CHAPTER II: STRUCTURES

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.[1]

La mathématique est l'art de donner le même nom à des choses différentes.
—Henri Poincaré.[2]

1. INTRODUCTION[3]

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.[4] By looking at the various roles he played in several types of

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[1] R. Queneau, "Bourbaki et les mathématiques de demain," *Critique,* 18, no. 176 (1962): 3-18; repr. in *Bords* (Paris: Hermann, 1963).
[2] "Mathematics is the art of giving the same name to different things." H. Poincaré, "L'avenir des mathématiques," *Atti del IV Congresso internzionale dei matematici, Roma, 1908,* 1 (Rome: Accademia dei Lincei, 1909): 167-182, 171.
[3] A version of this chapter is to be published in the summer issue of *Science in Context* (1997).
[4] 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 voice. But, since this chapter deals with the actual effects of this mythic image rather than the "true" history behind it, this odd usage seemed more appropriate. For Bourbaki's myth, see for example P. R. Halmos, "Nicolas Bourbaki," in *Scientific American,* 196 (May 1957): 88-97; for its functions with respect to Bourbaki's image, see below.

discourse, his rising impact among a portion of 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 Éléments de mathématique.*[5] 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.[6]

But, while the mythic Bourbaki could not grow old, the myth of Bourbaki has. To reconstitute the "thought" of a group of people probably is a futile affair. This hardly

[5] Histories of Bourbaki are to be found in L. Beaulieu, "A Parisian Café and Ten Proto-Bourbaki Meetings (1934-1935)," *Mathematical Intelligencer,* 15(1) (1993): 27-35; L. Beaulieu, "Dispelling a Myth: Questions and Answers about Bourbaki's Early Work, 1934-1944," *The Intersection of History and Mathematics,* ed. S. Chikara, S. Mitsuo, and J. W. Dauben (Basel: Birkäuser, 1994): 241-252; H. Cartan, "Nicolas Bourbaki and Contemporary Mathematics," *The Mathematical Intelligencer,* 1(2) (1980): 175-180; M. Chouchan, *Nicolas Bourbaki. Faits et légendes* (Paris: Éditions du Choix, 1995); J. Fang, *Bourbaki: Towards a Philosophy of Mathematics I.* (Hauppauge, New York: Paideia Press, 1970). The most comprehensive history is, however, still unpublished: L. Beaulieu, *Bourbaki. Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944),* Ph.D. thesis (université de Montréal, 1989); *Bourbaki: History and Legend* (Berlin: Springer, forthcoming).

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 for 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 from 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 to nature, rather than 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 below, had already begun to face serious challenges as early as the 1950s.[7] 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[8], mathematicians managed to dispense with it and still produce significant results enthusiastically embraced by their

[6] The historian Liliane Beaulieu, who has worked the most extensively on Bourbaki, told me that she never came accross any written trace of this rule and that, in any case, it was breached many times.
[7] See L. Corry, *Modern Algebra and the Rise of Mathematical Structure* (Basel: Birkhäuser, 1996).
[8] See, *e.g.*, V. I. Arnol'd, *Catastrophe Theory: Third, Revised, and Expanded Edition,* transl. G. S. Wassermann, based on transl. by R. K. Thomas (Berlin, Heidelberg: Springer, [1981] 1992).

community. In addition, these recent theories were developed, not solely out of motives 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 in 1979 the mathematics of the past decade, Christian Houzel, a president of the *Société mathématique de France* in 1982, 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 rather characterized 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."[9]

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. In order to present a compelling argument, it is however 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

[9] C. Houzel, "Les mathématiciens retournent au concret." *La Recherche,* 10 (1979), 508-509; all translations are mine, except when I quote from a published translation. See also C. Houzel, ed., *Rapport de prospective en mathématiques* (Paris: Éditions du CNRS, 1985); M. Arvonny, "Quarante ans de Bourbaki. Le célèbre mathématicien est toujours immortel, mais il a vieilli," *Le Monde* (9 April, 1980): 15; and D. Nordon, *Les Mathématiques pures n'existent pas!* (Paris: Actes Sud, 1981).

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 convincingly such connections 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.[10]

Paralleling the trajectory of structuralism, Bourbaki's rise and decline in postwar France provides, I believe, a perfect case for 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. A reconciliation between the culture of Bourbakian mathematics and larger currents, this seems to suggest, may therefore be possible. I mean to achieve this by looking at Bourbaki as a *cultural connector.* Recall that I defined cultural connectors in Chapter I, as more or less explicit references from other disciplines, used by actors when they attempt to argue for a point or when they want to increase the legitimacy of their methods and ideas.

Bourbaki 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

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[10] See, for example, M. Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993); L. Daston, *Classical Probability in the Age of the Enlightenment* (Princeton: Princeton University Press, 1988); P. Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision." *Critical Inquiry,* 21 (1994): 228-266; S. Schaffer, "Accurate Measurement an English Science," in *The Values of Precision,* ed. M. N. Wise (Princeton: Princeton University Press, 1995): 135-172; C. Smith and M. N. Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989); M. N. Wise, "Work and Waste: Political Economy and Natural Philosophy in Nineteenth-

to serve as a shortcut indicating a certain attitude towards 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 affair came to be, how it varied and evolved, and within which limits, is the topic of this paper. In the following, 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.[11] With this chapter, I hope to provide a firm historical basis for placing the evolution of mathematics, and of its image, into a larger cultural context. To doing so, I will both study the culture of mathematicians in postwar France and put culture back into the cultural history of mathematics.

2. ORIGINS

a) 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

Century Britain (1-3)." *History of Science,* 27 (1989): 263-301 and 391-449; *Ibid.,* 28 (1990): 221-261.

use whatsoever for mathematics. Although existentialism nearly monopolized intellectual debates in the immediate postwar, foundations were meanwhile being laid down for the next generation. Among the important works that came out in 1948/49—besides Braudel's *Méditerranée* and de Beauvoir's *Second Sex*—there 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,* is quite well known.[12] 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 next decades.[13]

The other work that I want to bring to our attention was 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."[14] Dating from 1939, the idea for this collection was impeded by the problems of wartime communication, which intensified its French focus, and further delayed by the internment in 1944 of Le Lionnais in a German camp. Although not as famous as Lévi-Strauss's

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[11] In addition, Bourbaki's name has often been invoked by reformers of mathematical education, and their adversaries, both in France and in the U.S., an issue that I scarcely address here, but that is part of the story of the cultural connector Bourbaki.
[12] C. Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris: Presses universitaires de France, 1949); *The Elementary Structures of Kinship,* transl. J. H. Bell, ed. J. R. von Sturmer and R. Needham (Boston: Beacon, 1969).
[13] Quoted from the chronology established by A. Simonin and H. Clastres [*Les Idées en France, 1945-1988. Une chronologie* (Paris: Gallimard, 1989), 79], which has been very useful for this chapter.
[14] See the reprinted volume: F. Le Lionnais, ed., *Les Grands Courants de la pensée mathématique*, new augmented edition (Paris: Albert Blanchard, 1962); *Great Currents of*

work, it included a seminal programmatic statement by Bourbaki.[15] 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 as diligently followed by mathematicians as structuralism was 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 Lévi-Strauss's.

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, one of Bourbaki's founders and foremost collaborators.[16] 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, author of *Zazie dans le métro*.[17] In 1960, Queneau and Le Lionnais cofounded an influential literary group, the *Oulipo* (Workshop for Potential Literature), that explored the possibility of language in a way

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Mathematical Thought, transl. R. A. Hall and H. G. Bergmann, 2 vols. (New York: Dover, 1971).

[15] N. Bourbaki, "The Architecture of Mathematics," in *Great Currents,* ed. F. Le Lionnais, I: 23-36; A different English translation of this article was published earlier: "The Architecture of Mathematics," *American Mathematical Monthly,* 57 (1950): 221-232.

[16] A. Weil, "Sur l'étude algébrique de certains types de lois de mariage (Système Murngin)," in C. Lévi-Strauss, *Structures élémentaires,* 278-285.

directly inspired by Bourbaki. It becomes apparent, therefore, that already in the immediate postwar period the discourses I want to talk about seem to have been involved in some discussion. Let us see how these relationships came into being.

b) Bourbaki: The Emergence of a Myth

On December 10, 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."[18] Their names: Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, René de Possel, and Weil; a few months later they formed Bourbaki.[19] The story of this meeting and the following years which saw the emergence of Bourbaki, has been told in much details by Liliane Beaulieu.

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

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[17] R. Queneau, "The Place of Mathematics in the Classification of the Sciences," in *Great Currents,* ed. F. Le Lionnais, II: 73-77. R. Queneau, *Zazie dans le métro* (Paris: Gallimard, 1959).

[18] L. Beaulieu, *Bourbaki,* 147.

