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Pierre Cartier (1932-2024)

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Pierre Cartier was a major figure in French mathematics in the second half of the 20th century, but he also embodied, for many of us, a certain scientific ideal that went far beyond mathematics per se and manifested itself, among other things, in his commitment to the history and philosophy of mathematics. It was a commitment that stemmed from mathematics, its practice, in keeping with a French tradition which, from Descartes and Pascal to Poincaré, has never completely dissociated the exercise of scientific thought from a reflection on science, the conditions under which it is possible, and its history.

For those who knew him, Pierre was also someone who loved to talk and share: his views on mathematics, his ideas, but also an infinite number of anecdotes drawn from the encounters and events that had punctuated his life. As his life had been largely organised around mathematics and the mathematical community, we learned from him a parallel history of recent mathematics, with a few key figures such as Bourbaki, Dieudonné and Grothendieck. His stories were always jovial and full of sympathy and empathy. True to the spirit of storytelling, they also often had a more or less direct moral dimension. A minor flaw in a great mathematician gave him a human dimension, while an account of a visit to the Vatican during an ecumenical symposium on modern science gave rise to amusing reflections on the Roman Church, its decorum and traditions...

As a master and a friend, I would like to pay tribute to him here, but a tribute that is not confined to a few historical facts and anecdotes, or to an account of his work, but which, more deeply and perhaps more faithfully, also addresses the exercise of thought and scientific work as he conceived it. At a time when mathematical practice and its philosophy are being examined with interest, it will no doubt be of interest to glimpse how this practice can sometimes take forms that go well beyond the conventional figures of mathematical activity and involve an entire life, in all its many dimensions, in the service of science.

§ 1. — The mathematician.

Pierre's passion for mathematics began at a very early age, and was first nourished by adolescent reading of works bought on trips to Paris at the Librairie Jacques Gabay, a publisher of scientific reference texts. This passion, for mathematics and for reading them, never left him.

A great mathematician, his name will remain attached to fundamental objects, theories, concepts and results: Cartier divisors in algebraic geometry, Cartier operation in differential calculus on algebraic varieties, formal group theory, typical curves, the notion of coalgebra and Cartier duality, structure theorems for Hopf algebras, etc.

His scientific career was rich and varied: algebraic geometry and group theory at first; combinatorics and probability, during his time in Strasbourg (Cartier-Foata theory of monoids, Baxter algebras, etc.); theoretical physics and quantum groups; multizeta numbers and motives. Pierre loved mathematical adventures, but they were never undertaken haphazardly and were always guided by his in-depth knowledge of the discipline and its history, which led him in each case in the directions it suggested.

This is not the place to go into more detail about his mathematical work. Other contexts will be more appropriate, and I shall only relate one anecdote. I only learned of it fairly recently, when we were writing a book on classical Hopf algebras, which aims to give an account of the main results, from the beginnings of the theory, to which Cartier had made a decisive contribution, right up to the recent period. A fundamental theorem of the theory, due to Cartier and Gabriel, describes the generic structure of an important family of algebras of this type (in this case cocommutative and over the field of complex numbers). This is an important result, the statement of which is of little importance here, but which affirms that their behaviour is the one expected from the point of view of group theory (technically, they are always obtained from group algebras and enveloping algebras).

This theorem has a rather confused history in the literature and in the community, a confusion that is explained when one knows its (unwritten and little-known) genesis. Cartier and Gabriel had obtained the theorem in the autumn of 1962, in the context of the Séminaire de Géométrie algébrique du Bois Marie, organised by Grothendieck and Demazure at the Institut des Hautes Etudes Scientifiques (IHES) and later published as *SGA 3, Schémas en groupes*. However, Cartier and Gabriel's presentation and result do not appear in the published version. This was because Grothendieck had opposed their publication, deeming the result uninteresting because it was too particular: it assumes the base field to be algebraically closed of characteristic zero, a restriction that was prohibitive in Grothendieck's mind. The proof of the theorem, which had become "folklore", was not published until much later, by Dieudonné, in his *Introduction to the theory of formal groups*, in 1973.

