes, etc.) was invoked; linguistics was completely neglected. In my view, the *Synthèse* week at least demonstrates that the notion of structure was then very commonly used and that in 1956, as opposed to 1959, it was up for anyone to grab.[18]

Given the considerable divergence among all three conferences on a number of fundamental issues, it is remarkable that each time mathematics played a compara-ble role and was given the same kind of prominence. Because of the endorsement it could offer, mathematics exerted a universal appeal. At both 1959 structuralist conferences participants eagerly emphasized the scientific character of their endeavor. Genetic and nongenetic structuralism tapped into the scientific prestige of biology and mathematics. But while both disciplines could offer legitimacy, the models they proposed were different. Biology served as a model for those empha-sizing relations among elements of the structure, mathematics for those studying its systemic essence. Significantly, the Paris symposium emphasized

[18] These proceedings were also published — see Synthèse 1957; and for quotes, see pp. xi–xii and xxiii.

biology, especially in the published proceedings, whereas the Cerisy conference considered biology in a rather inconsequential manner.

On the conceptual level, assessments of the structural view of mathematics, always emphasizing Bourbaki, were strikingly similar at all three meetings. Its most important contribution was precision. "For a mathematician, the meaning of the notion of structure offers no ambiguity at all" (Desanti 1965, 143). For Jean Desanti, Daniel Lacombe, and Georges Guilbaud — who, respectively, represented the mathematicians' view at the Cerisy conference, the *Synthèse* week, and the Paris symposium — structure was both a notion and a term that had internal histories in mathematics, which culminated in, but did not end with, Bourbaki. What a structure was for Bourbaki — an axiomatized collection of relationships among elements of a set, whose nature remained unspecified — was readily acceptable for any brand of structuralism.

The debate therefore was elsewhere: it focused on whether there was a nontrivial core to the notion of structure, and in particular on whether Bourbaki's definition meant something outside of mathematics. A deeper look at the two types of structuralism reveals that they diverged in their actual use of mathematics. In general, nongenetic structuralism was rather immune to mathematics. Even if at the Paris symposium mathematics started the show with Guilbaud's presentation, no one really talked about it later (except for Merleau-Ponty), and no specific piece on the mathematical uses of structure was included in the proceedings. Whether biologist, linguist, economist, historian, psychologist, political scientist, or lawyer, each participant showed that he was quite comfortable using structures without (explicitly or implicitly) referring to mathematics. It was moreover thought that the mathematician's unified definition hid the "splintered character" that the concept bore in the human sciences and, in the end, might not be very useful (Pagès in Bastide 1962, 156).

Paying lip service to or totally ignoring mathematics became a widespread attitude in (nongenetic) structuralism. In the late 1960s, a flurry of books and special journal issues dealt with the fashion that structuralism had become, introducing, explaining, or criticizing it for a wide range of readers; Bourbaki had seeped into intellectual folklore because of his high profile in the mathematical community and his alleged role in educational reforms. He had become a synonym for rigor, axiomatics, and set theory. Many authors, however, agreed that mathematics was *not really* a part of the structuralist vogue[19] — which of course is not very surprising considering that Bourbaki never informed the works of any great structuralist thinker other than to provide an illustration for the complexity of authorship (Foucault [1969] 1994, 797)!

[19] There is no mention of mathematics or Bourbaki in, for example, the special issues of the journals *L'Arc* (1965), and *L'Esprit* (1963, 1967), nor in the books of Fages 1967 and Ducrot et al. 1968, except for Sperber's chapter on anthropology, 179–80, and 223–28. Laconic passages appear in Auzias 1967, 9, and Corvez 1969, 12–13.

Jean Piaget and Genetic Structuralism

Swiss psychologist Jean Piaget, unlike most famous structuralists, was serious about mathematics in his "genetic structuralism," as Goldmann labeled it. At the same time Piaget's structural vision of the sciences was extremely influential. In 1959 he greatly inspired the Cerisy conference; and a decade later he published a short survey of structuralism, which was probably read by more people than any other. Published in the popular series *Que sais-je?*, the book had more than 55,000 copies printed in 1968 alone, its first year of publication. There, Piaget strongly empha-sized the centrality of mathematics. "A critical account of structuralism," he wrote, "must begin with a consideration of mathematical structures" (Piaget [1968] 1970, 17).

Mathematics as the key to a structural synthesis of the sciences was one of the suggestions that had already emerged during the *Synthèse* week in 1956. Although it failed to gather a consensus, this view was strongly defended. Interestingly the man who argued the most forcefully for this view was none other than François Le Lionnais, editor of the *Great Currents of Mathematical Thought:*

> When I turn to the outer world, I everywhere see laws of composition, neighborhoods, orders, [and] equivalencies. Here are thus four structures that, without being immoderate, we can consider as fundamental and that operate at all instant, in the domain of human realities as well as in the world of physics. I think that if we became better aware of this, we would achieve some progress. (*Synthèse* 1957, 415)

In 1959 the Cerisy conference, unlike the Paris symposium, did not start with mathematics. But Piaget brought it up forcefully on the second day. While it cannot be said that mathematics dominated the debates at Cerisy-la-Salle, it nevertheless constantly remained in the background. Bourbaki's were prime examples of what Piaget meant by structures — i.e., "systems that, as systems, presented laws of totality distinct from the proprieties of their elements" (Piaget 1965, 37). Indeed, if not all structures were mathematized, he "very resolutely leaned" toward thinking of them as at least potentially mathematizable (ibid., 54).[20]

Forty years of experimentation with children had lead Piaget to believe that, at each stage of the development of intelligence, thought processes came in highly structured ways. He used one of his famous experiments as an example (ibid., 44–48). A child is presented with two identical balls of clay, then one is rolled up into a sausage, and she is asked whether the ball or the sausage has more clay. Typically, the emergence in a child's mind of the principle of matter conservation, Piaget contended, will follow four stages. At first the child is likely to say that the

[20] Later, Jean Ullmo offered an extreme version: "Generally a structure can *only* be precisely defined by a group [of transformations]" (Ullmo 1969, 262, my emphasis).

sausage contains more clay because it is longer than the ball. Then she inverts her reasoning, focusing on thickness. She begins to doubt her deduction. Thirdly, the child considers both directions, but is confused. She discovers the solidarity between the transformations. Finally the child realizes that they are inverse, and a structure crystallizes in her mind: the matter conservation principle. For Piaget, this example showed that mental structures emerge in a sequence that is also structured. Moreover it underscored the intimate relation between structures and their genesis, so distinctive of Piaget's structuralism.

Piaget believed that the acquisition of propositional logic was crucial to a child's intellectual maturation. The mental structures enabling teenagers to think logically were themselves modeled on mathematical structures, such as the group structure. He contended that between twelve and fifteen years old, children acquired a new structure "whose influence is very striking in every domain of formal intelligence" (ibid., 40). It included four types of transformations that could be applied to logical propositions: the *identical (I), negative (N), reciprocal (R),* and *correlative (C)* transformations. He took for example the proposition "p implies q" (or equivalently "not p or q"), whose converse is "q implies p," and whose negation is "p and not q." Its correlative was defined as the permutation of *and*s and *or*s, or equivalently the converse of the negation — i.e., "not p and q." Since each of these transformations applied twice fell back on the identity, and taken two by two they were equivalent to the third one ($NR = C$, $CR = N$, and $NC = R$), they formed a group of four elements (Piaget 1949, 268–86). For Piaget, this group was inscribed in our minds, enabling us to perform the most basic logical operations. The mental structures of intelligence were none other than the mathematician's structures — or at least this was the desirable ideal.

Among all mathematical structures, the most important were Bourbaki's three mother structures (algebraic, topological, and order structures). They "correspond to elementary structures of intelligence" (Piaget 1955, 17). Once again it was a direct encounter with Bourbaki, this time in the person of Dieudonné, that led Piaget to this belief. In April 1952, they spoke at a conference outside Paris on "Mathematical Structures and Mental Structures," in relation with the International Commission for the Study and Improvement of Mathematical Education. Later, Piaget recalled the impression that this encounter had made on him:

> Dieudonné gave a talk in which he described the three mother structures. Then I gave a talk in which I described the structures I had found in children's thinking, and to the great astonishment of both of us, we saw that there was a direct relationship between these three mathematical structures and the three structures of children's operational thinking. We were, of course, impressed with each other. (Piaget 1970, 26)

By that time, Piaget had embarked on an ambitious project, which, bluntly put, aimed at making a science out of epistemology. In 1950 he published *Introduction à l'épistémologie génétique,* in which he argued that since the roots of the spontaneous psychological development of arithmetic and geometric operations in children paralleled the concepts used by mathematicians (but not Bourbaki yet), the "linear order" of science extolled by Vienna Circle positivists (and Auguste Comte earlier) was to be replaced by a "circle" (Piaget 1950, 1:49–50; Gayon 1989, 162). For him, the "logico-mathematical" sciences, on which the rest of science — including the sciences of man — was supposed to be built, in turn rested on the structures of the human mind. Or as Léo Apostel defended with more nuance: "There are laws of thought such that, in a certain social structure and for individuals possessing certain properties, we can infallibly constrain these individuals to accept our conclusions, if they accept our premises" (quoted by Piaget 1955, 30). After he met Dieudonné and studied "The Architecture of Mathematics," Piaget realized that the structures he had been talking about could be equated with Bourbaki's mother structures. Bourbaki's structuralism therefore significantly informed Piaget's own conception of structuralism.