[19] Paul Dubreil, Jean Leray, and Szolem Mandelbrojt participated to some of the next meetings. Leaving the group before the summer, Dubreil and Leray never were formally members of Bourbaki. Later in 1935, they were replaced by Jean Coulomb and Charles Ehresmann. See L. Beaulieu, *Bourbaki,* 12-13. About de Possel, see E. Gilquin, ed., *De Bourbaki à la machine à lire. Journée d'hommage à René de Possel, 1905-1974 [16 novembre 1994],* 2 vols. (Paris: Institut Blaise Pascal, 1994).

construct the foundation for all mathematics with the help of the axiomatic method. The *Éléments de mathématique* claimed to take up "mathematics at the beginning, and [give] complete proofs."[20] The modesty of the word *Éléments* 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 were dealing with the bases of analysis in a very general, abstract way. As Beaulieu has documented, this emphasis departed from traditional textbooks. True, Bourbaki promised: "The general principles studied in Part I will find their applications in the following Parts." But, of course, nobody knew then, what these parts would contain or when they would appear.[21]

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, as I said, was to write a textbook of analysis. At first, the group consulted physicists and applied mathematicians, such as Jean Leray, Jean Coulomb and

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[20] N. Bourbaki, "Mode d'emploi de ce traité," in *Théorie des ensembles (Fascicule de résultats). Éléments de mathématique,* Ière partie, livre I (Paris: Hermann, 1939).
[21] The (unpublished) global plan for the treatise, reproduced in Beaulieu, *Bourbaki,* 2: 104, reveals that, in 1941, Bourbaki was at least planning three other parts (functional analysis, differential topology, and algebraic analysis). Bourbaki's quote is to be found in his "Mode d'emploi." For Beaulieu study of earlier textbooks, see *Bourbaki,* 171-190.

Yves Rocard.[22] 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. Later that day, Delsarte however 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."[23] Beaulieu's account shows, on the contrary, how this "packet" grew in the following years without an explicit decision to that effect. This was nearly 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 had not been so well conceptualized yet. 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."[24] As Beaulieu writes, this "was not a sacred principle given *a priori.* Only after consultations and try-outs was Bourbaki's *exposé* progressively purified from examples."[25] But, Bourbaki knew that this mode of presentation was striking to most reader:

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[22] L. Beaulieu, *Bourbaki,* 156-161.
[23] L. Beaulieu, *Bourbaki,* 150-151.
[24] N. Bourbaki, "Mode d'emploi."
[25] L. Beaulieu, *Bourbaki,* 376.

> The choice of this method was imposed by the principal object of this first part, which is to lay the foundations to the rest of the treatise, and even to 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 each other 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, or then, if he has the patience of suspending his judgment until he has had the occasion of convincing himself of this [usefulness].[26]

This act of faith, demanded from Bourbaki's reader, was made easier by his myth. The pseudonym, as Beaulieu emphasized, certainly had its function for members of the group.[27] It helped diffuse the tensions of collective writing among eminent mathematicians who, investing time and effort, saw their work severely criticized or offhand rejected, with no hope of immediate professional reward. Indeed, authorship for Bourbaki was a complicated affair. In a letter to Jean Perrin, then Under-Secretary of Scientific Research, Szolem Mandelbrojt 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 details, 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." In practice, until the 1960s, it often was Jean Dieudonné who wrote the final version.[28]

From the point of view of his audience, Bourbaki's persona became a powerful guarantee of legitimacy for his authoritative pronouncements. If a group of prominent mathematicians had agreed that these were the basic structures of mathematics then it

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[26] N. Bourbaki, "Mode d'emploi."
[27] L. Beaulieu, *Bourbaki,* 297-306.This pseudonym came from an old student prank of the *École normale.* In 1923, the freshman class, including Cartan, attended a phony lecture culminating with "Bourbaki's theorem," the name of a French general in 1870. See L. Beaulieu, *Bourbaki,* 278-282.

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.[29] 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 look at 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.

c) The Architecture of Mathematics

"This text deserves a special study," Jacques Roubaud recently wrote of Bourbaki's "Architecture." "There, Bourbaki quietly handles properly Neandertalian 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.[30] Even if philosophically naive, and perhaps because of this, this article was widely read.[31] Beaulieu since discovered that Dieudonné was its principal

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[28] Cf. L. Beaulieu, "Jeux d'esprit et jeux de mémoire chez N. Bourbaki," to be published. For Mandelbrojt's quote, see M. Chouchan, *Bourbaki,* 10.

[29] Among the early reviews of Bourbaki addressed to non-mathematicians, let me note: G. Bouligand, "Bourbaki s'affirme!," *Revue générale des sciences et Bulletin de la Société philomathique,* 40 (1948): 241-243; "Bourbaki en expansion," *Ibid.,* 42 (1950): 3-4; "L'École Bourbaki en face des secteurs décisifs de l'analyse," *Ibid.,* 43 (1951): 65-66; "Entre l'algèbre et l'analyse foncionnelle, Bourbaki et ses prouesses," *Revue générale des sciences et Bulletin de l'Association française pour l'avancement des sciences,* 60 (1953): 193-195; "Perspectives mathématiques," *Ibid.,* 62 (1955): 69-81; A. Lichnérowicz, "Qui est Bourbaki?," *Vie intellectuelle,* 16(2) (1948): 118-123; and E. Kahane, "La recherche collective," *La Pensée,* 21 (1948): 85-88.

[30] J. Roubaud, *Mathématique:* (Paris: Seuil, 1997), 114-132; quote on p. 123.

[31] Compare with the sophisticated defense of axiomatics by C. Chevalley and A. Dandieu, "Logique hilbertienne et psychologie," *Revue philosophique de France et de l'étranger,*

author and that it apparently was not discussed by the group.[32] For my purpose, since neither Bourbaki, nor any of his collaborators ever retracted it, we can safely take this article 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 1900 Paris International Congress of Mathematicians.[33] By 1948, however, 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. Universal minds, like Poincaré and Hilbert, seemed to belong to the past. Now mathematicians only hoped to master their own specialties, which possessed its terminology and methods not necessarily applicable to other fields of mathematics. Worst still, the same terms sometimes meant different things whether used in algebra or topology. The question was therefore worth raising: "Is the mathematics of today singular or plural?"[34]

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

57 (1932): 399-411, and C. Chevalley, "Variations du style mathématique," *Revue de métaphysique et de morale,* no. 3 (1935): 375-384.

[32] L. Beaulieu, *Bourbaki,* vol. 2, 77.

[33] D. Hilbert, "Mathematical Problems." *Bulletin of the American Mathematical Society,* 8 ([1900] 1902): 437-479, authorized transl. M. W. Newson; quoted from C. Reid, *Hilbert--Courant* (New York: Springer, 1986), 82.

[34] N. Bourbaki, "The Architecture of Mathematics," 24. In the following section, I quote consistently from the version published in F. Le Lionnais, *Great Currents of Mathematical Thought,* and indicate the page number in parentheses.

> 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'.[35].

The axiomatization of various branches of mathematics was hardly anything new in 1948. But Bourbaki based his use of axiomatics on a new concept that allowed him, so he thought, to extend coherently this method to all of modern mathematics. "The common trait of the various notions designated by [the] generic name" of *mathematical structure,* Bourbaki wrote, "is that they apply to sets of elements whose nature *is not specified.*"[36] 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."[37] Then considered as not definable, not formally defined before 1957, the notion of structure always remained problematic for Bourbaki's enterprise, and at the same time central to his discourse.[38]

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 group—sets endowed with a law of composition

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[35] N. Bourbaki, "The Architecture," 25.
[36] N. Bourbaki, "The Architecture," 28. His emphasis.
[37] Quoted by L. Beaulieu, *Bourbaki,* 327.
[38] See L. Corry, "Nicolas Bourbaki and the Concept of Mathematical Structure," *Synthese,* 92 (1992): 315-348; and Corry's book, *Modern Algebra and the Rise of mathematical Structure.*

satisfying three simple axioms.[39] 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. Slowly, mathematicians had come up with the group concept, after having been exposed through decades of experience to similar properties in many different cases.[40] With Bourbaki, following here 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, substitutions, etc., were all groups.

Just as we always can 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 $x \tau y = x \tau z$, then surely $y = z$ can be proven at once for all groups.[41] Above all,

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[39] Given a set G, suppose that it can be endowed with an "operation" τ, which, to any two elements x and y of the set G, associates a third element z, corresponding to the "product" of x by y, noted $z = x \tau y$. In order for G to be a group under this operation, the following three properties have to be verified. (1) For all elements x, y and z of G, we have $x \tau (y \tau z) = (x \tau y) \tau z$ ("associativity"); (2) G possesses a neutral element e, such that $e \tau x = x \tau e = x$; and (3) each element of G has an inverse x^{-1}, so that $x \tau x^{-1} = x^{-1} \tau x = e$.

[40] H. Wussing, *The Genesis of the Abstract Group Concept: A Contribution to the History of the Abstract Group Concept,* transl. A. Shenitzer (Cambridge: MIT Press, [1969] 1984).

