Twenty-Five Years with Nicolas Bourbaki, 1949–1973

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The choice of dates is dictated by personal circumstances: they roughly bound the period in which I had inside knowledge of the work of Bourbaki, first through informal contacts with several members, then as a member for twenty years, until the mandatory retirement at fifty.

Being based largely on personal recollections, my account is frankly subjective. Of course, I checked my memories against the available documentation, but the latter is limited in some ways: not much of the discussions about orientation and general goals has been recorded.[1] Another member might present a different picture.

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[1] *The Archives of Bourbaki at the École Normale Supérieure, Paris, contain reports, surveys, successive drafts or counterdrafts of the chapters, remarks on those resulting from discussions, and proceedings of the Congresses, called "Tribus". Those provide mainly a record of plans, decisions, commitments for future drafts, as well as jokes, sometimes poems.*

To set the stage, I shall briefly touch upon the first fifteen years of Bourbaki. They are fairly well documented[2], and I can be brief.

In the early thirties the situation of mathematics in France at the university and research levels, the only ones of concern here, was highly unsatisfactory. World War I had essentially wiped out one generation. The upcoming young mathematicians had to rely for guidance on the previous one, including the main and illustrious protagonists of the so-called 1900 school, with strong emphasis on analysis. Little information was available about current developments abroad, in particular about the flourishing German school (Göttingen, Hamburg, Berlin), as some young French mathematicians (J. Herbrand, C. Chevalley, A. Weil, J. Leray) were discovering during visits to those centers.[3]

In 1934 A. Weil and H. Cartan were Maîtres de Conférences (the equivalent of assistant professors) at the University of Strasbourg. One main duty was, of course, the teaching of differential and integral calculus. The standard text was the *Traité d'Analyse* of E. Goursat, which they found wanting in many ways. Cartan was frequently bugging Weil with questions on how to present this material, so that at some point, to get it over with once and for all, Weil suggested they write themselves

[2] *See [2, 3, 6, 7, 8, 14].*

[3] *For this, see pp. 134–136 of [8].*

a new *Traité d'Analyse.* This suggestion was spread around, and soon a group of about ten mathematicians began to meet regularly to plan this treatise. It was soon decided that the work would be collective, without any acknowledgment of individual contributions. In summer 1935 the pen name Nicolas Bourbaki was chosen.[4]

The membership varied over the years; some people in the first group dropped out quickly, others were added, and later there was a regular process of additions and retirements. I do not intend to give a detailed account. At this point let me simply mention that the true "founding fathers", those who shaped Bourbaki and gave it much of their time and thoughts until they retired, are:

Henri Cartan
Claude Chevalley
Jean Delsarte
Jean Dieudonné
André Weil

born respectively in 1904, 1909, 1903, 1906, 1906—all former students at the École Normale Supérieure in Paris.[5]

A first question to settle was how to handle references to background material. Most existing books were found unsatisfactory. Even B. v.d. Waerden's *Moderne Algebra,* which had made a deep impression, did not seem well suited to their needs (besides being in German). Moreover, they wanted to adopt a more precise, rigorous style of exposition than had been traditionally used in France, so they decided to start from scratch and, after many discussions, divided this basic material into six "books", each consisting possibly of several volumes, namely:

I *Set Theory*
II *Algebra*
III *Topology*
IV *Functions of One Real Variable*
V *Topological Vector Spaces*
VI *Integration*

These books were to be linearly ordered: references at a given spot could only be to the previous text in the same book or to an earlier book (in the given ordering). The title "Éléments de Mathématique" was chosen in 1938. It is worth noting that they chose "Mathématique" rather than the much more usual "Mathématiques". The absence of the "s" was of course quite intentional, one way for Bourbaki to signal its belief in the unity of mathematics.