The anecdote is significant on several levels. First of all, it shows how a mathematician of genius (Grothendieck) can be misguided in his value judgements, no doubt obsessed by his own agenda. As readers of his autobiographical account, *Récoltes et Semailles* will know, this later made him insensitive to the interest and beauty of the work on Weil's conjectures by his favorite 'disciple', Pierre Deligne. Secondly, it shows that it is not always easy to recognise the value of an idea or a result straight away: you still have to know how to put them into the right context, which can require a certain mathematical culture. Hence the sometimes rather random results of the process of selecting articles for publication, a phenomenon that is in no way specific to mathematics. Finally, it shows how difficult it is to establish a reliable account of the genesis of a mathematical idea, even in a fairly recent context and under very favourable conditions of visibility (those of the Seminar organised by Grothendieck at the IHES). This is because many mathematical ideas circulated largely orally at a time when *publish or perish* was much less topical, and historians are often left with no usable trace of this circulation.

§ 2. — The question of culture.

One of the most striking features of Cartier's scientific personality was the breadth of his culture, driven and nurtured by a universal curiosity, and aided by an exceptional memory, of which I know of no equivalent, either in terms of theoretical content or at the more common level of factual and historical memory. Make no mistake about it: it's a fairly rare feature in the world of professional mathematicians to display such curiosity and theoretical openness. There are various reasons for this, including (and increasingly so) multiple academic and administrative obligations and a structural lack of time available to think freely, but also, more simply, personal scientific interests and objectives which, for the sake of efficiency, force us to concentrate on a specific theme.

Alexandre Grothendieck is an excellent illustration, as the publication of *Récoltes et semailles* has made his way of working and thinking familiar to the general public. He read little, and was only interested in his own research. He analyses the reasons for this in *Récoltes et semailles*, in passages that are very enlightening on the psychological motivations of scientific activity.

Cartier's example goes in the opposite direction, but is just as exceptional in its own way. It encourages philosophers of mathematics to reflect on a point that is all too neglected by the discipline, namely the question of culture. Pierre once told me how many books he had in his library, most of them on mathematics: several thousand. But quantity is not everything: the essence of any culture lies in the ways in which it is appropriated. Cartier - and I can testify to this, having been close to him for almost 40 years - always had a personal idea and understanding of the materials, often more original and richer than that of the experts on the subject, because he drew on the comparisons and analogies that his global vision of mathematics and his technical and conceptual knowledge of its history enabled him to establish.

This relationship with mathematical culture had yet another feature: that of humanism. Extensive knowledge can be acquired for the sake of efficiency. This was not the case with Pierre. Curiosity, theoretical interest and a taste for learning were paramount, almost in the form of an intellectual game. Knowledge and discovery came later, as an afterthought. It was this relationship with knowledge that immediately drew me to him, to his atypical approach to mathematics, nourished by reading, the excitement of playing with concepts and analogies rather than the desire to solve this or that problem, this or that conjecture.

As for the question of what a 'scientific culture' is and what it should or must be, this is a difficult one, which is no longer posed today in the same terms as it was in the 1950s. Too much quantitative progress has been made, too many fundamental disciplines have developed since then, forming mathematical continents in their own right. The question also differs considerably depending on whether it is asked about the general public, and in particular of the scientific objectives of general education, or about the scientific community.

Reflecting on Cartier's example, three fundamental features seem to me to characterise what might define the mathematical culture of a mathematician at a time when the overwhelming multiplication of theories and knowledge makes an aspiration to universal knowledge illusory:

- Relationship with written texts, time spent reading;
- A dimension of openness, freeness, and the pleasure of learning;
- Finally, and much more problematically, there is the question of the choice of themes and authors. The raison d'être of this last component is that a culture is meant to be shared, and sharing is only possible on the basis of common references.