[41] The proof is the following: from property (3) above, we know there is a x^{-1} in G, we deduce that $x^{-1} \tau (x \tau y) = x^{-1} \tau (x \tau z)$; then, because of associativity, $(x^{-1} \tau x) \tau y = (x^{-1} \tau x) \tau z$; therefore, using (3) again, we find $e \tau y = e \tau z$; finally, applying (2), we get $y = z$. Q.E.D.

Bourbaki wanted his readers to note that "the *nature* of the elements x, y, z was completely irrelevant in this argument."[42] This is why structures were defined as sets of elements whose nature was arbitrary. In his conception of mathematics, Bourbaki noted, 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 claimed:

> 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.[43]

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." 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').[44]

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[42] N. Bourbaki, "The Architecture," 28.
[43] N. Bourbaki, "Foundations of Mathematics for the Working Mathematician," *Journal of Symbolic Logic,* 14 (1949): 1-8, quote on p.8.
[44] N. Bourbaki, "The Architecture," 28-29.

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."[45]

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 behaviour [mathematicians] had a right to expect from the mathematical entities which a long association had rendered as familiar to [them] as objects of the real world."[46] 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 historical reliance on physics and 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.[47]

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[45] P. D. Lax, "The Flowering of Applied Mathematics in America," *A Century of Mathematics in America,* Part II, ed. P. Duren with R. A. Askey and U. C. Merzbach (Providence: American Mathematical Society, 1989): 455-466.
[46] N. Bourbaki, "The Architecture," 31.
[47] A. Weil, "The Future of Mathematics," in *Great Currents,* ed. F. Le Lionnais: 321-336, 332.

On the other hand, Bourbaki claimed that mathematics was relatively independent from 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.[48]

Axiomatics freed mathematics from reality, or rather from errors due to the abuse of our intuition. Therefore, its utility for other sciences 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. . . . 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.[49]

Moreover, this position conveniently made mathematics independent from 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.[50]

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[48] N. Bourbaki, *Éléments de mathématique. Livre 1: Théorie des ensembles,* Chapitre 1 et 2 (Paris: Hermann, 1954), introduction.
[49] N. Bourbaki, "The Architecture," 35-36. His emphasis.
[50] N. Bourbaki, "Foundations of Mathematics," 2.

d) Structures of Kinship

If Bourbaki was so skeptical of other sciences having a role to play in orienting contemporary mathematical research, how, then, are we to understand André Weil's collaboration with Claude Lévi-Strauss? He did, after all, write a mathematical appendix for *The Elementary Structures of Kinship.* In fact, Weil's involvement conformed to Bourbaki's philosophy. Bourbaki welcomed the application of mathematics to other fields of knowledge. The emphasis must be put here on the word *application,* which already presumes the nature of the relation between mathematics and science.[51] 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 them both to New York City, at the *École libre des hautes études* (Free School for Advanced Studies), 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

[51] About, the abuses of the word *application* concerning mathematics, see J.-M. Lévy-Leblond, "Physique et mathématique," *Encyclopaedia universalis,* 13 (1968): 4-8.

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.*"[52] Where for Hadamard marriage was not amenable to mathematical treatment because it was not a mathematical object, Weil could not care less, since 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 *École libre.*[53] The history of structural linguistics has often been told, starting with Saussure at the beginning of the century, culminating with the French structuralist movement of the sixties, *via* the interwar Prague Linguistic Circle to which Jakobson belonged.[54] The Prague linguists started to use the term *structure* around 1929.

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[52] C. Lévi-Strauss and D. Éribon, *Conversations with Claude Lévi-Strauss,* transl. P. Wissing (Chicago: University of Chicago Press, [1988] 1991), 52-53. My emphasis. See also A. Weil, *Apprenticeship of a Mathematician,* transl. J. Gage (Basel: Birkhäuser, [1991] 1992), 185.

[53] C. Lévi-Strauss and D. Éribon, *Conversations,* 41; and C. Lévi-Strauss, *The View from Afar,* transl. J. Neugroschel and P. Hoss (New York: Basic, [1983] 1985), 138-147.

[54] See, for example, J.-M. Benoist, *The Structural Revolution* (London: Weidenfeld & Nicholson, [1975] 1978); F. Dosse, *Histoire du structuralisme,* 2 vols. (Paris: La Découverte, 1991-1992); R. Harland, *Superstructuralism: The Philosophy of Structuralism and Post-Structuralism* (London, New York: Methuen, 1987); J. G. Merquior, *From Prague to Paris: A Critique of Structuralist and Post-Structuralist Thought* (London, New York: Verso, 1986); T. G. Pavel, *The Feud of Language: A*

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 had to admit 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 Émile Benveniste)."[55] 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 already talked of the "structure of a group," and Élie Cartan (Henri's father) wrote a thesis in 1894 titled *La Structure des groupes continus.* Later, in the early 20th century, Ore and Glivenko named *structures* what are now known as lattices.[56] 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."[57] There is therefore no firm historical reason to assume, on the sole basis of their common name, that the mathematicians' structures and 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

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History of Structuralist Thought, transl. L. Jordan and T. G. Pavel (Oxford: Basil Blackwell, 1989).
[55] A. Weil, *Apprenticeship,* 114.
[56] A. Marchal, "L'attitude structuraliste et le concept de structure en économie politique," in *Sens et usages du terme structure dans les sciences humaines et sociales,* ed. R. Bastide, Janua linguarum, 16 (The Hague: Mouton, 1962): 63-67; see also G. Guilbaud's comment *Ibid.,* 140-141.
[57] C. Chevalley, "Variations du style mathématique," 384.

probably the only one which can truly claim to be a science."[58] 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."[59] What Jakobson taught in his course on phonology could be directly applied, Lévi-Strauss thought, to anthropology.

In his lectures at the *École 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."[60] 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 aim at the discovery of 'the invariants behind all this

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[58] C. Lévi-Strauss, "Structural Analysis in Linguistics and in Anthropology," in *Structural Anthropology,* transl. Brooke G. Schoepf (New York: Basic Books, 1963), 31-54; 31. Originally published in French in *Word: Journal of the Linguistic Circle of New York,* 1(2) (1945), 1-21; repr. in *Anthropologie structurale* (Paris: Plon, 1957), 37-62.
[59] C. Lévi-Strauss and D. Éribon, *Conversations,* 41.
[60] R. Jakobson, *Six Lectures on Sound and Meaning,* transl. J. Mepham (Hassocks, UK: Harvester Press, [1976] 1978), 35. C. Lévi-Strauss's preface is repr. as "The Lessons of Linguistics," in *The View from Afar,* 138-147, cited above.

variety'."[61] 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."[62] 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.*[63] 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.[64]

Lévi-Strauss wanted to uncover common features among systems of kinship. How to 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

61 C. Lévi-Strauss, in R. Jakobson, *Six Lectures,* xii; C. Lévi-Strauss, *The View from Afar,* 139. Lévi-Strauss is quoting from p. 9 of Jakobson's book.
62 N. Trubetzkoy, "La phonologie actuelle", *Psychologie du langage* (Paris, 1933), 227-246, 233; quoted by É. Benveniste, " 'Structure' en linguistique," in *Sens et usages,* 31-39; on p. 35. My emphasis.
63 R. Jakobson, "Retrospect (1961)," *Selected Writings I* (The Hague, Paris: Mouton, 1971), 632-637.
64 Quoted by E. Holenstein, *Roman Jakobson's Approach to Language: Phenomenological Structuralism,* transl. C. and T. Schelbert (Bloomington: Indiana University Press, [1974] 1976), 1.

led astray by a multiplicity of terms, one should consider the simplest and most intelligible relationships uniting them."[65] An important aspect of Jakobson's structuralism indeed 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 of characteristic of a structure was that "*it consists of elements such that any modification of one of them entails a modification of all others.*"[66] With the help of such a structural 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."[67] 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 discipline by emphasizing elementary structures. But while Bourbaki imposed systemic structures onto sets of unspecified elements, Lévi-Strauss emphasized

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[65] C. Lévi-Strauss, *The View from Afar,* 139; "Preface" in R. Jakobson, *Six Lectures,* xii.
[66] C. Lévi-Strauss, "Social Structure," *Anthropology To-Day: Wener-Gren Foundation International Symposium on Anthropology,* ed. A. L. Kroeber (New York: Columbia University Press, 1952); repr. in *Anthropologie structurale* (Paris: Plon, 1958), 303-351. Quote in chap. 15, part 1; my emphasis. In this text, Lévi-Strauss considered analogies in mathematics, taken from cybernetics, information theory, and game theory, rather than from Bourbaki.