Piaget's genetic structuralism appeared as a unified methodology, equally applicable to logic and mathematics, physics and biology, psychology, linguistics, and the social sciences. In some respects, it was a strange view of structuralism, one that excluded the likes of Lacan and Barthes, harshly criticized Foucault's "structuralism without structures," and praised Noam Chomsky's work as the epitome of linguistic structuralism. Furthermore, Piaget saw a "direct adaptation of general algebra" in Lévi-Strauss's structural models and commended Bourbaki for "subordinat[ing] all mathematics . . . to the idea of structure" (Piaget [1968] 1970, 17, 23). By emphasizing the ontology of structures, he diverged from most structuralists. "What structuralism is really after is to discover 'natural structures' — some using this somewhat vague and often denigrated word to refer to an ultimate rootedness in human nature, others, on the contrary, to indicate a non-human absolute to which we must accommodate ourselves instead of the reverse" (ibid., 30). In Piaget's conception, these two alternatives were really just one, since he saw Bourbaki's atemporal structures as rooted in the human brain.

Still, if Piaget's book satisfied a thirst for a clear, straightforward explanation of structuralism, it hardly mirrored its diversity at a time when structuralism was quickly becoming a meaningless fashion from which its initial propagandists often seemed eager to distance themselves. A sure sign that structuralism was coming to the forefront of the French intellectual landscape came when *Les Temps modernes* devoted a special issue to "its problems." In his introduction, Jean Pouillon admitted: "Structuralism is indeed in fashion. Fashions have the exasperating aspect that by criticizing them one gives in to them" (Pouillon 1966, 769). By the late 1960s, when ironically it became a popular fad, the structuralist movement was losing all coherence, supposing it had some to start with. For Georges Canguilhem, "'structuralism' means nothing. . . . It is a journalist's concept, but not the concept of a scientist [*savant*], who himself knows very well that he is dealing with structures, but which he defines in a given way in mathematics, biology, linguistics, etc." (Auzias et al. 1970, 238). Piaget's book appears, in retrospect, as a desperate effort to present a unified structuralism with scientific pretense.

One last-resort attempt at salvaging structuralism indeed distinguished between the true scientific uses of structures and the merely ideological ones. For this reason Piaget found others who concurred in seeing modern mathematics as a prime example of structuralism. The more an author held on to the belief that structural methods offered the best hope for truly *scientific* social and human sciences, the more she would see mathematical structures as an exemplar for human structures. In 1969 Jeanne Parain-Vial explicitly made this distinction between science and ideology.[21] In order to criticize better the ideologies she attributed to Lacan, Althusser, and Foucault, she presented a panorama of scientific uses of structures, the first of which was Bourbaki's. Following a familiar strategy, she laid the emphasis on the clarity of the mathematical usage. She nonetheless pointedly questioned whether human structures really were the same as Bourbaki's.

The Oulipo: Bourbakist Literature?

"My idea of prose was greatly influenced by . . . Bourbaki's famous treatise" (Roubaud 1989, 148). Indeed, social scientists and mathematicians were not alone in toying with structures. There is perhaps no more telling sign of the hegemony of structuralist modes of thought in certain French intellectual milieus, and of Bourbaki's role as a cultural connector, than the story of the literary group that was called Oulipo — an important source of inspiration for writers like Georges Perec and Italo Calvino who belonged to it.

On 24 November, 1960 a peculiar semisecret literary society was founded, inspired mainly by the mathematician François Le Lionnais and the writer and amateur mathematician Raymond Queneau.[22] At their second meeting they adopted the name Ouvroir de littérature potentielle (Workshop for Potential Literature), abbreviated as Oulipo. Their somewhat surprising premise was that, as "mathematicians and scribblers [*écriverons*], we have the right to expect that our meetings will contribute to shedding light on the exercise of our respective activities" (Bens 1980, 20). They sought to experiment with formal constraints imposed on the production of literature. In a 1962 interview on French radio, Queneau defined potential literature thus: "The word 'potential' concerns the very nature of literature; that is, it's less a question of literature strictly speaking than of supplying forms for the good use one can make of literature. We call potential literature the search for new forms and *structures* — to use this slightly learned

[21] A similar strategy of distinguishing scientific method and ideology was at play in a critical conference held in 1967–68, cf. Auzias et al. 1970.

[22] A proud member of the Société mathématique de France, Queneau also published two articles in scientific journals, Queneau 1968, 1972.

word — that may be used by writers in any way they see fit" (Charbonnier 1962, 140; quoted [and slightly modified] by Jean Lescure in "A Brief History of the Oulipo," in Motte 1986, 38; my emphasis). Once again: structures! But whose, Bourbaki's or Lévi-Strauss's?

In his "second manifesto," François Le Lionnais opted for the former. He specified that Oulipism exhibited "a syntactic structurElist perspective [*sic*]," begging his readers not to confuse this word "with structurAlist, a term that many of us consider with circumspection" (Motte 1986, 29). Therefore, while the Oulipians sometimes invoked Lévi-Strauss's name, thought of meeting with Foucault, and seem to have been in contact with Lacan, their main inspiration was emphatically scientific and mathematical. "We live in the middle of the 20th century," declared Queneau. "*Everything* presents a rapport with science" (Bens 1980, 49). Like Piaget, Queneau conceived of the organization of science as a circle, and there was "nothing to stop Poetry from taking its place in the centre" (Queneau 1967, 864).[23] The role of mathematics was to provide the Oulipians with abstract structures that could be imported into literature. "Mathematics," Le Lionnais added, "particularly the abstract structures of contemporary mathematics, proposes thousands of possibilities for exploration, both algebraically, . . . and topologically" (Motte 1986, 27). But how were they supposed to use these structures in writing? This would remain a constant matter of discussion, as Queneau kept pushing the mathematicians to "give" them more abstract mathematical structures to play with (Bens 1980, 238; Le Lionnais 1973).

Their favorite exemplar of potential literature was Queneau's stunning *Hundred Thousand Billion Poems* (1961). On the face of it, this was just a collection of ten sonnets, each comprising fourteen verses, as it should. But their structure was so carefully designed that each line of a poem could be replaced by its homologue from any of the nine others, while preserving rhythm, rhyme, and grammatical structure of the newly obtained poem. Thus the first four verses of the first poem:

Le roi de la pampa retourne sa chemise
Pour la mettre à sécher aux cornes des taureaux
Le cornédbîf en boîte empeste la remise
Et fermentent de même et les cuirs et les peaux
[The king of the pampas turns his shirt
To let it dry on the horns of the bulls
The canned corned beef makes the shed stink
And so are fermenting leathers and skins.]

could be turned into grammatically correct, rhyming nonsense, such as (replacing the verses above by the corresponding ones from, respectively, the sixth, first, second, and tenth sonnets):

[23] Note that Piaget edited a volume on "logic and human knowledge" for the *Encyclopédie de la Pléiade*, whose general editor was none other than Queneau (Piaget 1967).

Il se penche il voudrait attraper sa valise
Pour la mettre à sécher aux cornes des taureaux
Le Turc de ce temps-là pataugeait dans sa crise
Et tout vient signifier la fin des haricots
[He bends down he would like to grab his luggage
To let it dry on the horns of the bulls
The Turk from that time became entangled in his crisis
And everything comes to signify the end of beans.]

The global result was a *potential* 10^{14} perfectly legitimate sonnets — much more than anyone, including the author, could hope to read in their entire lifetime! This accomplishment however is deceiving. The only mathematics that it might involve was combinatorics, disdained by Bourbaki for providing "problems without posterity" (Dieudonné 1977, xii). More in line with Bourbaki's interests were the repeated but rather unsuccessful efforts made notably to exploit the notions of "intersection" of classic texts, "boundaries" of poems, etc. (Bens 1980, passim.).

The Oulipo cataloged both new structures and old ones unearthed from the depths of literary history. Le Lionnais called these two activities: "synthoulipism" (synthesis + Oulipism) and "anoulipism" (analysis). While the former "examines and classifies ancient and modern texts [and] extracts from them their apparent or hidden structures and constraints," Noël Arnaud explained, the latter "invents entirely new structures . . . often starting from new mathematics" (Bens 1980, 9). The Oulipians embraced history as a whole. When they discovered, Le Lionnais declared, "that a structure we believed to be entirely new had in fact already been discovered or invented in the past, . . . we make it a point of honor to . . . qualify the text in question as 'plagiarism by anticipation'" (Motte 1986, 31; see also Bens 1980, 179). Did this attitude also characterize Bourbaki's often criticized teleological view of history ([1960] 1994)?