The first volumes to appear were the *Fascicle of Results on Set Theory* (1939) and then, in the forties, *Topology* and three volumes of *Algebra.*

At that time, as a student and later assistant at the E.T.H. (Swiss Federal Institute of Technology) in Zurich, I read them and learned from them, especially from *Multilinear Algebra,* for which there was no equivalent anywhere, but with some reservations. I was rather put off by the very dry style, without any concession to the reader, the apparent striving for the utmost generality, the inflexible system of internal references and the total absence of outside ones (except in Historical Notes). For many, this style of exposition represented an alarming tendency in mathematics, towards generality for its own sake, away from specific problems. Among those critics was H. Weyl, whose opinion I knew indirectly through his old friend and former colleague M. Plancherel, who concurred, at a time I was the latter's assistant.

In fall 1949 I went to Paris, having received a fellowship at the C.N.R.S. (Centre National de la Recherche Scientifique), benefiting from an exchange convention just concluded between the C.N.R.S. and the E.T.H. I quickly got acquainted with some of the senior members (H. Cartan, J. Dieudonné, L. Schwartz) and, more usefully for informal contacts, with some of the younger ones, notably Roger Godement, Pierre Samuel, Jacques Dixmier, and, most importantly, Jean-Pierre Serre, the beginning of intense mathematical discussions and a close friendship. Of course, I also attended the Bourbaki Seminar, which met three times a year, offering each time six lectures on recent developments.

Those first encounters quickly changed my vision of Bourbaki. All these people—the elder ones, of course, but also the younger ones—were very broad in their outlook. They knew so much and knew it so well. They shared an efficient way to digest mathematics, to go to the essential points, and reformulate the math in a more comprehensive and conceptual way. Even when discussing a topic more familiar to me than to them, their sharp questions often gave me the impression I had not really thought it through. That methodology was also apparent in some of the lectures at the Bourbaki seminar, such as Weil's on theta functions (Exp. 16, 1949) or Schwartz's on Kodaira's big *Annals* paper on harmonic integrals (Exp. 26, 1950). Of course, special problems were not forgotten—in fact, were the bread and but-

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[4] *See [3] for the origin of the name.*

[5] *They all contributed in an essential way. For Cartan, Chevalley, Dieudonné, and Weil I could witness it at first-hand, but not for Delsarte, who was not really active anymore when I came on board. But his importance has been repeatedly stressed to me by Weil in conversations. See also [14] and comments by Cartan, Dieudonné, Schwartz in [3, pp. 81–83]. In particular, he played an essential role in transforming into a coherent group, and maintaining it so, a collection of strong, some quite temperamental, individuals. Besides, obviously, Book IV,* Functions of one real variable, *owes much to him. Some other early members, notably Szolem Mandelbrojt and René de Possel, also contributed substantially to the work of the group in its initial stages.*

ter of most discussions. The writing of the books was obviously a different matter.

Later I was invited to attend (part of) a Bourbaki Congress and was totally bewildered. Those meetings (as a rule three per year: two of one week, one of about two) were private affairs, devoted to the books. A usual session would discuss a draft of some chapter or maybe a preliminary report on a topic under consideration for inclusion, then or later. It was read aloud line by line by a member, and anyone could at any time interrupt, comment, ask questions, or criticize. More often than not, this "discussion" turned into a chaotic shouting match. I had often noticed that Dieudonné, with his stentorian voice, his propensity for definitive statements, and extreme opinions, would automatically raise the decibel level of any conversation he would take part in. Still, I was not prepared for what I saw and heard: "Two or three monologues shouted at top voice, seemingly independently of one another" is how I briefly summarized for myself my impressions that first evening, a description not unrelated to Dieudonné's comments in [8]:

> Certain foreigners, invited as spectators to Bourbaki meetings, always come out with the impression that it is a gathering of madmen. They could not imagine how these people, shouting—some times three or four at the same time—could ever come up with something intelligent....