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."[68] 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.[69] 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."[70] In conclusion, we can reasonably say that the intersection of Lévi-Strauss, Jakobson, and Weil, in New York City in 1943, by crossbreeding anthropology, linguistics, and

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[67] H. Gardner, *The Quest for the Mind: Piaget, Lévi-Strauss, and the Structuralist Movement,* 2nd ed. (Chicago and London: University of Chicago Press, 1981), 40.
[68] C. Lévi-Strauss, *Structures élémentaires,* 285-286.
[69] P. Jorion and C. Assaba's comments (pp. 377-378) following an article by M. W. Barbosa de Almeida ["Symmetry and Entropy: Mathematical Metaphors in the Work of Lévi-Strauss," *Current Anthropology,* 31 (1990):367-385], an article in which the importance of Lévi-Strauss's use of mathematics is hotly debated. Weil's models were however developped by H. C. White, *An Anatomy of Kinship: Mathematical Models for Structures of Cumulated Roles* (Englewoods Cliffs, NJ: Prentice-Hall, 1963) and P. Courrège, "Un modèle mathématique des structures élémentaires de parenté," *L'Homme,* 5, no. 3-4 (1965): 248-290; repr. in *Anthropologie et calcul,* ed. P. Richard and R. Jaulin (Paris: Union générale d'éditions, coll. "10/18", 1971), 126-181. Courrège imself was an ardent Bourbakist, it should be noted, according to the romanced portrait in J. Roubaud, *Mathématique:,* 73-81. See C. Lévi-Strauss, "The Mathematics of Man," *International Social Science Bulletin,* 6(4) (1954): 581-590, for his view of the usefulness of mathematical methods; and for a critique, see A. Régnier, "De la théorie des groupes à la pensée sauvage," *Anthropologie et calcul,* 271-298 et *La Crise du langage scientifique* (Paris: Anthropos, 1974).

mathematics, helped make structuralism possible. And although the dialogue between mathematics and structuralism failed to be sustained, this fortuitous encounter was the seed for a lasting cultural connection.

3. 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 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.

a) Bourbaki's Reign

Bourbaki did not need structuralism to establish his hegemony over his discipline. He added new booklets to his *Éléments de mathématique,* more than 35 of which were published by the end of the 1970s. As early as 1951, several of the earlier volumes were 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

[70] C. Lévi-Strauss and D. Éribon, *Conversations,* 53.

programmatic article "The Architecture of Mathematics" was translated into English, Portuguese, Russian, German, and Japanese.[71]

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 in 1958 (never himself a Bourbaki, but a student of Cartan and Ehresmann). 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, who lavishly praised his colleague Alexander Grothendieck when he presented him with the fourth Medal in 16 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."[72]

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 *École normale supérieure* from 1940 on, he bred an entire generation of French mathematicians to whom he would

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71 L. Beaulieu, *Bourbaki.*

72 Quoted by J. Fang, *Bourbaki,* 58. Retrospectively, it may appear highly problematic to include Thom, Atiyah, and Smale in Bourbaki's sphere of influence, but their styles and topics certainly were, then, close to his own. For a view of Bourbaki's international dominance, see V. I. Arnol'd, "Will Mathematics Survive?: A Report from the Zurich Congress." *Mathematical Intelligencer,* 17(3) (1995): 6-10.

strongly suggest studying Bourbakist mathematics.[73] 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 F) from the Academy of Sciences in 1966.[74] 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), three more Bourbakis having been elected either as non-resident member (Dieudonné), or correspondents (Chevalley and Serre).

Mostly, from the forties onward, Bourbaki's dynamic nature oriented ambitious students towards his topics of predilection: algebraic geometry above all, but also number theory, group theory, algebraic and differential topology, a hierarchy best exemplified by Dieudonné's *Panorama.*[75] 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 exhaustively 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

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[73] M. Andler, "Les mathématiques à l'École normale supérieure au XXe siècle: une esquisse." *École normale supérieure. Le livre du bicentenaire,* ed. J.-F. Sirinelli (Paris: Presses universitaires de France, 1994): 351-404, esp. pp. 371-380.
[74] L. Beaulieu, *Bourbaki,* 160; *Comptes rendus de l'Académie des Sciences, Vie académique,* 263 (1966), 146.
[75] J. Dieudonné, *Panorama des mathématiques pures. Le choix bourbachique* (Paris: Gauthier-Villars, 1977).

University, in the 1940s and 1950s have testified to the subtle blend of pressure and appeal that Bourbaki exerted on the young generation.[76]

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."[77] 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 Marseille 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 were especially exciting to him, this led the study of nonlinear systems to sit on the border of physics and mathematics: an "uncomfortable

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[76] See, *e.g.,* A. Grothendieck, *Récoltes et semailles. Réflexions et témoignage sur un passé de mathématicien,* 7 vols. (Montpellier: Université des Sciences et Techniques du Languedoc and CNRS, 1985), Fine Library, Princeton University; M. Serres, *Éclaircissements. Entretiens avec Bruno Latour* (Paris: François Bourin, 1992), 21-23; *Conversations on Science, Culture, and Time,* transl. R. Lapidus (Ann Arbor: University of Michigan, 1995), 10-11; J. Roubaud, *Mathématique:,* L. Schwartz, *Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997). See also the account of the mathematical education of the Economics Nobel prize winner, Gérard Debreu, by E. R. Weintraub and P. Mirowski, "The Pure and the Applied: Bourbakism Comes to Mathematical Economics," *Science in Context,* 7 (1994): 245-272.

[77] G. Choquet, "Témoignage sur Paul André Meyer," *Gazette des mathématiciens,* no. 68 (April 1996): 13-14

situation, certainly in France."[78] Only in the late 1960s could a French school of applied mathematics develop under the leadership of Jacques-Louis Lions.[79] Meanwhile, as mathematicians looked for ways get around Bourbaki's dominance over their field, his name had begun being invoked again in the human sciences.

b) The Rise of Structuralism: The First Interdisciplinary Conferences (1956-1959)

At first, Lévi-Strauss's *Elementary Structures* was well received even among existentialists.[80] But structuralism's rise to prominence 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 1966-1968 did structuralism definitively replace existentialism at the zenith of French philosophy. Only then had Claude Lévi-Strauss, Roland Barthes, Jacques Lacan, and Michel Foucault taken Sartre's place; "those were the names of the 'new masters' [*nouveaux maîtres*]," among whom Louis Althusser should definitely be counted.[81] 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

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[78] T. Vogel, ed., *Les Vibrations forcées dans les systèmes non-linéaires,* Colloque international du CNRS, no. 148, Marseille: 7-12 September 1964 (Paris: Éditions du CNRS, 1965), 11-12.
[79] A. Dahan Dalmedico, "Polytechnique et l'École française de mathématiques appliquées," *La France des X, deux siècles d'histoire,* ed. B. Belhoste, et al. (Paris: Economica, 1995) and "L'essor des mathématiques appliquées aux États-Unis: l'impact de la seconde guerre mondiale," *Revue d'histoire des mathématiques,* 2 (1996): 149-213.
[80] See S. de Beauvoir's review in *Les Temps modernes,* 5, no. 49 (1949): 943-949.

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.[82] 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*, 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 January 10-12, a symposium on the "Meanings and Uses of the Term Structure in the Human and Social Sciences," sponsored

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[81] B. Pingaud in *L'Arc,* no. 30 (March 1966). Quoted in *Les Idées en France,* ed. A. Simonin and H. Clastres, 228.
[82] É. Roudinesco, *Jacques Lacan. Esquisse d'une vie, histoire d'un système de pensée* (Paris: Fayard, 1993), 469.

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 July 25 to August 3, at Cerisy-la-Salle, where Swiss psychologist Jean Piaget was the driving force. This conference focused on the dual theme of "Genesis and Structure."[83]

Although both conferences were sponsored in part by the VIth Section of the *École pratique des hautes études,* few people participated in both. But, to the sociologist Lucien Goldmann, who did, the approaches of the two conferences differed enough to warrant a distinction between two sorts of structuralism—a distinction which I shall adopt here, more for convenience than because it reflected a fundamental division of structuralists, because I found that it often aptly overlapped with different attitudes towards the relation of structuralism to mathematics.[84] On the one hand there was the more standard "non-genetic 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."[85] The lesson taken

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[83] Proceedings for these two conferences were subsequently published: R. Bastide, ed., *Sens et usages du terme structure dans les sciences humaines et sociales,* Janua linguarum, 16 (The Hague: Mouton, 1962); M. de Gandillac, L. Goldmann, and J. Piaget, eds., *Entretiens sur les notions de genèse et de structure,* Colloque de Cerisy-la-Salle (Paris, The Hague: Mouton, 1965).

[84] L. Goldmann, "Introduction générale," *Genèse et structure,* 7-22; and "Le concept de structure significative en histoire de la culture," *Sens et usages,* 124-135.

[85] R. Bastide, "Introduction à l'étude du mot 'structure'," in *Sens et usages,* 9-19, on p. 17.