The things that interested the Oulipo as a group were not specific examples but methods. In the reports of their first forty meetings, the Oulipians never seem concerned with the message or politics of a piece of literature, and hardly ever with its esthetic quality. "The method in itself suffices. There are methods without examples. The example is an additional reward that one allows oneself," Le Lionnais mused (Bens 1980, 81). "The very meaning of the Oulipo is to provide empty structures," Queneau concurred (Charbonnier 1962, 154–55). This is of course reminiscent of Bourbaki's distaste for application.

Nicolas Bourbaki always inspired the Oulipo, which included a few mathematicians (Claude Berge, Jacques Roubaud). Queneau once, in March 1962, visited a Bourbaki congress (*La Tribu: Bulletin œcuménique, apériodique et bourbachique,* 56 [Congrès d'Amboise, March 1962], 1).[24] He helped popularize his work: "The article [Queneau 1962] that represents the intersection of these two interesting personalities," wrote Le Lionnais, "constitutes a subset of the issue no. 176 of *Critique*, . . . according to which it is allowed to detect [*subodorer*] a few isomorphisms between Queneau and Bourbaki" (Le Lionnais 1962, 68). Both indeed shared a common insistence on axiomatics, formal beauty versus future utility, and especially humor, which seemed to delight Queneau.

Beyond humor, the Oulipo was much like Bourbaki. Both were semisecret societies founded on myths; both looked at the formal bases of their respective disciplines and wished to rewrite their histories from the current structural perspective; and both left to their members the task of producing original work based on structural approaches (new texts, new theorems). While Bourbaki always remained a significant source of structures for the Oulipo, Le Lionnais toyed with the idea of founding an OuMathPo, which would investigate the fecundity of their approach for mathematics (Le Lionnais 1975, 279, note; Motte 1986, 190, note 3). Even if some Bourbakis appreciated Oulipian prose, an OuMathPo seems never to have gotten off the ground (Chouchan 1995, 9).

In conclusion, as he reigned over mathematics Bourbaki became an omnipresent cultural connector across the French cultural landscape. While some writers seriously tried to adapt the Bourbakist tools to formal literature, this scarcely was the case in the human and social sciences. For some exegetes of structuralism, Bourbaki provided a compelling model, but one that social scientists could hardly live up to. Thus almost none of them, with the notable exception of Jean Piaget, attempted to base their structural method on his mathematics in a critical way. Bourbaki served most usefully as a guarantor for rigor, a signal meaning that structuralism was real science. Typical of late modernist thought, structural discourses in mathematics, literature, and the social sciences were on the lookout for hidden essences. The meaning of the world was to be achieved either by abstracting structures from messy external appearances or by constructing them. Thus even if the dialogue between these discourses rarely succeeded in connecting them in a meaningful way, their demise would be common. Once the attacks on self-reflexivity, metanarrative, and senseless abstraction were launched, they would ring true across disciplinary boundaries of discourses that had been used as resources for one another. But the attacks would come mostly from epistemic concerns specific to each discipline.

Part III: Decline

The decade of the 1970s witnessed an effacement from prominence of both Bourbaki in mathematics and structuralism on the French intellectual scene. At the same time, Bourbaki consequently ceased to play an important role as a cultural connector, and new ones took his place. The history of this recent time however remains, for the most part, to be written, a task that cannot be envisaged

[24] I thank Liliane Beaulieu for some issues of Bourbaki's internal newsletter. Interestingly, Queneau's *Exercices de style* was cited by Claude Chevalley in a rejected draft of the introduction to Bourbaki's *Theory of Sets*. I thank Catherine Chevalley for providing me with a copy of this 1951 draft.

in the confines of this article. By following, in the work of Michel Serres and Bourbaki, the misfortune of the structure concept and the subsequent rise of new mathematical ideas such as catastrophes and fractals (which I select for their importance as cultural connectors at the interface of mathematics, the social sciences, and philosophy), I want to suggest that, although internal dynamics or social factors could be mobilized to account for the demise of structural approaches in different disciplines, an understanding of the concordance involves the recognition that a discussion established earlier went on with different cultural connectors, with an impact that resonated widely.

Michel Serres: From Structuralism to Post-Bourbakism

To insist too stringently on modeling the definition of structure on Bourbaki's leads to an oddity: that "the only philosopher in France to abide by the structuralist method, defined in this way, would no doubt be Michel Serres" (Descombes [1979] 1980, 85). A media figure and an idiosyncratic thinker, the philosopher and historian of science Serres nonetheless provides a useful guiding light for looking at the parallel unraveling of structuralism and Bourbakism. In a series of books, he described, mostly without footnotes, a personal evolution that should be seen here as an illustration, not as a direct cause, of general intellectual shifts.

In 1961, Michel Serres, like Piaget, saw the idea of structure as stemming directly from Bourbaki's mathematics. By now, his definition should sound familiar: "A structure is an operational set with an undefined meaning, ... grouping any number of elements, whose content is not specified, and a finite number of relations whose nature is not specified." Despotic, Serres insisted: "The term 'structure' has this definition and no other" (Serres 1968a, 32). Moreover, he offered no ambiguity as to where this notion came from. This was exclusively a mathematical concept. In algebra, "it is devoid of mystery"; algebra was "the point where the content of the concept is the truest." Not that mathematicians had invented it, "only they were the first to endow it with *the precise, codified meaning* that *is the novelty of contemporary [structuralist] methods*" (ibid., 28–29).[25] Though he refrained from saying so explicitly, structures came, once again, from Bourbaki. Or rather from Leibniz, read as a structuralist, and a Bourbakist, *avant la lettre*.

From this structuralism — one of the most strictly modeled on Bourbaki — Serres soon diverged radically. Not that he came to acknowledge that the notion of structure, when used in philosophy, needed a more supple form; rather he realized that "the structure ... blew up" (Serres 1977, 110). He espoused the radical belief that regions of order were created from a turbulent sea of chaos, and this stabiliza-

[25] This definition of structure is repeated word for word in Serres 1968b, I, 4.

tion of order, of knowledge, became his major object of study. Serres renounced structuralism on the ground that "reality is not rational" (ibid., 10). In the process his style of writing slowly evolved, mirroring his philosophical path. If his first texts were dense, tightly articulated pieces that presented his arguments rationally and structurally, his later books were literary, almost poetic works of philosophy that appealed more to the senses and feelings than to logic. As a result, no other contemporary French philosopher, except perhaps Derrida, could be harder to summarize and paraphrase. I restrict myself here to a description of Serres's shifting relation with mathematics, and show how philosophy could, to use his own metaphor, discover the Northwest Passage between the two cultures (Serres 1980).

Serres's early education in mathematics made a crucial impression on him and shaped his philosophy. He always claimed to belong to no school of thought. According to him, three or four professional "superhighways" could then be taken: Marxism, phenomenology, the human and social sciences, and epistemology (which was moribund). None of them appealed to him; he became a "self-taught man," a surprisingly common claim among *normaliens*. Still, he was attentive to contemporary intellectual currents, especially Bourbakism, which had launched one of the "scientific revolutions" he said he had had the good luck to live through. In Bourbaki, Serres already saw a "structuralism — well defined in mathematics — which I sought to redefine in philosophy, long before it came into fashion in the humanities a good decade later" (Serres and Latour [1992] 1995, 10; Serres 1972, 70–71).

Aware of algebra and topology, Serres first worked, as a student, on the epistemology of Bourbaki's structures (Serres 1968b, 75 note). He then went back in time for his doctoral thesis and studied their origins in the work of Leibniz. In this book, sitting on the border of history and philosophy, Leibniz was the core of a revolution revealing a complex, multicentered universe. While Serres's structural analysis seemed directly taken from Bourbaki, he acknowledged Bourbaki more as a historian than as a mathematician (Bourbaki [1960] 1994). Serres based his understanding of Leibniz on contemporary mathematics, without sinking into naive teleology. Rather than seeing Leibniz as the precursor of modern mathematics, Serres used modern mathematical structures as a way to bring a new light to the work of Leibniz without divorcing it from its historical context. Thus could he exhibit a paradox: "Bourbaki's Leibniz is ultimately less of a Bourbakist than Leibniz himself" (Serres 1968b, 86). Serres used metaphors from modern graph theory (lattices and networks) to articulate the "multilinearity" and the "plurality of orders" that characterized both the system of Leibniz in Serres's view, and his own interpretation of Leibniz's work (Serres 1968b, 16; 1968a, 11–20).