A recurring question that runs through the history and philosophy of mathematics is its usefulness for amateur or professional mathematicians. It may have a role to play here, in the problematic process of identifying what a mathematical culture might be today, by helping to identify and select some major guiding ideas, the historical moments in which they were formed, and the ways in which they have developed.

This, it seems to me, is one of the profound reasons why the Bourbakist adventure, in which Cartier participated and which I will discuss later, insisted on the historical dimension of mathematical knowledge: not because this history has any importance as such (mathematical content is fundamentally timeless), but because it structures the dynamics of the constitution of concepts, their organisation, and the way in which they participate in shared knowledge.

§ 3. — Bourbaki and structuralism.

With Cartier disappeared a certain way of doing mathematics, and of being able to do it that way. This has already been said about many other mathematicians, but he was undoubtedly the last truly universal mathematician. Because of his immense culture, his exceptional memory, but also - and this is irreplaceable and impossible to repeat - because he lived through and followed the day-to-day development of mathematics from the 1950s onwards.

From that point of view, his involvement with the Bourbaki group was extremely important, for several complementary reasons. Cartier was Jean Dieudonné's successor as Bourbaki secretary. In addition to taking part to the writing, he was responsible for monitoring and coordinating publications, including the final proofreading of the volumes published by the group. He recounted how, in the early years, Dieudonné, who had had to officially leave the group because he had reached the age limit (50) for membership, still kept an eye on his work, protesting inordinately (it was his way of doing things, gruff, excessive and friendly) at the slightest typographical error missed during proofreading.

In particular, Cartier claimed to have a precise and global vision of the Bourbakist corpus. From experience, this had a profound effect on the way he did mathematics. He would often say to me, when we were discussing technical points or choices of conventions: "Bourbaki said..., Bourbaki wrote..., Bourbaki chose to...", and sometimes also: "Bourbaki would..., Bourbaki would say that...". In each case, he was expressing a personal point of view on the question, but one that was transcended by the implicit validation given by his inclusion in, and approval by a collective project. Listening to him, one could also understand the level of detail in the debates that had taken place within the group when a particular approach, method of demonstration or terminology had to be favoured.

I'll always remember his excitement one day when he read some notes I had written for the book we were writing together. It was a point of duality, with no technical difficulties, but I had stumbled over a choice of wording and ended up adopting a convention, not necessarily the most obvious, but the one that best suited the demonstrations in the chapter. Pierre was almost euphoric, telling me: "But of course, that's what we have to do, Bourbaki made a mistake". I was struck by the importance he attached to these choices of presentation, down to the smallest detail and almost to the level of aesthetics.

Beyond these questions, Pierre has always embodied for me the best ideal of Bourbakism: a certain generosity of intelligence, the search for the right and balanced formulation of theories, without pedantry or useless formalism. And above all, the systematic use of a structural approach that favours universal explanations that cut across the particular mathematical disciplines. This is the only approach that can guarantee the unity of mathematics in the long term.

Structuralism comes in many forms. Grothendieck's structuralism was profoundly categorical, for example. Cartier's structuralism was, it seems to me, rather algebraic, both in the choice of objects he was interested in and in the choice of methods adopted to study them.

The philosophy of mathematics has taken a great interest in structuralism and even today often traces it back, in its own field, to an article by Benacerraf in the mid-1960s, while attributing its modern formulation to more recent authors such as S. Shapiro and G. Hellman. For anyone familiar with the texts of Bourbaki and his members from the 1940s onwards, and with the entire development of mathematics in the second half of the twentieth century, this is historical nonsense, that can only be justified by a gross ignorance of the sources.