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."[86]

Contrary to the two conferences above who offered clear visions of what structuralism should be, a third meeting had been held in Paris, three years earlier, on April 18-27, 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 *non-structuralist* (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 non-structuralist: "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.[87]

Given the considerable divergence between all three conferences on a number of fundamental issues, it is remarkable that, each time, mathematics played a comparable

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[86] M. de Gandillac, "Jalons pour une conclusion," in *Genèse et structure,* 337-353, on p. 347; paraphrasing a comment made earlier by Jean Desanti, *Ibid.,* 153.
[87] Proceedings were also published: XX^{e} Semaine de Synthèse, *Notion de structure et structure de la connaissance* (Paris: Albin Michel, 1957), pp. xi-xii, and xxiii for quotes.

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 non-genetic 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 emphasizing relations among elements of the structure; and mathematics, for those studying its systemic essence. Significantly, the Paris symposium emphasized biology, especially in the published proceedings, and 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."[88] 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 if Bourbaki's definition meant something

[88] J. Desanti, "Remarques sur la connection des notions de genèse et structure en mathématiques," *Genèse et structure,* 143-159, 143.

outside of mathematics. A deeper look at the two types of structuralism reveals that they diverged in their actual use of mathematics. In general, non-genetic 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 in 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.[89]

Paying lip service to, or totally ignoring, mathematics became a widespread attitude in (non-genetic) structuralism. When in the late sixties, a flurries 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.[90] Which, of course, cannot be very surprising considering that

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[89] R. Pagès in "Compte rendu du colloque sur le mot structure," *Sens et usages,* 156.
[90] No mention of mathematics, nor Bourbaki in, for example, the special issues of the journals *L'Arc,* no. 26, devoted to "Lévi-Strauss" (March 1965), *L'Esprit,* 31, no. 322, devoted to "*La Pensée sauvage* et le structuralisme" (November 1963), 545-653; and *L'Esprit,* 35, no. 360, devoted to "Structuralismes, idéologie et méthode" (May 1967), 769-901; and the books of J.-B. Fages, *Comprendre le structuralisme* (Toulouse: Privat, 1967); O. Ducrot, T. Todorov, D. Sperber, M. Safouan, and F. Wahl, *Qu'est-ce que le structuralisme?* (Paris: Seuil, 1968), except for Sperber's chapter on anthropology, 179-

Bourbaki never informed the works of any great structuralist thinker, other than for providing an illustration for the complexity of authorship![91]

c) Jean Piaget and Genetic Structuralism

Swiss psychologist Jean Piaget, contrary to 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. From 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 emphasized the centrality of mathematics. "A critical account of structuralism," he wrote, "must begin with a consideration of mathematical structures."[92]

Mathematics, as the key to a structural synthesis of the sciences, was one of the suggestions that, already in 1956, emerged during the *Synthèse* week. 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:*

180, and 223-228. Laconic passages in J.-M. Auzias, *Clefs pour le structuralisme* (Paris: Seghers, 1967), 9; and M. Corvez, *Les Structuralistes, les linguistes... Michel Foucault, Claude Lévi-Strauss, Jacques Lacan, Louis Althusser, les critiques littéraires* (Paris: Aubier-Montaigne, 1969), 12-13.

[91] M. Foucault, "Qu'est-ce qu'un auteur?," *Bulletin de la société française de philosophie,* 63(3) (1969): 73-104; repr. in *Dits et écrits, 1954-1988,* ed. D. Defert and F. Ewald, vol. 1 (Paris: Gallimard, 1994): 789-821, 797.

[92] J. Piaget, *Structuralism,* transl. C. Maschler (New York: Basic, [1968] 1970), 17.

> 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.[93]

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 which, as systems, presented laws of totality distinct from the proprieties of their elements."[94] Indeed, if not all structures were mathematized, he "very resolutely leaned" towards thinking of them as at least potentially mathematizable.[95]

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.[96] 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 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

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[93] Synthèse, *Notion de structure,* 415.
[94] J. Piaget, "Genèse et structure en psychologie," in *Genèse et structure,* ed. M. de Gandillac et al.: 37-61, 37.
[95] *Ibid.,* 54. Later, Jean Ullmo offered an extreme version: "Generally a structure can *only* be precisely defined by a group [of transformations]." *La Pensée scientifique moderne* (Paris: Flammarion, 1969), 262. My emphasis.

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 emerged in a sequence which was 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."[97] 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 was '*q* implies *p*', and whose negation was '*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 that taken two by two, they were equivalent to the third one ($NR = C$, $CR = N$, and $NC = R$), they formed a group of 4 elements.[98] 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.

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96 J. Piaget, "Genèse et structure," 44-48.
97 J. Piaget, "Genèse et structure," 40.
98 J. Piaget, *Traité de logique. Essai de logistique opératoire* (Paris: Armand Colin, 1949), 268-286.

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."[99] 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 left 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 that I had found in children's thinking, and to the great astonishment of us both, 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.[100]

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), then the "linear order" of science extolled by Vienna Circle positivists (and Auguste Comte before) was to be replaced by a "circle."[101] For him, the "logico-mathematical" sciences, on which the rest

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[99] J. Piaget, "Les structures mathématiques et les structures opératoires de l'intelligence," *L'Enseignement des mathématiques:* 11-33 (Neuchatel and Paris: Dulachaux & Niestlé, 1955), 17.
[100] J. Piaget, *Genetic Epistemology,* transl. Eleanor Duckworth (New York and London: Columbia University Press, 1970), 26.
[101] J. Piaget, *Introduction à l'épistémologie génétique,* 3 vols., esp. tome I, *La pensée mathématique* (Paris: Presses universitaires de France, 1950), 49-50. See also J. Gayon,

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."[102] 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 respect, it was a strange view of structuralism 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."[103] 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

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"Génétique, psychologie génétique et épistémologie génétique dans l'œuvre de Jean Piaget (1896-1980): une ambiguïté remarquable," *L'Ordre des caratères. Aspects de l'hérédité dans l'histoire des sciences de l'homme,* ed. C. Bénichou (Paris: Vrin, 1989): 147-173, 162.

[102] L. Apostel, *Logique et preuve,* vol. 5 (Methodos, 1953), 305; quoted by J. Piaget, "Les structures mathématiques," 30.

which we must accommodate ourselves instead of the reverse."[104] 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 front stage 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."[105] By the late sixties, when ironically it became a popular fad, the structuralist movement was loosing all coherence, supposing it once 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 knew very well that he was dealing with structures, but which he defines in a given way in mathematics, biology, linguistics, etc.[106]

Piaget's book appears, in retrospect, as a desperate effort at presenting a unified structuralism with scientific pretense.

One last-resort attempt at salvaging structuralism indeed distinguished between the true scientific uses of structures and mere ideological ones. For this reason, Piaget

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[103] J. Piaget, *Structuralism,* 17 and 23.
[104] *Ibid.,* 30.
[105] J. Pouillon, "Présentation: un essai de défintion," in *Les Temps modernes,* 22, no. 246 (November 1966): 769-790, 769.

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.[107] In order to better criticize 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, the emphasis lay on the clarity of the mathematical usage. She nonetheless pointedly questioned whether human structures really were the same as Bourbaki's.

d) The Oulipo: Bourbakist Literature?

"My idea of prose was greatly influenced by . . . Bourbaki's famous treatise."[108] 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.

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[106] Comment in J.-M. Auzias et al., *Structuralisme et marxisme* (Paris: Union générale d'éditions, 1970), 238.
[107] J. Parain-Vial, *Analyses structurales et idéologies structuralistes* (Toulouse: Privat, 1969). A similar strategy of distinguishing scientific method and ideology is at play in a critical conference held in 1967-1968, cf. J.-M. Auzias et al., *Structuralisme et marxisme.*
[108] J. Roubaud, *'Le grand incendie de Londres'* (Paris: Seuil, 1989), 148.

On November 24, 1960, a peculiar semi-secret literary society was founded, mainly inspired by the mathematician François Le Lionnais, once again, and the writer and amateur mathematician Raymond Queneau.[109] On 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."[110] They sought to experiment with formal constraints, imposed on the production of literature. In a 1962 interview on French radio, Queneau defined potential literature as such: "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 word—that may be used by writers in any way they see fit."[111] 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

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[109] A proud member of the *Société mathématique de France,* Queneau also published two articles in scientific journals: "Théorie des nombres: sur les suites *s*-additives," *Comptes-rendus de l'Académie des Sciences A,* 266 (6 May 1968): 957-958; and "Sur les suites *s*-additives," *Journal of Combinatorial Theory,* 13 (1972): 31-71.
[110] J. Bens, *OuLiPo, 1960-1963* (Paris: Christian Bourgois, 1980), 20. This book contains summaries of the first three years of Oulipian meetings.
[111] G. Charbonnier, *Entretiens avec Raymond Queneau* (Paris: Gallimard, 1962), 140; quoted (and slightly modified) by J. Lescure, "A Brief History of the Oulipo," *Oulipo: A Primer of Potential Literature,* ed. and transl. W. F. Motte (Lincoln, University of Nebraska, 1986), 38. My emphasis.

circumspection."[112] 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."[113] 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."[114] The role of mathematics was to provide the Oulipians with abstract structures that could be imported in literature. "Mathematics," Le Lionnais added, "particularly the abstract structures of contemporary mathematics, proposes thousands of possibilities for exploration, both algebraically, . . . and topologically."[115] 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.[116]

Their favorite exemplars of potential literature was Queneau's stunning *Hundred Thousand Billion Poems.*[117] On the face of it, this was just a collection of ten sonnets, each comprising 14 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,

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[112] F. Le Lionnais, "Second Manifesto," *Oulipo: A Primer*, 29.
[113] J. Bens, *OuLiPo,* 49.
[114] R. Queneau, "Science and Literature," *Times Literary Supplement* (September 28, 1967), 863-864. Note that Jean Piaget edited a volume on "logic and human knowledge" for the *Encyclopédie de la Pléiade,* whose general editor was none other than Queneau. *Logique et connaissance humaine* (Paris: Gallimard, 1967).
[115] F. Le Lionnais, "Lipo: First Manifesto," *Oulipo: A Primer,* 27.
[116] J. Bens, *OuLiPo,* 238; see also pp. 148 and 180. See F. Le Lionnais, " Quelques structures et notions mathématiques," Oulipo, *La Littérature potentielle (Créations, re-créations, récréations)* (Paris: Gallimard, 1973), 295-298.