Such parallels between Serres's philosophy and his methodology are a constant feature of his work. At this early stage, however, the parallel was only partial. Serres relied on structural mathematics to argue for a multiplicity of possible orders. He stated that the compartmentalization of the sciences was artificial; they "form a continuous body like an ocean" (Serres 1968b, 16).[26] Local orders met and accommodated one another, but none was hierarchically superior. Auguste Comte's ladder was knocked down. Serres replaced it not with a circle, like Piaget and Queneau, but with a network — or better, several networks that intersected in several dimensions, without foundation or center. Science was a multiplicity of orders.

But rejecting the idea of a single order in science, Serres neared a radical questioning of his own structural method. How could he still maintain that structuralism was a unique approach to the philosophy of knowledge? Chaos loomed. At the interstices between regions of order the networks had to be tied up with one another; some confusion was always possible. When, like in a thaw, his method broke down, Serres began to see pockets of order isolated in a sea of disorder. "Consequently, in science, there are only exceptions, rarities, and miracles. There are only islands of knowledge" (Serres 1977a, 11). Was the domain of science, including structuralism, limited to these few islands? Serres hesitated for while. Then he rebelled once again. Knowledge did not need to be restricted. The problem rested in the methods. "Structured" systems, like Bourbaki's, "of totalities without exterior, of perfect universal explanation or understanding, . . . are obsolete." The remedy was simple: "To come back to the things themselves, to mixed multiplicities, . . . not to restrain ourselves to linear sequences or . . . networks, but to treat them directly as large numbers, [or as] clouds" (ibid., 39–40). And there was hope that newly emerging sciences could help achieve this reversal of perspective.

Serres dropped the structures, dropped the networks, dropped Bourbaki. New metaphors took their place. More and more, Serres drew his inspiration from Ilya Prigogine's irreversible time and self-organizing disorder, from Thom's catastrophes, from Mandelbrot's fractals, and from the images of fluid mechanics, turbulence and chaos theory (see Serres 1977a, 79ff; Serres 1980, 40–113; Serres 1977b; and Serres [1982] 1995). In 1982 Serres's *Genesis* argued for the introduction of the concept of a "positive chaos" into philosophy. He now envisioned the world as a turbulent fractal, mixing foreseeable regions with chaotic regimes. Serres strongly rejected structures, which he now saw as the largest of the world's ordered systems. Summarizing the path he had covered, he wrote:

> Once we [philosophers] had order to conceive, new orders to construct. Then we thought through structures, with the sciences, but outside of them. . . . We conceived order under its broadest and most powerful category: a structure. . . . Thus new orders have appeared in unexpected places[;] the social sciences, literature, the history of religions, even philosophy, have been able to participate in *the algebraic festival of structure.* With it and outside it. . . . [Then] we found ourselves in the presence of multiplicity. . . .
>
> This pure multiple is the ground of order, but it is also, I think, its birth. (Serres [1982] 1995, 106)

[26] Serres's *thèse complémentaire,* published as Serres 1972, articulated this vision.

Then, he concluded: "Science is not necessarily a matter of one [unity], or of order, the multiple and noise are not necessarily the province of the irrational. This can be the case, but it is not always so" (ibid., 131). This marked the death of a Bourbakist's dream; this was also typical of a widespread rejection of abstract structures in French thought.

The Trouble with Bourbaki's Structures

Ironically, the structures of Bourbaki — which, as we have seen, had once been a paragon of scientific rigor among mathematicians, social scientists, philosophers, and writers — had actually turned out to be quite disappointing in this respect. Despite the emphasis he put on them, Bourbaki never formally defined structures in "The Architecture." Of course, he was aware of this, as he noted that the definition he provided was "not sufficiently general for the needs of mathematics" (Bourbaki [1948] 1971, 29, note 7). Understandable in an article written for a general audience, this omission was a glaring shortcoming for the entire edifice.

Bourbaki always intended to endow "structure" with a satisfactory formal meaning. Since he saw set theory, and structures especially, as the basis upon which mathematics should be built, the first booklet he published was his *Fascicule de résultats* (1939) on set theory. But, as he was the first to admit, this summary only presented definitions and propositions from a "naive" point of view, in direct opposition to the "formalist" approach that he had promised to follow in Book I. The first chapters of *Theory of Sets,* however, did not appear until 1954 — fifteen years after the first *fascicule.* The chapter dealing with structures, which was announced in the leaflet spelling out the "directions for the use of this treatise" accompanying each published booklet, appeared only in 1957. It was the twenty-second in the series!

Ironically, although the exact circumstances of the writing of this book have still to emerge, it seems that following this, "the old idea of 'fundamental structures' . . . disappeared from Bourbaki's vocabulary, . . . with however such a discreteness that few people seem to have noticed" (Malgrange 1975, 37). Indeed, during the 1960s the emphasis on structures vanished from the new version of the "directions." With the publication of the chapter on structure (1957), Bourbaki's enterprise was realigned. On the one hand, he was for the first time explicitly warning: "The treatise aims in no way at constituting an encyclopedia of present mathematical knowledge" (Bourbaki 1957, iii). On the other, he began dealing with topics outside of Part I, issuing books without a serial number (Lie groups and algebras, commutative algebras).

Almost immediately it was noticed that Book I differed markedly from the rest of the treatise. "The work of Bourbaki remains coherent after omission of this book," Michel Zisman wrote in 1956. "In some measure, it may even be considered as forming a whole distinct from the following books. Moreover, Book I is contested by some Bourbakists who are among the first who fail to understand what it brings to mathematics" (Zisman 1956, 49, note). There were three unsatisfactory aspects about *Theory of Sets*. First, in his introduction Bourbaki refined his vision of his axiomatic method: "the art of writing texts whose formalization is easy to be conceived of" (Bourbaki 1954, 2). Thus *Theory of Sets* was *not* a formal text; and Bourbaki often resorted to natural language, as he did throughout the treatise. Second, as Zisman had noted, the other books of the treatise, as a consequence, hardly seemed to require this purported foundation at all. Finally, other foundational approaches which had been developed since the publication of the *fascicule* in 1939 appeared difficult to integrate into Bourbaki's scheme, and perhaps even superseded his structural approach. In particular, the theory of categories elaborated by Saunders MacLane and Samuel Eilenberg after 1942 provided a suitable framework for describing general properties of objects studied by mathematicians — a framework that, following unsuccessful attempts, Bourbaki decided not to include within his own (Chouchan 1995, 33).

Although this formal shortcoming in Bourbaki's work was noticed early, it had little impact. In *Les Temps modernes*'s critique of structuralism, Pouillon noted that "even in Bourbaki, . . . the definition [of structure] remains largely implicit" (Pouillon 1966, 769; see also Barbut 1966, 799). But the criticism they addressed to structuralists in the human sciences was — discerningly — not about the problems with Bourbaki's structures. For Marc Barbut, who dealt with mathematical structuralism in the special issue, they were not really problematic. Mathematical structures were just so much poorer than the ones used in the human sciences that they were, more often than not, totally useless. In *Structuralism*, Jean Piaget saw category theory as the future direction of structuralism in mathematics, where the emphasis was shifting from objects to actions exerted on them. Needless to say, these developments did not undermine his vision but pointed to new syntheses to come (Piaget [1968] 1970, 27–28 and 143). Even the Oulipian logician Jacques Roubaud was appalled by *Theory of Sets*. He said he had once been part of a committee charged with conceiving a chapter on category theory for Bourbaki, which never materialized. "In any case, it would have been bad," says Roubaud. "Fortunately, May 68 came up and everybody became occupied with other things" (quoted in Chouchan 1995, 124). In this context, how are we to understand the widespread appeal of Bourbaki's definition of structures?

Recently, the historian Leo Corry has carefully examined the shortcomings of Bourbaki's structures, and provided a convincing scheme to account for his important impact. He clearly noted that "*Theory of Sets* was meant to provide a formally rigorous basis for the whole of the treatise. . . . The result, however, was different: *Theory of Sets* appears as an ad hoc piece of mathematics imposed upon Bourbaki by his own declared positions about mathematics, rather than a rich and fruitful source of ideas and mathematical tools" (Corry 1992, 320–21; see also Corry 1996). Corry's (1989) distinction between "body of knowledge" and "image of mathematics" goes some way to explain why Bourbaki was so powerful a symbol for practitioners of different disciplines. The above has shown how the "image" projected by Bourbaki's mathematics was appropriated and misappropriated by various groups.

Ultimately, I contend, what caused Bourbaki's image to recede in the 1970s was not the debate about whether structures were a sound basis for mathematics. By then, it had become irrelevant to many people, whether they were in favor of his program or not. Typically, even mathematicians who opposed it considered the issue rather marginal. If Bourbaki's laborious effort to found mathematics on this notion had proved something, it was rather that with it his irrelevance had finally become obvious. By being bogged down with foundational problems, he had abandoned the real world, a critique that recalls Serres's. Some French mathematicians would soon endeavor to retrieve the concrete world, and if rigor blocked their way to progress, then they would do away with it!