It was only about ten years ago, reading the text of a 1961 lecture by Weil on organization and disorganization in mathematics [13], that I realized this anarchic character, if not the shouting, was really by design. Speaking of Bourbaki, Weil said, in part (freely translated):

> ...keeping in our discussions a carefully disorganized character. In a meeting of the group, there has never been a president. Anyone speaks who wants to and everyone has the right to interrupt him....
>
> The anarchic character of these discussions has been maintained throughout the existence of the group....
>
> A good organization would have no doubt required that everyone be assigned a topic or a chapter, but the idea to do this never occurred to us....
>
> What is to be learned concretely from that experience is that any effort at organization would have ended up with a treatise like any other....

The underlying thought was apparently that really new, groundbreaking ideas were more likely to arise from confrontation than from an orderly discussion. When they did emerge, Bourbaki members would say, "the spirit has blown" ("l'esprit a soufflé"), and it is indeed a fact that it blew much more often after a "spirited" (or should I say stormy) discussion than after a quiet one.

Other rules of operation also seemed to minimize the possibility of a publication in a finite time:

Only one draft was read at a given time, and everyone was expected to take part in everything. A chapter might go through six or even more drafts. The first one was written by a specialist, but anyone might be asked to write a later one. Often this was hardly rewarding. Bourbaki could always change his mind. A draft might be torn to pieces and a new plan proposed. The next version, following those instructions, might not fare much better, and Bourbaki might opt for another approach or even decide that the former one was preferable after all, and so on, resulting sometimes in something like a periodicity of two in the successive drafts.

To slow down matters further, or so it seemed, there were no majority votes on publications: all decisions had to be unanimous, and everyone had a veto right.

However, in spite of all those hurdles, the volumes kept coming out. Why such a cumbersome process did converge was somewhat of a mystery even to the founding members (see [6, 8]), so I do not pretend to be able to fully explain it. Still, I will venture to give two reasons.

The first one was the unflinching commitment of the members, a strong belief in the worthiness of the enterprise, however distant the goals might seem to be, and the willingness to devote much time and energy to it. A typical Congress day would include three meetings, totaling about seven hours of often hard, at times tense, discussions—a rather grueling schedule. Added to this was the writing of drafts, sometimes quite long, which might take a substantial part of several weeks or even months, with the prospect of seeing the outcome heavily criticized, if not dismissed, or even summarily rejected after reading of at most a few pages, or left in abeyance ("put into the refrigerator"). Many, even if read with interest, did not lead to any publication. As an example, the pièce de résistance of the second Congress I attended was a manuscript by Weil of over 260 pages on manifolds and Lie groups, titled "Brouillon de calcul infinitésimal", based on the idea of "nearby points" ("points proches"), a generalization of Ehresmann's jets. This was followed later by about 150 pages of elaboration by Godement, but Bourbaki never published anything on nearby points.

On the other hand, whatever was accepted would be incorporated without any credit to the author. Altogether, a truly unselfish, anonymous, demanding work by people striving to give the best possible exposition of basic mathematics, moved by their belief in its unity and ultimate simplicity.

My second reason is the superhuman efficiency of Dieudonné. Although I did not try to count pages, I would expect that he wrote more than any two or three other members combined. For about twenty-five years he would routinely start his day (maybe after an hour of piano playing) by writing a few pages for Bourbaki. In particular, but by far not exclusively, he took care of the final drafts, exercises, and preparation for the printer of all the volumes (about thirty) which appeared while he was a member and even slightly beyond.

This no doubt accounts to a large extent for the uniformity of style of the volumes, frustrating any effort to try to individualize one contribution or the other. But this was not really Dieudonné's style, rather the one he had adopted for Bourbaki. Nor was it the personal one of other Bourbaki members, except for Chevalley. Even to Bourbaki he seemed sometimes too austere, and a draft of his might be rejected as being "too abstract". The description "severely dehumanized book...", given by Weil in his review of a book by Chevalley [12, p. 397] is one many people would have applied to Bourbaki itself. Another factor contributing to this impersonal, not user-friendly presentation[6] was the very process by which the final texts were arrived at. Sometimes a heuristic remark, to help the reader, would find its way into a draft. While reading it, in this or some later version, its wording would be scrutinized, found to be too vague, ambiguous, impossible to make precise in a few words, and then, almost invariably, thrown out.