Having said that, and more radically, this ignores the fact that structuralism is much more than a theory about mathematics (or about mathematical objects when one is mainly interested in ontology). For Cartier, but also for Cartan, Dieudonné, Grothendieck and others, whether they were members of the Bourbaki group or influenced by its ideas, structuralism was first and foremost a way of practicing mathematics on a daily basis, in the very working of problems and concepts. It is a conceptual requirement that is not satisfied with a result or a demonstration if it does not allow access to the ultimate core of meaning that 'mathematical flair' (and experience) suggests is immanently present beyond the formulas.

§ 4. — The Bourbaki seminar.

It was perhaps with the Bourbaki seminar that Cartier's structural quest best manifested its profound nature. The Bourbaki seminar has long been a veritable institution. It was there that the most recent and important mathematical results were supposed to be presented. In practice, this was true, at least for the most part, in the more theoretical disciplines (algebra, algebraic geometry, number theory, algebraic topology, etc.), and much less true — if not downright false — for the applied disciplines or those regarded with condescension by the Bourbaki group (combinatorics, probability, etc.). Dieudonné was particularly known for his hasty and sometimes very aggressive judgements about mathematical disciplines, their interest, or the relationship between mathematics and physics.

Cartier often emphasised the role he played in broadening the group's interests, particularly with respect to probability and mathematical physics. In fact, he gave an incredible number of talks at the seminar: according to his bibliography on the IHES website, he gave around 40 talks on a wide variety of subjects.

His presentations — I haven't read them all, but I have read many — are always very interesting. They don't just give an account of a result, but more often than not enrich it with a perspective that is both historical and programmatic. I've always thought that this work was quite close to what could be a way of carrying out the philosophy of mathematics: giving an account of the profound meaning of scientific advances, beyond their mere content.

In this connection, I would like to tell another anecdote, which is interesting for several reasons: it tells us something about the practice of mathematics, about the way institutions work, and about the ups and downs of recognising the legitimacy of a particular research topic. I got it from a former colleague at the University of Nice, Francine Diener, who was close to Pierre, whom she had known since her childhood in Strasbourg.

Non-standard analysis, which is hardly talked about any more, has long had an ambiguous, almost sulphurous reputation in a large part of the community, but also enthusiastic supporters. Francine, who was finishing her doctoral thesis on the subject, met Pierre at the beginning of the summer and tried to convince him of its interest. At first very negative and sceptical, and after trying to try to deter her to go on in that direction, he was finally persuaded to read a few texts. He quickly became interested and in November 1981, just a few months later, gave a talk at the Bourbaki seminar entitled *Perturbations singulières des équations différentielles ordinaires et analyse non-standard* (*Singular perturbations of ordinary differential equations and non-standard analysis*). Cartier's paper, which was subsequently studied at the other major seminar of the time — the Gelfand seminar in the USSR —, and the Bourbaki recognition that went with it, helped to give the subject a credibility that it lacked in the community.

§ 5. — **The death of Bourbaki.**

Cartier had a form of non-conformism and a deep taste for intellectual freedom that was evident in his open-minded approach to mathematics, but was also found in his more concrete positions. He is famous in the community for having announced in 1998 the 'death of Bourbaki'. The interview he gave that year to Sylvestre Huet for the French daily newspaper *Libération* is interesting and also sheds light on his intellectual and scientific journey. The article begins by looking back at the subversive nature of the Bourbakist project in its early days: against a certain French practice of mathematics in the first half of the 20th century, academic constraints and establishment.

Elected to the Académie des Sciences at the initiative of Henri Cartan, Cartier had refused his election: it had been an act of loyalty to the rejection of institutional constraints that the first Bourbaki had embodied for him and that he associated with a certain idea of mathematics, quite incompatible with all that the Académie embodied of conformism and obsolescence. He was also aware that the times and the spirit of the group had gradually changed: although he himself had refused to join, "many of yesterday's rebels had donned the green suit" [green is the color of the academician's suit]. "These [same] rebels, great lovers of derision [actors in a kind of luxury anarchism, who had themselves become members of the establishment] had become its targets".