"Nice Visible Novelties"[27] *in Mathematics*

In November 1968, at the first *séminaire Bourbaki* following the events of May, the Oulipian Bourbakist Jacques Roubaud distributed a witty leaflet parodying Bourbaki's humor. It announced the death of the great mathematician (Chouchan 1995, 97). While this might have been premature, it signaled a new period in French mathematics. In fact, Bourbaki did not die, nor was he overthrown by a revolution. He was just too successful in making "commonplaces truly common," as Claude Chevalley wrote in 1951 in a rejected draft for *Theory of Sets*. He had fulfilled his ambition, and become less relevant.

Christian Houzel's *Prospective Report in Mathematics* (1985) offers a vivid contrast with Dieudonné's *Panorama of Pure Mathematics* published less than ten years earlier but already at odds with current research. Chapter headings included "Mechanics and Meteorology," "Applications to Biology," and "Applications to the Sciences of Man, the Sciences of Society and Linguistics," etc. Where Dieudonné devoted one laconic paragraph at the end of each chapter to applications, Houzel integrated them into the body of mathematics. "This effort to open mathematics to other sectors now seems to me essential for the survival of our science in France" (Houzel 1985, viii). What had changed? The simple answer is: so much that it is impossible to know where to begin. Many factors must be mobilized to account for this change in the general outlook of the discipline. Internal developments in both mathematics and science, availability of computers, renewed contacts with the Soviet Union, social demands for the application of mathematics, the popular image of science, the job market — all contributed. At least two

[27] "Du beau neuf visible," Serres, 1980, 99.

symptoms showing that the social place of mathematics was shifting can be gathered from the *Gazette des mathématiciens,* the professionnal journal of the Société mathématique de France (SMF). While Pierre Lelong complained that not enough Ph.D.s were conferred in 1961, an awareness that too few positions were available emerged in the mid-1970s (Lelong 1963; Malgrange 1975; Aubin and Cornet 1976; Berger 1980). A commission was then created in 1979 for "the defense and illustration of mathematics" at the SMF, "with the rather vague goal of popularizing mathematics and defending it against the threats we began to feel" (Ferrand 1980, 29; see also Nordon 1978; Lévy-Leblond and Jaubert 1975).

In particular, the view of mathematics as opening up to other sciences had been helped by the emergence of two avenues of research in the late 1960s and early 1970s: René Thom's catastrophe theory and Benoît Mandelbrot's fractal geometry. Catastrophe theory was an attempt to model discontinuous changes resulting from smooth variations of internal variables. Fractals were a geometrical description of extremely broken sets, like coastlines. Both, it was claimed, could describe natural phenomena when classical differential calculus — with which Bourbaki had started his enterprise and whose foundations he sought to secure once and for all — became useless. Both Thom and Mandelbrot strongly emphasized the need for a new mathematics tackling mundane reality.

> Many phenomena of common experience, in themselves trivial (often to the point that they escape attention altogether!) — for example, the cracks in an old wall, the shape of a cloud, the path of a falling leaf, or the froth on a pint of beer — are very difficult to formalize, but is it not possible that a mathematical theory launched for such homely phenomena might, in the end, be more profitable for science? (Thom [1972] 1975, 9)

Mandelbrot echoed this call. "Clouds are not spheres, mountains are not cones, coastlines are not circles and, more generally, man's oldest questions concerning the shape of this world were left unanswered by Euclid and his successors," among whom he surely counted Bourbaki (Mandelbrot 1987, 117). Although overtly opposed to him, Thom and Mandelbrot knew Bourbaki very well. Mandelbrot's uncle was among Bourbaki's founders; Thom was one of the first "guinea pigs," or potential members, "carefully selected by Cartan for their notable susceptibility to the *bourbachique* virus" (*La Tribu,* no. 8, 15 July 1945, 2).[28] But they chose to develop their mathematics in a different direction.

Distrustful of the assumption that they had only to harvest the mathematical tree planted by Bourbaki, structuralists often expressed their desire to enroll mathematicians in their pursuit of a "science of man." Lévi-Strauss, in particular, predicted that mathematics itself would benefit from dealing with social science issues. "One-way collaboration," he wrote in 1954, "is not enough. On the one hand, mathematics will help the advance of the social sciences, but on the other, the special requirements of those sciences will open new possibilities for mathematics" (Lévi-Strauss 1954, 590). This call was widely repeated but rarely heard by mathematicians. Their stubborn deafness was informed by Bourbaki's hegemony over their field. As early as 1956, it was recognized that "Bourbaki's inclination for the absolute can have unhappy consequences. This bent sometimes leads Bourbakists . . . to exclude all other aspects of mathematics" (Zisman 1956, 53). This was an "intellectual strategy," Mandelbrot wrote, but also an issue of "raw political power" (Mandelbrot 1989, 12; see also his recollections, Mandelbrot and Barcellos 1985; and Mandelbrot 1986).

[28] Thom attended Bourbaki's congresses at least twice: as a guinea pig in 1945 and as a visitor in 1953. *La Tribu,* no. 30, March 1953.

By not the least of ironies, it was Thom and Mandelbrot who answered Lévi-Strauss's call and, in order to oppose Bourbaki, explicitly drew resources from structural linguistics. The sources of fractal geometry can indeed be traced back to Mandelbrot's collaboration with Piaget's group, in the late fifties, on a project to use information theory in linguistics (Mandelbrot 1956–58). As for Thom, he asked in 1972 whether "structuralist developments in anthropological sciences (such as linguistics, ethnology, and so on) [could] have a bearing on the methodology of biology? I believe this is so," he answered (Thom 1972, 68). And much of his interpretation of the kind of knowledge produced by catastrophe theory depended on this answer. As I have shown elsewhere, Thom's morphogenetic theories could best be characterized as a *dynamics of structures* (see Aubin 1995; on catastrophe theory, see Woodcock and Davis 1978). Although their understanding of structuralism remained rather superficial, both Mandelbrot and Thom sought to translate, and indeed surpass, structural methods in mathematics.

It might not have been impossible to reframe both catastrophe theory and fractal geometry as pure mathematics, but in their inspiration, in the way they were brought to bear with the world and appealed to intuition, they were deeply involved with other fields of research. Besides structuralism, Thom drew his inspiration mainly from embryology (Aubin 1995), and Mandelbrot from economics and fluid mechanics. Mandelbrot crucially depended on the computer to conduct his research and let others share his powerful intuition with the help of striking graphics. As mathematicians, Thom and Mandelbrot claimed to revive forgotten traditions, especially Poincaré's qualitative work. To emphasize his own originality, Mandelbrot, in an obvious caricature, called Poincaré Bourbaki's "devil incarnate" (Mandelbrot 1989, 11). Like Poincaré, both Thom and Mandelbrot based their thinking on topology, rather than algebra; both showed disdain for mathematical rigor when it lagged behind intuition.

Their impact would subtly make itself felt in many ways. Among the descendants of fractals and catastrophes was chaos theory.[29] In 1969 the physicist David Ruelle, one of Thom's colleagues, wrote, in a book on statistical mechanics, that a

[29] The historical path from catastrophe to chaos has to be appreciated with nuance (Aubin 1998). Although theoretically speaking not a direct ancester of chaos, catastrophe theory was nonetheless crucial in attracting attention on the promises offered by topological approaches to the study of nature. Fractals were later found quite useful to study and describe strange attractors.

Bourbakist treatment of many fields of physics was a "rewarding experience" (Ruelle 1969, viii). At that time, in contact with Thom's theory, he had already started thinking about turbulence. In 1971 he and Floris Takens published an article that introduced the notion of "strange attractors," and in many ways initiated chaos theory — a scientific theory that explicitly emphasized limits to prediction. The fruitful intercourse between mathematics and physics, between mathematics and the world, had been resumed. Mandelbrot acknowledged the "perfect timing" of his books. "They came out when the feeling was beginning to spread that the Bourbaki *Foundations* treatise, like a Romantic prince's dream castle, was never to be completed. . . . The Constitution phrase to ensure that the group would remain eternally a cohesive young rebel was of course not working. In a way, the whole enterprise had become boring" (Mandelbrot and Barcellos 1985, 221). Bourbaki was getting old.

Catastrophes and Fractals as Cultural Connectors

I have illustrated post-Bourbakist mathematics with catastrophe theory and fractal geometry not because they were alone — as we have seen with probability theory and applied mathematics — but because they acted most visibly as cultural connectors in 1970s France.[30] Admittedly, from the mathematicians' point of view, catastrophes and fractals may often have been perceived more as media fads than research avenues — a view boosted by the special character of Thom and Mandelbrot's books.[31] Indeed, perhaps in order to get around Bourbaki's hegemony, they published manifestos that reached beyond mathematicians and scientists (Mandelbrot [1975] 1977; Thom [1972] 1975; Thom [1974] 1983). This would help to ensure their wide cultural diffusion.