As a by-product, so to say, the activity within Bourbaki was a tremendous education, a unique training ground, obviously a main source of the breadth and sharpness of understanding I had been struck by in my first discussions with Bourbaki members.

The requirement to be interested in all topics clearly led to a broadening of horizon, maybe not so much for Weil, who, it was generally agreed, had the whole plan in his mind almost from the start, or for Chevalley, but for most other members, as was acknowledged in particular by Cartan [7, p. xix]:

> This work in common with men of very different characters, with a strong personality, moved by a common requirement of perfection, has taught me a lot, and I owe to these friends a great part of my mathematical culture.[7]

and by Dieudonné [8, pp.143-44]:

> In my personal experience, I believe that if I had not been submitted to this obligation to draft questions I did not know a thing about, and to manage to pull through, I should never have done a quarter or even a tenth of the mathematics I have done.

But the education of members was not a goal per se. Rather, it was forced by one of the mottoes of Bourbaki: "The control of the specialists by the nonspecialists". Contrary to my early impressions in Zurich, related earlier, the aim of the treatise was not the utmost generality in itself, but rather the most efficient one, the one most likely to fill the needs of potential users in various areas. Refinements of theorems which seemed mainly to titillate specialists, without appearing to increase substantially the range of applications, were often discarded. Of course, later developments might show that Bourbaki had not made the optimal choice.[8] Nevertheless, this was a guiding principle.

Besides, many discussions took place outside the sessions about individual research or current developments. Altogether, Bourbaki represented an awesome amount of knowledge at the cutting edge which was freely exchanged.

This made it obvious that for Bourbaki current research and the writing of the "Éléments" were very different, almost disjoint, activities. Of course, the latter was meant to supply foundations for the former, and the dogmatic style, going from the general to the special, was best suited for that purpose (see [5]). However, the "Éléments" were not meant to stimulate, suggest, or be a blueprint for, research (as stressed in [8, p. 144]). Sometimes I have wondered whether a warning should not have been included in the "Mode d'emploi".

All this bore fruit, and the fifties was a period of spreading influence of Bourbaki, both by the treatise and the research of members. Remember in particular the so-called French explosion in algebraic topology, the coherent sheaves in analytic geometry, then in algebraic geometry over $\mathbb{C}$, later in the abstract case, and homological algebra. Although very much algebraic, these developments

[6]*Called "abstract, mercilessly abstract" by E. Artin in his review of* Algebra *[1], adding however "...the reader who can overcome the initial difficulties will be richly rewarded for his efforts by deeper insights and fuller understanding" (p. 479).*

[7]*"Ce travail en commun avec des hommes de caractères très divers, à la forte personnalité, mus par une commune exigence de perfection, m'a beaucoup appris, et je dois à ces amis une grande partie de ma culture mathématique."*

[8]*For instance, the emphasis on locally compact spaces in* Integration, *on which P. Halmos had expressed strong reservations in his review [11], indeed did not address the needs of probability theory, and this led to the addition of a chapter (IX) to* Integration.

also reached analysis, via Schwartz's theory of distributions and the work of his students B. Malgrange and J.-L. Lions on PDE. Early in 1955 A. Weinstein, a "hard analyst", had told me he felt safe from Bourbaki in his area. But less than two years later he was inviting Malgrange and Lions to his institute at the University of Maryland.