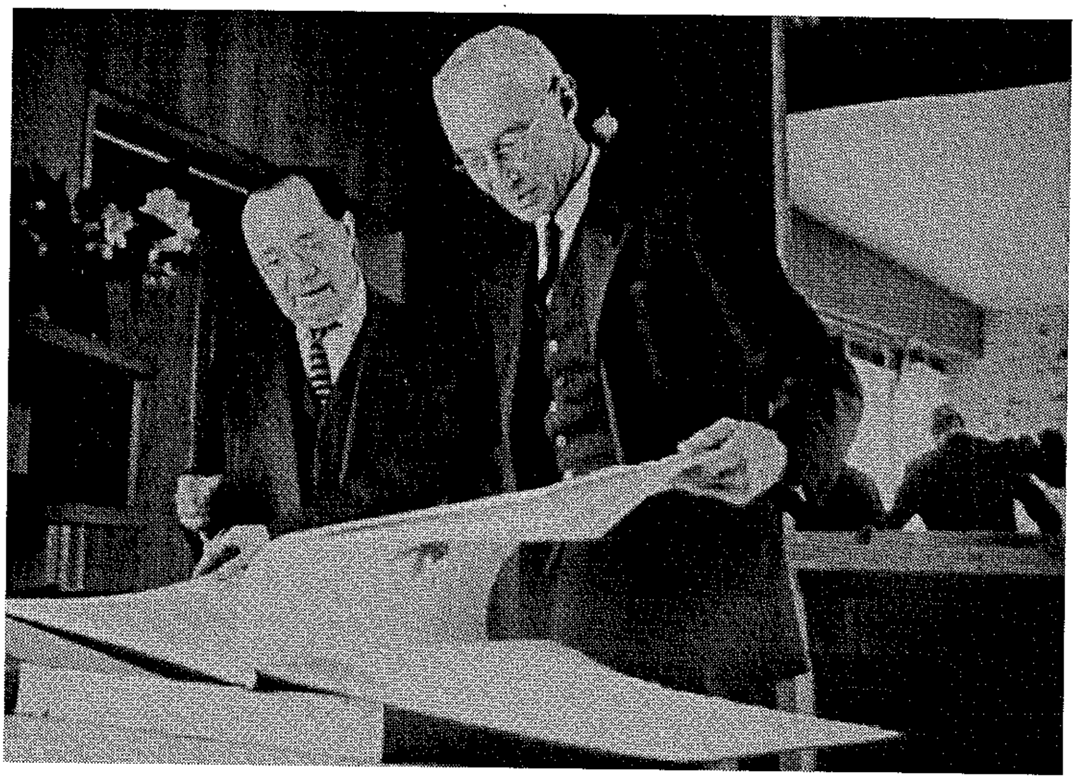

Figure 2: François Le Lionnais and Robert Oppenheimer at the IHÉS in 1963. Copyright © Arch. IHÉS.

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],

[117] R. Queneau, *Cent mille milliards de poèmes* (Paris: Gallimard, 1961).

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):

> *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."[118] 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.[119]

The Oulipo cataloged both new structures and old ones unearthed from the depth 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

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[118] J. Dieudonné, *Panorama,* xii.
[119] J. Bens, *OuLiPo,* passim.

starting from new mathematics."[120] 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 a point of honor to . . . qualify the text in question as 'plagiarism by anticipation'."[121] Did this attitude also characterize Bourbaki's oft-criticized teleological view of history?[122]

The only things that interested the Oulipo as a group were, not specific examples, but methods. In the reports of their first 40 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.[123] "The very meaning of the Oulipo is to provide empty structures," Queneau concurred.[124] 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 visited, in March 1962, a Bourbaki congress.[125] He helped popularize his work:

> The article that represents the intersection of these two interesting personalities . . . constitutes a subset of the issue no. 176 of *Critique* . . . according to which it is

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[120] J. Bens, *OuLiPo,* 9.
[121] F. Le Lionnais, "Second Manifesto," 31; see J. Bens, *OuLiPo,* 179.
[122] N. Bourbaki, *Elements of the History of Mathematics,* transl. J. Meldrum (Berlin and New York: Springer, [1960] 1994).
[123] J. Bens, *OuLiPo,* 81.
[124] G. Charbonnier, *Entretiens with Queneau,* 154-155.
[125] *La Tribu. Bulletin oecuménique, apériodique et bourbachique,* 56 (Congrès d'Amboise, mars 1962), 1. I thank Beaulieu for communicating me 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 of Bourbaki's *Theory of Sets.* I thank Catherine Chevalley for providing me a copy of this 1951 draft.

allowed to detect [*subodorer*] a few isomorphisms between Queneau and Bourbaki.[126]

Both indeed shared a common insistence on axiomatics, formal beauty vs. future utility, and especially humor, which seemed to delight Queneau. Both the Oulipo and Bourbaki were semi-secret societies founded on myths; both looked at the formal bases of their respective disciplines, and wished to rewrite its history 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,* that would investigate the fecundity of their approach for mathematics. Even if some Bourbakis appreciated Oulipian prose, an *OuMathPo* seems never to have gotten off the ground.[127]

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 Bourbakian 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 which social scientists could hardly live up to. Thus, almost none of them, with the notable exception of Jean Piaget, attempted to base, in a critical way, their structural method on his mathematics. Bourbaki served most usefully as a guarantor for rigor, a signal meaning that structuralism was real science. Typical of late

[126] F. Le Lionnais, in *Dossiers du Collège de 'Pataphysique,* nos. 18/19 (7 Clinamen 89 EP [vulg. 29 March 1962]), 68. R. Queneau, "Bourbaki et les mathématiques de demain," *Critique,* no. 176 (janvier 1962): 3-18.

modernist thought, structural discourses in mathematics, literature, and the social sciences were on the look 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, metanarratives, 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.

4. DECLINE

The decade of 1970 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. 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

[127] F. Le Lionnais, "Queneau et les mathématiques," in *Raymond Queneau,* ed. A. Bergens (Paris: l'Herne, 1975), 279n; see also *Oulipo: A Primer,* 190n.3. M. Chouchan, *Bourbaki,* 9.

involves the recognition of the fact that a discussion established earlier went on with different cultural connectors with an impact that resonated widely.

a) 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."[128] 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 book, he described, mostly without footnotes, a personal evolution that should be seen here as an illustration, not 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." 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*

[128] V. Descombes, *Le Même et l'autre. Quarante-cinq ans de philosophie française (1933-1978)* (Paris: Minuit, 1979); *Modern French Philosophy,* transl. L. Scott-Fox and J. M. Harding (Cambridge: Cambridge University Press, 1980), 85.

the novelty of contemporary [structuralist] methods."[129] 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." He espoused the radical belief that regions of order were created from a turbulent sea of chaos, and this stabilization of order, of knowledge, became his major object of study. Serres renounced structuralism on the ground that "reality is not rational."[130] 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 latter books were literary, almost poetic, works of philosophy that appealed more to 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

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129 M. Serres, *Hermès I. La communication* (Paris: Minuit, 1968), 32, and 28-29. His emphasis. This definition of structure is repeated word for word in M. Serres, *Le Système de Leibniz et ses modèles mathématiques,* I (Paris: Presses universitaires de France, 1968), 4. Serres's distinction between objects and relations between objects also stemmed from Gaston Bachelard's reading of quantum mechanics and relativity. See esp. his "Noumène et microphysique," (1931) in *Études* (Paris: Vrin, 1970); and *La Valeur inductive de la relativité* (Paris: Vrin, 1929), 98-99; both repr. in G. Bachelard *Épistémologie: textes choisis,* ed. D. Lecourt (Paris: PUF, 1971), 11 and 28.