A channel for communication between mathematics and other cultural spheres having been established through Bourbaki, people could naturally mobilize, in a similar way, mathematics critical of Bourbaki to undermine structuralism. In no other work than Michel Serres's and Jean-François Lyotard's is it clearer how Thom's catastrophes and Mandelbrot's fractals could supplant Bourbaki's structures as cultural connectors.

> Like an iceberg inverting itself, mathematics globally veered to formalism at the beginning of the century. It forsook intuition. It forgot intuition. It even, sometimes, condemned intuition. . . . Physicists or philosophers, sociologists or biologists, we all were formalists. . . . [But] here is intuition again. Here is space again. . . . Catastrophes *à la* Thom or fractals *à la* Mandelbrot. For five to ten years, again, it's been a party. (Serres 1980, 99)

Similarly, in *The Postmodern Condition*, Lyotard played Thom and Mandelbrot against Bourbaki as a way to fault modernist structuralism. For him, the legitimacy of Bourbaki's knowledge hinged on the acceptance of statements (the axioms) to which it was subordinated ([1979] 1984, 43). This act of faith was based on power. For Lyotard, Thom's and Mandelbrot's work crucially informed new paths taken by knowledge. They were symptomatic of a "postmodern science [that] — by concerning itself with such things as undecidables, the limits of precise control, conflicts characterized by incomplete information, *'fracta,'* catastrophes, and pragmatic paradoxes — is theorizing its own evolution as discontinuous, catastrophic, nonrectifiable, and paradoxical" (ibid., p. 60; see also Carson 1995).

The connection was not accidental. The bylaws of the Institut des hautes études scientifiques, where René Thom worked, dictated that it devote some of its activities to the "methodology of the sciences of man" (Aubin 1998). Always moribund compared to the other sections of the IHES in mathematics and physics, this section was never closer to realization than during the late 1970s. Thom was nominated as a professor for this section and got support to explore applications of catastrophe theory. He educated a group of philosophers and searchers in social science (Scheurer, Pomian, Petitot-Cocorda, Boutot, among others). By then, Bourbaki was more or less evacuated from the discourses of social science, and structuralism from mathematics. Cultural connectors, like catastrophes and fractals, remained, but would quickly wither. They had benefited from the connection established through Bourbaki. But they could not sustain it. Connections would have to follow other channels.

Conclusion

How to account for changes in the outlook of mathematics remains a troublesome question for historians. It is not enough to exhibit striking resonances that may have existed between scientific and cultural movements. We must locate them in history and find mechanisms able to account for them. Several strategies can be deployed. We may argue for a causal link from one to the other, for a common source, or for an extensive dialogue between them. None of these, I have shown, can well account for the subtleties of the cultural dynamics of postwar France. A dialogue between structuralism and Bourbaki's mathematics indeed took place. But on the whole, it was forced on them, unsustained, and ultimately rather superficial, even when taking into account Piaget's serious efforts. The above however suggests that even a failed discussion can have effects on the fields themselves, as well as on the outside — as the Oulipo experience demonstrates.

In order to describe this cultural dynamic, I have introduced a notion of cultural

[30] Other important cultural connectors of the late 1970s and early 1980s were cybernetics and systems theory (Rosnay 1975; Morin 1977), self-organization inspired by Prigogine (Dumouchel and Dupuy 1983), and later, deterministic chaos.

[31] This however is rather difficult to evaluate and of little consequence for my argument. See Malgrange 1975, 36; and CNRS 1982, 93 for evaluations of the impact of catastrophe theory on mathematics.

connection rooted in the actors' practice and leaving them a lot of autonomy. When they used Bourbaki as a cultural connector, they had much leeway in interpreting its meaning in their own field. Still, the connection they thus established helped strengthen the successes of their respective approaches in each discipline. The connection emerged from the constant, self-reinforcing call to the cultural connector, rather than from common causes. But this act of connection was not without effect. It exposed their interpretations to similar counterarguments. Once the connection was established, it became easier to replace the connector by a new one that would serve to undermine previously received ideas in both fields.

In this view, the postmodernist turn represents a change in the cultural connectors deployed, but not in the way they were used. Much more radical challenges were posed to science in the years after May 1968, in France and elsewhere. Both Serres and Lyotard, to name just two, strongly argued in ethical and moral terms. It may have been an understandable — and perhaps wise — strategy for a generation of middle-aged Frenchmen, at home or in exile, sidestepped by the events of World War II, to isolate itself in the pursuit of pure knowledge and distance itself from forceful attempts to control nature and society. But for the generation after May 1968 it seemed that totalizing science and philosophy entailed a disposition to totalitarianism. By acknowledging the limits of knowledge, and by grounding it in the contemporary world, they wished to construct ethical islands of truth that would speak to the mundane reality of existence. Whether mathematicians also sought to "wage a war on totality" remains to be seen (Lyotard [1979] 1984, 82). Will a detailed study of the cases of Thom, Mandelbrot, Prigogine, and French chaologists prove that a kinder science could indeed be achieved? In view of the controversy that pitted Thom against Prigogine in the early 1980s (Pomian 1990), answers will hardly be univocal.

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5

References

Andler, Martin. 1994. "Les mathématiques à l'Ecole normale supérieure au XXe siècle: une esquisse." In *Ecole normale supéorieure: Le livre du bicentenaire*, edited by Jean-François Sirinelli, 351–404. Paris: Presses universitaires de France.

L'Arc. 1965. No. 26, special issue "Lévi-Strauss," March.

Arnol'd, Vladimir I. [1981] 1992. *Catastrophe Theory: Third, Revised, and Expanded Edition*. Translated by G. S. Wassermann, based on translation by R. K. Thomas. Berlin: Springer.

—. 1995. "Will Mathematics Survive?: A Report from the Zurich Congress." *Mathematical Intelligencer* 17(3):6–10.

Arvonny, Maurice. 1980. "Quarante ans de Bourbaki: Le célèbre mathématicien est toujours immortel, mais il a vieilli." *Le Monde*, 9 April, 15.

Aubin, David. 1995. "The Catastrophe Theory of René Thom: Topology, Embryology, and Structuralism." In Wise, in preparation.

—. 1998. A Cultural History of Catastrophes and Chaos: Around the Institut des Hautes Études Scientifiques, Ph.D. thesis. Princeton University.

Aubin, Jean-Pierre, and B. Cornet. 1976. "Rapport sur la situation de la recherche mathématique en France." *Gazette des mathématiciens* 7:18–73.

Auzias, Jean-Marie. 1967. *Clefs pour le structuralisme*. Paris: Seghers.

Auzias, Jean-Marie, et al. 1970. *Structuralisme et marxisme*. Paris: Union générale d'éditions.

Bachelard, Gaston. 1971. *Epistémologie: textes choisis*. Edited by Dominique Lecourt. Paris: Presses universitaires de France.

Barbosa de Almeida, Mauro W. 1990. "Symmetry and Entropy: Mathematical Metaphors in the Work of Lévi-Strauss." *Current Anthropology* 31:367–85.

Barbut, Marc. 1966. "Sur le sens du mot structure en mathématiques." *Les Temps modernes* 22 (246), special issue on "Problèmes du structuralisme": 791–814.

Bastide, Roger, ed. 1962. *Sens et usages du terme structure dans les sciences humaines et sociales*, Janua linguarum 16. The Hague: Mouton.

Beaulieu, Liliane. 1989. *Bourbaki: Une histoire du groupe de mathématiciens français et de ses travaux (1934–1944)*. Ph.D. thesis, Université de Montréal.

—. 1993. "A Parisian Café and Ten Proto-Bourbaki Meetings (1934–1935)." *Mathematical Intelligencer* 15(1): 27–35.

—. 1994. "Dispelling a Myth: Questions and Answers about Bourbaki's Early Work, 1934–1944." In *The Intersection of History and Mathematics*, edited by Sasaki Chikara, Sugiura Mitsuo, and Joseph W. Dauben, 241–52. Basel: Birkhäuser.

—. Forthcoming a. *Bourbaki: History and Legend*. Berlin: Springer.

—. Forthcoming b. "Jeux d'esprit et jeux de mémoire chez N. Bourbaki."

Beauvoir, Simone de. 1949. Review of Lévi-Strauss 1949. *Temps modernes* 5 (49):943–49.