I am not claiming at all that all these developments were solely due to Bourbaki. After all, the tremendous advances in topology had their origin in Leray's work, and R. Thom was a main contributor. Also, K. Kodaira, D. Spencer, and F. Hirzebruch had had a decisive role in the applications of sheaf theory to complex algebraic geometry, but undeniably the Bourbaki outlook and methodology were playing a major role. This was recognized early on by H. Weyl in spite of the critical comments mentioned earlier. Once R. Bott told me he had heard negative remarks on Bourbaki by H. Weyl in 1949 (similar to those I knew about), but by 1952 the latter said to him, "I take it all back." Others, however (like W. Hurewicz, in a conversation in 1952), would assert that all that had nothing to do with Bourbaki, only that they were strong mathematicians. Of course, the latter was true, but the influence of Bourbaki on one's work and vision of mathematics was obvious to many in my generation. For us H. Cartan was the most striking illustration, almost an incarnation, of Bourbaki. He was amazingly productive, in spite of having many administrative and teaching duties at the École Normale Supérieure. All his work (in topology, several complex variables, Eilenberg-MacLane spaces, earlier in potential theory (with J. Deny), or harmonic analysis on locally compact abelian groups (with R. Godement)) did not seem to involve brand new, groundbreaking ideas. Rather, in a true Bourbaki approach, it consisted of a succession of natural lemmas, and all of a sudden the big theorems followed. Once, with Serre, I was commenting on Cartan's output, to which he replied, "Oh, well, twenty years of messing around with Bourbaki, that's all." Of course, he knew there was much more to it, but this remark expressed well how we felt Cartan exemplified Bourbaki's approach and how fruitful the latter was. At the time Cartan's influence through his seminar, papers, and teaching was broadly felt. Speaking of his generation, R. Bott said of him, "He has been truly our teacher," at the colloquium in honor of Cartan's seventieth year [4].

The fifties also saw the emergence of someone who was even more of an incarnation of Bourbaki in his quest for the most powerful, most general, and most basic—namely, Alexander Grothendieck. His first research interests, from 1949 on, were in functional analysis. He quickly made mincemeat of many problems on topological vector spaces put to him by Dieudonné and Schwartz and proceeded to establish a far-reaching theory. Then he turned his attention to algebraic topology, analytic and algebraic geometry and soon came up with a version of the Riemann-Roch theorem that took everyone by surprise, already by its formulation, steeped in functorial thinking, way ahead of anyone else. As major as it was, it turned out to be just the beginning of his fundamental work in algebraic geometry.

The fifties was thus outwardly a time of great success for Bourbaki. However, in contrast, it was inwardly one of considerable difficulties, verging on a crisis.

Of course there were some grumblings against Bourbaki's influence. We had witnessed progress in, and a unification of, a big chunk of mathematics, chiefly through rather sophisticated (at the time), essentially algebraic methods. The most successful lecturers in Paris were Cartan and Serre, who had a considerable following. The mathematical climate was not favorable to mathematicians with a different temperament, a different approach. This was indeed unfortunate, but could hardly be held against Bourbaki members, who did not force anybody to carry on research in their way.[9]

The difficulties I want to discuss were of a different, internal nature, partly engineered by the very success of Bourbaki, tied up with the "second part", i.e., the treatise beyond the first six books. In the fifties these were essentially finished, and it was understood the main energies of Bourbaki would henceforth concentrate on the sequel; it had been in the mind of Bourbaki very early on (after all, there was still no *Traité d'Analyse*). Already in September 1940 (Tribu No. 3), Dieudonné had outlined a grandiose plan in twenty-seven books, encompassing most of mathematics. More modest ones, still reaching beyond the "Éléments", also usually by Dieudonné, would regularly conclude the Congresses. Also, many reports on and drafts of future chapters had already been written. However, mathematics had grown enormously,

[9] *In this connection, I would like to point out that the subtitle* Le choix bourbachique *in [9] is extremely misleading. Bourbaki members gave many talks at the seminar and had much input in the choice of the lectures, so it is fair to say that most topics discussed were of interest to at least some members, but many equally interesting ones turned out to be left out, if only because no suitable speaker appeared to be available. So the seminar is by no means to be viewed as a concerted effort by Bourbaki to present a comprehensive survey of all recent research in mathematics of interest to him and a ranking of contributions. Such conclusions by Dieudonné are solely his own. He says that much in his introduction, p. xi, but it seems worth repeating. Of course, like most mathematicians, Bourbaki members had strong likes and dislikes, but it never occurred to them to erect them as absolute judgments by Bourbaki, as a body. Even when it came to his strong belief in the underlying unity of mathematics, Bourbaki preferred to display it by action rather than by proclamation.*

the mathematical landscape had changed considerably, in part through the work of Bourbaki, and it became clear we could not go on simply following the traditional pattern. Although this had not been intended, the founding members had often carried a greater weight on basic decisions, but they were now retiring[10] and the primary responsibility was shifting to younger members. Some basic principles had to be reexamined.