In the same interview, he associated the decline of Bourbaki and its 'death' with Grothendieck's departure. Cartier often spoke of him. They had been close and he was saddened to see his evolution towards an increasingly marked form of unreason in the years following the publication of *Récoltes et semailles.* I remember how sad he was when he received a letter from Grothendieck announcing that he had proof of the existence of the devil in the fact that the speed of light was not exactly 300,000 km per second. What affected Cartier, I think, was of course Grothendieck's psychological evolution, which had made him give up trying to see him again, but also, more subtly, his betrayal of the criteria of scientificity (in this case the fact of not taking into account and not understanding the conventional character of metrology).

To come back to Bourbaki, when Cartier announced his 'death', he was also trying to give an account of a profound change that had taken place within mathematics itself. The discussions we had in the 1990s had helped to give rise to the project for my book on *Contemporary Mathematical Thinking*, in which I addressed the fate of structuralism. At the time, my analysis of its limitations was largely conditioned by ontological arguments. I had growing doubts about the universality of the structural method and its ability to address all mathematical phenomena.

Pierre had been deeply impressed by Drinfeld's work, which he had begun to read shortly before the latter was awarded the Fields Medal in 1990. He saw in it a different way of doing mathematics, one that was highly creative and influenced by mathematical physics. This led him to work himself, from those years onwards, on quantum groups — Hopf algebras of a different type from those he had studied in his youth. In retrospect, it seems to me that, remaining faithful to his fundamental conception of mathematics, he saw and experienced this development more as a new way of practicing structuralism than as going beyond it. For example, the spirit of the working group on the subject that he had organised at the Ecole Normale Supérieure at the time remained extremely structuralist!

§ 6. — History and philosophy of mathematics.

Pierre was also one of the leading figures in the philosophy of mathematics in France, animating until the end the Philosophy of Mathematics Seminar at the Ecole Normale Supérieure. The content of the seminar was unique in the international panorama of the field in that it focused on actual mathematics, the history and evolution of concepts, including their technical dimension and complexity. Professional mathematicians found a place where they could reflect on their discipline and talk about it.

All these features of the seminar also characterise the texts on the history and philosophy of mathematics that he wrote, always driven by a concern for balance between three dimensions of mathematics: its historicity; the conceptual character of its content; and its epistemological dimension.

I have already mentioned that his Bourbaki seminars intrinsically had a historical and epistemological dimension, but this was often also the case for the countless other seminars he gave, where he always sought to contextualise the results and open up perspectives.

I'll take a concrete example as an illustration: one of his best-known texts, from 1998, *La folle journée, de Grothendieck à Connes et Kontsevich. Evolution of the concepts of space and symmetry.* In this beautifully written, highly literary work, Cartier begins with a biography of Grothendieck, with a factual and theoretical dimension, not forgetting his activist commitments. It is a balanced analysis that does not forget to underline, alongside many remarkable elements, the weaknesses of his character and his fundamental contradictions.

The article then develops into an analysis of Grothendieckian conceptions of space and symmetry. The article will seem singular to anyone unfamiliar with Cartier, his culture and the originality of his views. The analysis immediately combines history, philosophy and mathematics: Leibnizian monads, Newtonian conception of space, Bourbakist set theory, Mach's conceptions, Einstein's theory of gravitation. The article, which I won't repeat or analyse in detail here, then continues in a constant juxtaposition of complementary levels of analysis, historical moments, and points of view that are sometimes opposed but which come together in a form of Hegelian *Aufhebung*. Mathematics, in its technical and conceptual dimensions, plays a leading role, and the article does not conclude without opening up new perspectives. There are very few examples in recent times of texts of this nature and level.

Pierre's philosophy of mathematics is singular, attractive and enriching for both the mathematician and the philosopher; in it, the study of historical and technical evolution is not an end in itself, but is placed at the service of thought and understanding of scientific progress in all its topicality.

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