Benoist, Jean-Marie. [1975] 1978. *The Structural Revolution.* London: Weidenfeld and Nicholson.
Bens, Jacques. 1980. *OuLiPo, 1960–1963.* Paris: Christian Bourgois.
Benveniste, Emile. 1962. "'Structure' en linguistique." In Bastide 1962, 31–39.
Berger, Marcel. 1980. "Note sur le flux de survie de la recherche mathématique en France." *Gazette des mathématiciens,* no. 13: 5–13.
Biagioli, Mario. 1993. *Galileo, Courtier: The Practice of Science in the Culture of Absolutism.* Chicago: The University of Chicago Press.
Bourbaki, N. 1939. "Mode d'emploi de ce traité," in *Théorie des ensembles (Fascicule de résultats). Eléments de mathématique,* part 1, book 1. Paris: Hermann.
——. [1948] 1971. "The Architecture of Mathematics." In Le Lionnais [1948, 1962], 1971 1: 23–36.
——. 1949. "Foundations of Mathematics for the Working Mathematician." *Journal of Symbolic Logic* 14:1–8.
——. 1950. "The Architecture of Mathematics." *The American Mathematical Monthly* 57:22–232.
——. 1954–57. *Théorie des ensembles.* Chap. 1: "Description des mathématiques formelles" (1954); chap. 2: "Théorie des ensembles" (1954); chap. 3: "Ensembles ordonnés, cardinaux, nombres entiers" (1956); chap. 4: "Structures" (1957). Paris: Hermann.
——. [1960] 1994. *Elements of the History of Mathematics.* Translated by John Meldrum. Berlin: Springer.
——. 1968. *Theory of Sets.* Paris: Hermann, and Reading: Addison-Wesley.
Carson, Cathryn. 1995. "Who Wants a Postmodern Physics?" *Science in Context* 8(4):635–55.
Cartan, Henri. 1980. "Nicolas Bourbaki and Contemporary Mathematics." *The Mathematical Intelligencer* 2:175–80.
Charbonnier, Georges. 1962. *Entretiens avec Raymond Queneau.* Paris: Gallimard.
Chevalley, Claude. 1935. "Variations du style mathématique." *Revue de mdtaphysique et de morale,* no. 3: 375–84.
Chevalley, Claude, and Arnaud Dandieu. 1932. "Logique hilbertienne et psychologie." *Revue philosophique de France et de l'étranger* 57:399–411.
Choquet, Gustave. 1996. "Témoignage sur Paul André Meyer." *Gazette des mathématiciens,* no. 68, 13–14.
Chouchan, Michèle. 1995. *Nicolas Bourbaki: Faits et légendes.* Paris: Editions du Choix.
CNRS. 1982. "Rapport de conjoncture 1981." *Gazette des mathématiciens* 20:16–110.
Corry, Leo. 1989. "Linearity and Reflexivity in the Growth of Mathematical Knowledge." *Science in Context* 3:409–40.

——. 1992. "Nicolas Bourbaki and the Concept of Mathematical Structure." *Synthese* 92:315–48.
——. 1996. *Modern Algebra and the Rise of Mathematical Structure.* Basel: Birkhäuser.
Corvez, Maurice. 1969. *Les Structuralistes, les linguistes . . . Michel Foucault, Claude Lévi-Strauss, Jacques Lacan, Louis Althusser, les critiques littéraires.* Paris: Aubier-Montaigne.
Courrège, Philippe. [1965] 1971. "Un modèle mathématique des structures élémentaires de parenté." *L'Homme* 5 (3–4):248–90. Reprinted in *Anthropologie et calcul,* edited by Philippe Richard and Robert Jaulin, 126–81. Paris: Union générale d'éditions, coll. "10/18."
Dahan Dalmedico, Amy. 1995. "Polytechnique et l'Ecole française de mathématiques appliquées." *La France des X, deux siècles d'histoire,* edited by Bruno Belhoste, et al. Paris: Economica.
——. 1996. "L'essor des mathématiques appliquées aux Etats-Unis: l'impact de la seconde guerre mondiale." *Revue d'histoire des mathématiques* 2:149–213.
——. 1997. "Les mathématiques au XXème siècle." *Science in the Twentieth Century.* Edited by John Krige and Dominique Pestre. London: Harwood.
Daston, Lorraine. 1988. *Classical Probability in the Age of the Enlightenment.* Princeton: Princeton University Press.
Desanti, Jean. 1965. "Remarques sur la connection des notions de genèse et structure en mathématiques." In Gandillac et al. 1965, 143–59.
Descombes, Vincent. [1979] 1980. *Modern French Philosophy.* Translated by L. Scott-Fox and J. M. Harding. Cambridge: Cambridge University Press.
Dieudonné, Jean. 1977. *Panorama des mathématiques pures. Le choix bourbachique.* Paris: Gauthier-Villars.
Dosse, François. 1991–92. *Histoire du structuralisme,* 2 vols. Paris: La Découverte.
Ducrot, Oswald, Tzvetan Todorov, Dan Sperber, Moustafa Safouan, and François Wahl. 1968. *Qu'est-ce que le structuralisme?* Paris: Seuil.
Dumouchel, Paul, and Jean-Pierre Dupuy, eds. 1983. *L'Auto-Organisation: De la physique au politique.* Paris: Seuil.
L'Esprit. 1963. Vol. 31 (322), special issue, "*La Pensée sauvage* et le structuralisme": 545–653.
L'Esprit. 1967. Vol. 35 (360), special issue, "Structuralismes, idéologie et méthode": 769–901.
Fages, J.-B. 1967. *Comprendre le structuralisme.* Toulouse: Privat.
Fang, Jon. 1970. *Bourbaki: Towards a Philosophy of Mathematics I.* Hauppauge, N.Y.: Paideia Press.
Ferrand, Jacqueline. 1980. "Rapport d'activité." *Gazette des mathématiciens,* no. 14: 29.
Foucault, Michel. [1969] 1994. "Qu'est-ce qu'un auteur?" *Bulletin de la société française de philosophie* 63(3) (1969):73–104. Reprinted in *Dits et écrits, 1954–1988,* vol. 1, edited by Daniel Defert and François Ewald, 789–821. Paris: Gallimard.

Galison, Peter. 1994. "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision." *Critical Inquiry* 21:228–66.
Gandillac, Maurice de, Lucien Goldmann, and Jean Piaget, eds. 1965. *Entretiens sur les notions de genèse et de structure,* Colloque de Cerisy-la-Salle, juillet-août 1959. Paris and The Hague: Mouton.
Gardner, Howard. 1981. *The Quest for the Mind: Piaget, Lévi-Strauss, and the Structuralist Movement,* 2nd ed. Chicago: The University of Chicago Press.
Gayon, Jean. 1989. "Génétique, psychologie génétique et épistémologie génétique dans l'oeuvre de Jean Piaget (1896–1980): une ambiguïté remarquable." In *L'Ordre des caractères. Aspects de l'hérédité dans l'histoire des sciences de l'homme,* edited by Claude Bénichou, 147–73. Paris: Vrin.
Goldmann, Lucien. 1962. "Introduction générale." In Bastide 1962, 7–22.
——. 1965. "Le concept de structure significative en histoire de la culture." In Gandillac et al. 1965, 124–35.
Grothendieck, Alexandre. 1985. *Récoltes et semailles: Réflexions et témoignage sur un passé de mathématicien,* 7 vols. Montpellier: Université du Languedoc and CNRS. Manuscript, Fine Library, Princeton University.
Halmos, Paul R. 1957. "Nicolas Bourbaki." *Scientific American,* 196(May): 88–97.
Harland, Richard. 1987. *Superstructuralism: The Philosophy of Structuralism and Post-Structuralism.* London: Methuen.
Hilbert, David. [1900] 1902. "Mathematical Problems." *Bulletin of the American Mathematical Society* 8:437–79, authorized translation by Mary Winston Newson.
Holenstein, Elmar. [1974] 1976. *Roman Jakobson's Approach to Language: Phenomenological Structuralism.* Translated by Catherine and Tarcisius Schelbert. Bloomington: Indiana University Press.
Houzel, Christian. 1979. "Les mathématiciens retournent au concret." *La Recherche* 10:508–9.
——, ed. 1985. *Rapport de prospective en mathématiques.* Paris: Editions du CNRS.
Jakobson, Roman. [1961] 1971. "Retrospect (1961)." In *Selected Writings I.* The Hague: Mouton.
——. [1976] 1978. *Six Lectures on Sound and Meaning.* Translated by John Mepham. Hassocks, UK: Harvester Press.
Lax, Peter D. 1989. "The Flowering of Applied Mathematics in America." *A Century of Mathematics in America,* Part II, edited by Peter Duren with Richard A. Askey and Uta C. Merzbach, 455–66. Providence: American Mathematical Society.
Le Lionnais, François, ed. [1948, 1962] 1971. *Great Currents of Mathematical Thought.* Translated by R. A. Hall and Howard G. Bergmann, 2 vols. New York: Dover.
——. 1962. Review of Queneau 1962. *Dossiers du Collège de Pataphysique,* nos. 18/19, 7 Clinamen 89 EP [vulg. 29 mars 1962]: 68.