130 M. Serres, *Hermès IV. La distribution* (Paris: Minuit, 1977), 110 and 10. See "Estime," in *Ibid,* 275-290.

relation with mathematics, and show how philosophy could, to use his own metaphor, discover "the Northwest passage" between the two cultures.[131]

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, and especially Bourbakism, which had set forth one of the "scientific revolutions" he said he 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."[132]

Aware of algebra and topology, Serres first worked, as a student, on the epistemology of Bourbaki's structures.[133] 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 an historian than a mathematician.[134] Serres based his understanding of Leibniz on contemporary mathematics, without sinking

[131] M. Serres, *Hermès V. Le Passage du Nord-Ouest* (Paris: Minuit, 1980).
[132] M. Serres, *Conversations,* 10. See also M. Serres, *Hermès II. L'interférence* (Paris: Minuit, 1972), 70-71.
[133] M. Serres, *Le Système de Leibniz,* 75n.
[134] N. Bourbaki, *Elements of History.* See M. Serres, *Le Système de Leibniz,* 86n for a complete collection of Bourbaki's references to Leibniz.

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."[135] 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.[136]

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."[137] Local orders met and accommodated one another, but none was hierarchically superior. Auguste Comte's ladder was knocked down. Serres replaced it, not by a circle like Piaget and Queneau, but by a network, or better, several networks that intersected in several dimensions, without foundation nor 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

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135 M. Serres, *Le Système de Leibniz,* 86.
136 M. Serres, *Le Système de Leibniz,* 16; for Serres's use of graph theory, see his *Hermès I,* 11-20.

confusion was always possible. When, like 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." Notice that this parsing of knowledge into islands had been already envisaged by René Thom in his famous article that first introduced the notion of catastrophes. Identifying determinism and structural stability as the very conditions for the building of scientific theories, he wrote that "in every natural process, one first tries to isolate those areas where the process is structurally stable, . . . islets of determinism separated by zones where the process is indeterminate or structurally unstable."[138]

Was, in Serres's view, 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."[139] And there was hope that new 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

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137 M. Serres, *Le Système de Leibniz,* 16n. Serres's *thèse complémentaire,* published as *Hermès II* (Paris: Minuit, 1972), articulated this vision.
138 R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edingburgh Press, 1968): 152-166; repr. *MMM,* 13-38. Quote on p. 16.
139 M. Serres, *Hermès IV,* 11, and 39-40.

Prigogine's irreversible time and selforganizing disorder, from Thom's catastrophes, from Mandelbrot's fractals, and from the images of fluid mechanics, turbulence and chaos theory.[140] 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.[141]

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."[142] This marked the death of a Bourbakist's dream; this was also typical of a widespread rejection of abstract structures in French thought.

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[140] See Serres, *Hermès V. Le passage du Nord-Ouest* (Paris: Minuit, 1977), 99. See M. Serres, *Hermès IV,* 79ff; *Hermès V,* 40-113; and especially, *La Naissance de la physique dans le texte de Lucrèce. Fleuves et turbulences* (Paris: Minuit, 1977) and *La Genèse* (Paris: Minuit, 1982); *Genesis,* transl. G. James and J. Nielson (Ann Arbor: University of Michigan, 1995). In this last work, M. L. Assad has seen a critical tropical shift in Serres's thought. See her "Michel Serres: In Search of a Tropography," *Chaos and Order: Complex Dynamics in Literature and Science,* ed. N. Katherine Hayles (Chicago: University of Chicago Press, 1991): 278-298.
[141] M. Serres, *Genesis,* 106.
[142] M. Serres, *Genesis,* 131.

b) The Trouble with Bourbaki's Structures

Ironically, the structures of Bourbaki, which, as we have seen, once became 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."[143] 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* on set theory.[144] But, as he was the first to admit, this summary only presented definitions and propositions from a "naive" point of view, in direct opposition with the "formalist" approach that he 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, only appeared in 1957. It was the 22nd in the series![145]

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[143] N. Bourbaki, "The Architecture," 29n7.
[144] N. Bourbaki, *Éléments de mathématique,* Première partie: Structures fondamentales de l'analyse, Livre I: Théorie des ensembles – *Fascicule de résultats* (Paris: Hermann, 1939); *Theory of Sets* (Paris: Hermann and Reading: Addison-Wesley, 1968).
[145] N. Bourbaki, *Ibid.,* Chapter 1: "Description des mathématiques formelles," and 2: "Théorie des ensembles" (Paris: Hermann, 1954); *Ibid.,* Chapter 3: "Ensembles ordonnés,

Ironically, although the exact circumstance 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."[146] 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."[147] On the other, he began dealing with topics outside of Part I, issuing Books without 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."[148] 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

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cardinaux, nombres entiers" (Paris: Hermann, 1956); *Ibid.,* Chapter 4: "Structures" (Paris: Hermann, 1957).

146 B. Malgrange, "À propos d'un article de J. Dieudonné," *Gazette des mathématiciens,* 3 (1975): 35-37, 37.

147 N. Bourbaki, *Théorie des ensembles,* Chapter 4: "Structures," iii.

148 M. Zisman, "Mathématiques et axiomatique, qu'apporte de nouveau Bourbaki?," *La Pensée. Revue du rationalisme moderne,* n.s., 65 (1956): 46-54, 49n.

easy to be conceived of."[149] 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 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.[150]

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."[151] 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 treated 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

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[149] N. Bourbaki, *Théorie des ensembles,* Chapter 1: "Description des mathématiques formelles," 2.
[150] M. Chouchan, *Bourbaki,* 33.

not undermine his vision, but pointed to new syntheses to come.[152] Even the Oulipian logician Jacques Roubaud was appalled by *Theory of Sets.* He said he once participated to a committee in charge of 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."[153] 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."[154] Corry's distinction between "body of knowledge" and "image of mathematics" goes some way in explaining why Bourbaki was so powerful a symbol for practitioners of different disciplines.[155] 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

[151] J. Pouillon, "Présentation," 769; see M. Barbut, "Sur le sens du mot structure en mathématiques," *Les Temps modernes,* 22(246) (1966): 791-814, 799.
[152] J. Piaget, *Structuralism,* 27-28 and 143.
[153] Quoted in M. Chouchan, *Bourbaki,* 124.
[154] L. Corry, "Nicolas Bourbaki and the Concept of Mathematical Structure," *Synthese,* 92 (1992): 315-348, 320-321; *Modern Algebra and the Rise of Mathematical Structure* (Basel: Birkhäuser, 1996).

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 rather was 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!

c) 'Nice Visible Novelties' in Mathematical Research

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.[156] 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* offers a vivid contrast with Dieudonné's *Panorama of Pure Mathematics* published less than ten years earlier, but already at odds with current research.[157] Chapter headings included 'Mechanics and meteorology', 'Applications to biology' and to 'the sciences of man, the sciences of society

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[155] L. Corry, "Linearity and Reflexivity in the Growth of Mathematical Knowledge," *Science in Context,* 3 (1989): 409-440.
[156] Repr. in M. Chouchan, *Bourbaki,* 97.

and linguistics', etc. Where Dieudonné reserved one laconic paragraph at the end of each chapter to applications, Houzel integrated them in the body of mathematics. "This opening effort of mathematics to other sectors now seems to me essential for the survival of our science in France."[158] 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 of mathematics and science, availability of computers, renewed contacts with the Soviet Union, social demands for applications of mathematics, the popular image of science, the job market, all contributed. At least two symptoms showing that the social place of mathematics was shifting can be gathered from the *Gazette des mathématiciens,* the professional journal of the *Société mathématique de France.* First, while 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.[159] Second, a commission was created in 1979 for "the defense and illustration of mathematics" at the SMF, "in the rather vague goal of popularizing mathematics and defending it against threats we began to feel."[160]

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[157] C. Houzel, ed., *Rapport de prospective en mathématiques* (Paris: Éditions du CNRS, 1985); J. Dieudonné, *Panaorama.*
[158] C. Houzel, *Rapport de prospective,* viii.
[159] See, e.g., P. Lelong, "Questions d'actualité et de prospective." *Gazette des mathématiciens,* 2(3) (1963): 1-3; B. Malgrange, "Quelques mots sur les dernières sessions du Comité consultatif," *Gazette des mathématiciens,* 3 (1975): 32-35; J.-P. Aubin and B. Cornet, "Rapport sur la situation de la recherche mathématique en France," *Gazette des mathématiciens,* 7 (1976): 18-73; M. Berger, "Note sur le flux de survie de la recherche mathématique en France," *Gazette des mathématiciens,* 13 (1980): 5-13.
[160] J. Ferrand, "Rapport d'activité," *Gazette des mathématiciens,* 14 (1980): 29. See also D. Nordon, "Sommes-nous tous pareils?" *Gazette des mathématiciens,* 10 (1978): 65-89; *Les Mathématiques pures n'xistent pas!;* and J.-M. Lévy-Leblond and A. Jaubert, eds., *(Auto)critique de la science* (Paris: Seuil, 1975).