One, for instance, was the linear ordering and the system of references. We were aiming at more special topics. To keep a strict linear ordering might postpone unduly the writing of some volumes. Also, when that course had been adopted at the beginning, there was indeed a dearth of suitable references. But Bourbaki had caught on, some new books were rather close to Bourbaki in style, and some members were publishing others. To ignore them might lead to a considerable duplication and waste of effort. If we did not, how could we take them into account without destroying the autonomous character of the work? Another traditional basic tenet was that everyone should be interested in everything. As meritorious as it was to adhere to, it had been comparatively easy while writing the "Éléments", which consist of basic mathematics, part of the baggage of most professional mathematicians. It might, however, be harder to implement it when dealing with more specialized topics closer to the frontier. The prospect of dividing up, of entrusting the primary responsibility of a book to a subset of Bourbaki, was lurking but was not one we would adopt lightly. These questions and others were debated, though not conclusively for a while. There were more questions than answers. In short, two tendencies, two approaches, emerged: one (let me call it the idealistic one) to go on building up broad foundations in an autonomous way, in the tradition of Bourbaki; the other, more pragmatic, to get to the topics we felt we could handle, even if the foundations had not been thoroughly laid out in the optimal generality.

Rather than remain at the level of vague generalities, I would like to illustrate this dilemma by an example.

At some point a draft on elementary sheaf theory was produced. It was meant to supply basic background material in algebraic topology, fibre bundles, differential manifolds, analytic and algebraic geometry. However, Grothendieck objected[11]: we had to be more systematic and provide first foundations for this topic itself. His counterproposal was to have as the next two books:

Book VII: *Homological Algebra*
Book VIII: *Elementary Topology*

the latter to be tentatively subdivided into:

Chap. I: Topological categories, local categories, gluing of local categories, sheaves
Chap. II: H^1 with coefficients in a sheaf
Chap. III: H^n and spectral sequences
Chap. IV: Coverings

to be followed by

Book IX: *Manifolds*

which had already been planned.

He also added a rather detailed plan for the chapter on sheaves that I shall not go into.

This was surely in the spirit of Bourbaki. To oppose it would have been a bit like arguing against motherhood, so it had to be given a hearing. Grothendieck lost no time and presented to the next Congress, about three months later, two drafts:

Chap. 0: Preliminaries to the book on manifolds. Categories of manifolds, 98 pages

Chap. I: Differentiable manifolds, The differential formalism, 164 pages

and warned that much more algebra would be needed, e.g., hyperalgebras. As was often the case with Grothendieck's papers, they were at points discouragingly general, but at others rich in ideas and insights. However, it was rather clear that if we followed that route, we would be bogged down with foundations for many years, with a very uncertain outcome. Conceived so broadly, his plan aimed at supplying foundations not just for existing mathematics, as had been the case for the "Éléments", but also for future developments to the extent they could be foreseen. If the label "Chapter 0" was any indication, one could fear that the numbering might go both ways, Chapters -1, -2,...being needed to give foundations to foundations, etc.

On the other hand, many members thought we might achieve more tangible goals in a finite time, not so fundamental maybe, but still worthwhile. There were quite a number of areas (algebraic topology, manifolds, Lie groups, differential geometry, distributions, commutative algebra, algebraic number theory, to name a few) in which they felt the Bourbaki approach might produce useful expositions, without needing such an extensive foundational basis as a prerequisite.