——. 1973. " Quelques structures et notions mathématiques." In Oulipo 1973, 295–98.
——. 1975. "Queneau et les mathématiques." *Raymond Queneau,* edited by Andrée Bergens. Paris: l'Herne.
Lelong, Pierre. 1963. "Questions d'actualité et de prospective." *Gazette des mathématiciens* 2(3):1–3.
Lévi-Strauss, Claude. 1949. *Les Structures élémentaires de la parenté.* Paris: Presses universitaires de France.
——. [1949] 1969. *The Elementary Structures of Kinship.* Translated by James Harle Bell, edited by John Richard von Sturmer and Rodney Needham. Boston: Beacon.
——. [1952] 1958. "Social Structure." *Anthropology To-Day: Wener-Gren Foundation International Symposium on Anthropology,* edited by A. L. Kroeber. New York: Columbia University Press. Reproduced in *Anthropologie structurale,* 303–51. Paris: Plon. See Lévi-Strauss [1958] 1963.
——. 1954. "The Mathematics of Man." *International Social Science Bulletin* 6 (4):581–90.
——. [1958] 1963. *Structural Anthropology.* Translated by Brooke G. Schoepf. New York: Basic.
——. [1983] 1985. *The View from Afar.* Translated by Joachim Neugroschel and Phobe Hoss. New York: Basic.
Lévi-Strauss, Claude, and Didier Eribon. [1988] 1991. *Conversations with Claude Lévi-Strauss.* Translated by Paula Wissing. Chicago: The University of Chicago Press.
Lévy-Leblond, Jean-Marc. 1968. "Physique et mathématique." *Encyclopaedia universalis* 13:4–8.
Lévy-Leblond, Jean-Marc, and Alain Jaubert, eds. 1975. *(Auto)critique de la science.* Paris: Seuil, coll. "Points/Sciences."
Lyotard, Jean-François. [1979] 1984. *The Postmodern Condition.* Translated by G. Bennington and B. Massumi. Minneapolis: University of Minnesota Press.
Malgrange, Bernard. 1975. "Quelques mots sur les dernières sessions du Comité consultatif," and "A propos d'un article de J. Dieudonné." *Gazette des mathématiciens* 3:32–37.
Mandelbrot, Benoît. 1956. "Sur la définition abstraite de quelques degrés d'équilibre." *Etudes d'épistémologie génétique* 2:1–26.
——. 1957. "Linguistique statistique macroscopique." *Etudes d'épistémologie génétique* 3:1–78.
——. 1958. "Quelques problèmes de la théorie de l'observation dans le contexte des théories modernes de l'induction des statisticiens." *Etudes d'épistémologie génétique* 5:29–47.
——. [1975] 1977. *Fractals: Form, Chance, and Dimension.* San Francisco: Freeman.
——. 1986. "Comment j'ai découvert les fractales." *La Recherche* 17:420–24.

—. 1987. "Towards a Second Stage of Indeterminism in Science." *Interdisciplinary Science Review* 12:117–27.
—. 1989. "Chaos, Bourbaki, and Poincaré." *Mathematical Intelligencer* 11(3):10–12.
Mandelbrot, Benoît, and Anthony Barcellos. 1985. In *Mathematical People: Profiles and Interviews,* edited by Donald J. Albers and G. L. Alexanderson, 207–25. Boston: Birkhäuser.
Marchal, André. 1962. "L'attitude structuraliste et le concept de structure en économie politique." In Bastide 1962, 63–67.
Merquior, J. G. 1986. *From Prague to Paris: A Critique of Structuralist and Post-Structuralist Thought.* London: Verso.
Morin, Edgar. 1977. *La Méthode. 1: La nature de la nature.* Paris: Seuil.
Motte, Warren F., ed. 1986. *Oulipo: A Primer of Potential Literature.* Lincoln, Nebraska: University of Nebraska Press.
Nordon, Didier. 1978. "Sommes-nous tous pareils?" *Gazette des mathématiciens* 10:65–89.
Oulipo. 1973. *La Littérature potentielle (créations, re-créations, récréations).* Paris: Gallimard.
Parain-Vial, Jeanne. 1969. *Analyses structurales et idéologies structuralistes.* Toulouse: Privat.
Pavel, Thomas G. 1989. *The Feud of Language: A History of Structuralist Thought.* Translated by L. Jordan and T. G. Pavel. Oxford: Basil Blackwell.
Piaget, Jean. 1949. *Traité de logique: Essai de logistique opératoire.* Paris: Armand Colin.
—. 1950. *Introduction à l'épistémologie génétique,* 3 vols. Paris: Presses universitaires de France.
—. 1955. "Les structures mathématiques et les structures opératoires de l'intelligence." In *L'Enseignement des mathématiques,* 11–33. Neuchatel: Dulachaux & Niestlé.
—. 1965. "Genèse et structure en psychologie." In Gandillac et al. 1965, 37–61.
—. ed. 1967. *Logique et connaissance humaine.* Paris: Gallimard.
—. [1968] 1970. *Structuralism.* Translated by Chaninah Maschler. New York: Basic.
—. 1970. *Genetic Epistemology.* Translated by Eleanor Duckworth. New York: Columbia University Press.
Pomian, Krzysztof, ed. 1990. *La Querelle du déterminisme: Philosophie de la science d'aujourd'hui.* Paris: Gallimard.
Pouillon, Jean. 1966. "Présentation: un essai de définition." *Les Temps modernes* 22 (246):769–90.
Queneau, Raymond. [1948] 1971. "The Place of Mathematics in the Classification of the Sciences." In Le Lionnais [1948] 1971 2:73–77.
—. 1961. *Cent mille milliards de poèmes.* Paris: Gallimard.
—. 1962. "Bourbaki et les mathématiques de demain." *Critique* 18 (176):3–18. Repr. *Bords.* Paris: Hermann, 1963.

—. 1967. "Science and Literature." *Times Literary Supplement,* 28 Sept.: 863–64.
—. 1968. "Théorie des nombres: sur les suites *s*-additives." *Comptes-rendus de l'Académie des sciences de Paris A* 266:957–58.
—. 1972. "Sur les suites *s*-additives." *Journal of Combinatorial Theory* 13:31–71.
Régnier, André. 1971. "De la théorie des groupes à la pensée sauvage." *Anthropologie et calcul,* edited by P. Richard and R. Jaulin, 271–98. Paris: Union générale d'éditions.
—. 1974. *La Crise du langage scientifique.* Paris: Anthropos.
Reid, Constance. 1986. *Hilbert — Courant.* New York: Springer.
Rosenstiehl, Pierre. 1996. "La mathématique et l'École." *Une École pour les Sciences Sociales: De la VIe Section à l'École des hautes études en sciences sociales,* edited by Jacques Revel of Nathan Wachtel, 167–84. Paris: Éditions du Cerf and Éditions de l'EHESS.
Rosnay, Joël de. 1975. *Le Macroscope: Vers une vision globale.* Paris: Seuil.
Roubaud, Jacques. 1989. *'Le grand incendie de Londres.'* Paris: Seuil.
—. 1997. *Mathématique.* Paris: Seuil.
Roudinesco, Elisabeth. 1993. *Jacques Lacan: Esquisse d'une vie, histoire d'un système de pensée.* Paris: Fayard.
Ruelle, David. 1969. *Statistical Physics: Rigorous Results.* New York: W. A. Benjamin.
—. 1991. *Chance and Chaos.* Translated by David Ruelle. Princeton, N.J.: Princeton University Press.
Ruelle, David, and Floris Takens. 1971. "On the Nature of Turbulence." *Communications in Mathematical Physics* 20:167–92.
Saint-Sernin, Bertrand. 1995. *La Raison au XXe siècle.* Paris: Seuil.
Schaffer, Simon. 1995. "Accurate Measurement Is an English Science." In *The Values of Precision,* edited by M. Norton Wise, 135–72. Princeton, N.J.: Princeton University Press.
Schwartz, Laurent. 1997. *Un mathématicien aux prises avec le siècle.* Paris: Odile Jacob.
Serres, Michel. 1968a. *Hermès I: La communication.* Paris: Minuit.
—. 1968b. *Le Système de Leibniz et ses modèles mathématiques,* 2 vols. Paris: Presses universitaires de France.
—. 1972. *Hermès II: L'interférence.* Paris: Minuit.
—. 1977a. *Hermès IV: La distribution.* Paris: Minuit.
—. 1977b. *La Naissance de la physique dans le texte de Lucrèce: Fleuves et turbulences.* Paris: Minuit.
—. 1980. *Hermès V: Le passage du Nord-Ouest.* Paris: Minuit.
—. [1982] 1995. *Genesis.* Translated by Geneviève James and James Nielson. Ann Arbor: University of Michigan.
Serres, Michel, and Bruno Latour. [1992] 1995. *Conversations on Science, Culture, and Time.* Translated by Roxanne Lapidus. Ann Arbor: University of Michigan.