In particular, the view of mathematics as opening 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 at modeling 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?[161]

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.[162] 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

[161] R. Thom, *Structutal Stability and Morphogenesiss: Outline of a General Theory of Models,* transl. D. H. Fowler (Reading: Benjamin, [1972] 1975), 9. Hereafter *SSM.* For more on catastrophe theory, see Chapters III and VI.
[162] B. Mandelbrot, "Towards a Second Stage of Indeterminism in Science," *Interdisciplinary Science Review,* 12 (1987): 117-127, 117.

notable susceptibility to the *bourbachique* virus."[163] But they chose to develop their mathematics in a different direction.

Distrustful of the assumption that they only had 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."[164] 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."[165] This was an "intellectual strategy," Mandelbrot wrote, but also an issue of "raw political power."[166]

Not by 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

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[163] *La Tribu,* no. 8 (15 July 1945), 2. 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).
[164] C. Lévi-Strauss, "The Mathematics of Man." *International Social Science Bulletin,* 6(4) (1954): 581-590, 590.
[165] M. Zisman, "Mathématiques et axiomatique," 53.
[166] B. Mandelbrot, "Chaos, Bourbaki, and Poincaré," *Mathematical Intelligencer,* 11(3) (1989): 10-12, 12. See his recollections in "Benoît Mandelbrot interviewed by Anthony Barcellos," in *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson: 207-225 (Boston: Birkhäuser, 1985); and B. Mandelbrot, "Comment j'ai découvert les fractales." *La Recherche,* 17 (1986): 420-424.

collaboration with Piaget's group, in the late fifties, on a project to use information theory in linguistics.[167] Already in 1955, he explored the signification of, and innovations brought about by, cybernetics, game theory, and information theory, which he all placed under the heading of "structural theories."[168] 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.[169] And much of his interpretation of the kind of knowledge produced by catastrophe theory depended on this answer. As I show later in Chapter III, Thom's morphogenesis could best be characterized as a *dynamics of structures*.[170] 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

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[167] B. Mandelbrot, "Sur la définition abstraite de quelques degrés d'équilibre," *Études d'épistémologie génétique,* 2 (1956): 1-26; "Linguistique statistique macroscopique," *Études d'épistémologie génétique,* 3 (1957): 1-78; "Quelques problèmes de la théorie de l'observation dans le contexte des théories modernes de l'induction des statisticiens," *Études d'épistémologie génétique,* 5 (1958): 29-47.

[168] B. Mandelbrot, "L'Ingénieur en tant que stratège: théories du comportement. Une dèfinition de la cybernétique; applications linguistiques," *Revue génerale des sciences pures et appliquées et Bulletin de l'Association française pour l'avancement de la science,* 62 (1955): 278-294. In 1950, the "officer cadet [*aspirant*]" Benoît Mandelbrot prepared a report on a conference on cybernetics organized by the Royal Society in London for the French Air Force. SHAA Carton 1579.

[169] R. Thom, "Structuralism and Biology," in *Towards a Theoretical Biology,* 4, ed. C. H. Waddington (Edinburgh: University of Edinburgh Press, 1972): 68- 82, 68.

[170] See also D. Aubin, "The Catastrophe Theory of René Thom: Topology, Morphology, and Structuralism," *Growing Explanations: Historical Perspective on the Sciences of Complexity,* ed. M. Norton Wise (in preparation); on catastrophe theory, see A. Woodcock and M. Davis, *Catastrophe Theory* (New York: E. P. Dutton, 1978), and Chapter III below.

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 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."[171] 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.[172] In 1969, the physicist David Ruelle, one of Thom's colleagues, wrote, in a book on statistical mechanics, that a Bourbakist treatment of many fields of physics was a "rewarding experience."[173] 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

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[171] B. Mandelbrot, "Chaos, Bourbaki," 11.

[172] The historical path from catastrophe to chaos has to be appreciated with nuance. Although not theoretically speaking 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. This is one of the main topic of this dissertation. Fractals were later found quite useful to study and describe strange attractors.

[173] D. Ruelle, *Statistical Physics: Rigorous Results* (New York: W. A. Benjamin, 1969), viii.

limits to prediction.[174] 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 insure that the group would remain eternally a cohesive young rebel was—of course—not working. In a way, the whole enterprise had become boring.[175]

Brief, Bourbaki was getting old.

d) Catastrophes and Fractals as Cultural Connectors

I illustrated post-Bourbakist mathematics with catastrophe theory and fractal geometry, not because they were alone, as we have seen above with probability theory and applied mathematics, but because they acted most visibly as cultural connectors in 1970s France.[176] Admittedly, from the mathematicians' point of view, catastrophes and fractals may often have been perceived more as media fad than research avenues, especially because of Thom and Mandelbrot's remoteness from French students. This view was

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[174] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971): 167-192. and their "Note" in *Ibid.,* 23 (1971): 343-344; repr. Hao B.-L., ed., *Chaos II* (Singapore: World Scientific, 1990) [hereafter *Chaos II*]: 120-147; and D. Ruelle, *Turbulence, Strange Attractors, and Chaos* (Singapore: World Scientific, 1992) [hereafter *TSAC*]: 57-84.

[175] *Mathematical People,* 221.

[176] Other important cultural connectors of the late 1970s and early 1980s were cybernetics and systems theory (J. de Rosnay, *Le Macroscope. Vers une vision globale* [Paris: Seuil, 1975]; E. Morin, *La Méthode I. La Nature de la Nature* [Paris: Seuil, 1977]), self-organization inspired by Ilya Prigogine (P. Dumouchel and J.-P. Dupuy, eds., *L'Auto-Organisation. De la physique au politique* [Paris: Seuil, 1983]), and, later, deterministic chaos.

moreover boosted by the special character of Thom and Mandelbrot's books.[177] Indeed, perhaps in order to get around Bourbaki's hegemony, they published manifestos that reached beyond mathematicians and scientists.[178] This would help their wide cultural diffusion.

A channel for communication between mathematics and other cultural spheres having been established through Bourbaki, people therefore 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, condemn 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.[179]

Similarly, in *The Postmodern Condition,* Jean-François 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)

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[177] This however is rather difficult to evaluate and of little consequence for my argument. See B. Malgrange, "À propos," 36; CNRS, "Rapport de conjoncture 198," *Gazette des mathématiciens,* 20 (1982): 16-110, 93, for evaluations of the impact of catastrophe theory on mathematics.

[178] B; Mandelbrot, *Fractals: Form, Chance, and Dimension* (San Francisco: Freeman, [1975] 1977); R; Thom, *Structural Stability; Mathematical Models of Morphogenesis,* transl. W. M. Brookes and D. Rand (Chichester: Ellis Horwood, [1974] 1983).

[179] M. Serres, *Hermès V,* 99.

to which it was subordinated.[180] 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.[181]

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." Always moribund compared to the other sections of the IHÉS in mathematics and physics, this section never was closer from realization than during the 1970s. Thom finally was nominated as a professor for this section in 1980 after having gathered a group of philosophers and searchers in social sciences who worked on exploring the possibilities of applications of catastrophe theory (Scheurer, Pomian, Petitot-Cocorda, Boutot, among others). By then, Bourbaki was more or less evacuated from the discourse of the social sciences, and structuralism from mathematics. Cultural connectors remained, like catastrophes and fractals, but that would quickly withered. They had benefited from the connection established through Bourbaki. But they could not sustain it. Connections would have to follow other channels.

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[180] J.-F. Lyotard, *The Postmodern Condition: A Report on Knowledge,* transl. G; Bennington and B. Massumi (Minneapolis: University of Minnesota Press, [1979] 1984), 43.
[181] *Ibid.,* 60.

5. CONCLUSION

How to account for changes in the outlook of mathematics remains a troublesome question for historians. It is not enough to exhibit striking resonance's 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 actual effects on the fields themselves, as well as on the outside, as the Oulipo experience demonstrates.

In order to describe this cultural dynamics, I have introduced a notion of cultural 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 strengthening the successes of their respective approach 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 Mai 1968 in France, and elsewhere. Both Serres and Lyotard, just to name a few, 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 in the pursuit of pure knowledge and distance itself from forceful attempts at controlling nature and society. But for the following generation, after Mai 1968, it seemed that totalizing science and philosophy entailed a disposition for totalitarianism.[182] 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.[183] Will a detailed study of the cases of Thom, Mandelbrot, Prigogine, and French chaologists prove

[182] About the links between totalizing systems and totalitarianism, see C. Ruby, *Les Archipels de la différence. Foucault, Derrida, Deleuze, Lyotard* (Paris: Éditions du Félin, 1989).
[183] F. Lyotard, "Answering the question: What Is Postmodernism?" (transl. R. Durand) in *The Postmodern Condition,* 82.

that a kinder science could indeed be achieved? In view of the controversy that pitted Thom against Prigogine in the early 1980s, answers will hardly be univocal.[184]

[184] K. Pomian, ed., *La Querelle du déterminisme* (Paris: Gallimard, 1990).