The ideal solution would have been to go both ways, but this exceeded by far our possibilities. Choices had to be made, but which ones? The

[10] *It had been apparently agreed early on that the retirement age would be fifty (at the latest). However, when the time came to implement that rule, from 1953 on, there was little mention of it until 1956, when Weil wrote a letter to Bourbaki announcing his retirement. From then on it has been strictly followed.*

[11] *At the March 1957 Congress, later called "Congress of the inflexible functor".*

question was not answered for some time, resulting in a sort of paralysis. A way out was finally arrived at a year later: namely, to write a fascicle of results on differential and analytic manifolds, thus bypassing, at least provisorily, the problem of foundations, at any rate for the main topics we had in mind. After all, as far as manifolds were concerned, we knew what kind of basic material was needed. To state what was required and prove it for ourselves was quite feasible (and was indeed carried out rather quickly).

This decision lifted a stumbling block, and we could now set plans for a series of books which we hoped would essentially include commutative algebra, algebraic geometry, Lie groups, global and functional analysis, algebraic number theory, and automorphic forms.

Again, this was too ambitious. Still, in the next fifteen years or so a sizable number of volumes appeared:

Commutative Algebra (9 chapters)
Lie Groups and Lie Algebras (9 chapters)
Spectral Theory (2 chapters)

besides preliminary drafts for several others.

In 1958 a decision had also been made to solve in principle a problem which had been plaguing us for quite a while: additions to the "Éléments". On occasions, while writing a new chapter, we would realize that some complement to one of the first six books was in order. How to handle this? Sometimes, if a volume was out of print, it was possible to include these complements in a revised edition. If not, one could conceivably add an appendix to the new chapter. But this threatened to create a lot of confusion in the references. In 1958 it was resolved to revise the "Éléments" and publish a "final" edition, not to be tampered with for at least fifteen years. Unfortunately, it took more time and effort than anticipated. It is in fact not quite finished as of now, and (I feel) it slowed down progress in the more innovative parts of the treatise. But it was certainly in the logic of Bourbaki and hardly avoidable.

Of the three books listed above, *Commutative Algebra* was obviously well within Bourbaki's purview; it could, and did in fact, proceed independently of the resolution of the dilemma we had faced. But the fascicle of results on manifolds was an essential prerequisite for the book on Lie groups and Lie algebras. The latter also shows that the more pragmatic way could lead to useful work. A good example is provided by Chapters 4, 5, and 6 on reflection groups and root systems.

It started with a draft of about 70 pages on root systems. The author was almost apologetic in presenting to Bourbaki such a technical and special topic, but asserted this would be justified later by many applications. When the next draft, of some 130 pages, was submitted, one member remarked that it was all right, but really Bourbaki was spending too much time on such a minor topic, and others acquiesced. Well, the final outcome is well known: 288 pages, one of the most successful books by Bourbaki. It is a truly collective work, involving very actively about seven of us, none of whom could have written it by himself. Bourbaki had developed a strong technique to elicit a collaboration on a given topic by specialists and people with related interests looking at it from different angles. My feeling (not unanimously shared) is that we might have produced more books of that type but that the inconclusive discussions and controversies, and the difficulties in mapping out a clear plan of activity had created a loss of momentum from which Bourbaki never fully recovered. There is indeed a tremendous amount of unused material in Bourbaki's archives.

This approach was less ambitious than the Grothendieck plan. Whether the latter would have been successful, had we gone fully in that direction, seems unlikely to me, but is not ruled out. The development of mathematics does not seem to have gone that way, but implementation of that plan might have influenced its course. Who knows?

Of course, Bourbaki has not realized all its dreams or reached all of its goals by far. Enough was carried out, it seems to me, to have a lasting impact on mathematics by fostering a global vision of mathematics and of its basic unity and also by the style of exposition and choice of notation, but as an interested party I am not the one to express a judgment.

What remains most vividly in my mind is the unselfish collaboration over many years of mathematicians with diverse personalities toward a common goal, a truly unique experience, maybe a unique occurrence in the history of mathematics. The underlying commitment and obligations were assumed as a matter of course, not even talked about, a fact which seems to me more and more astonishing, almost unreal, as these events recede into the past.

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