Grothendieck: The Myth of a Break

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Ask the man in the street about Alexander Grothendieck, and he will most likely answer you: "Alexander Grothendieck was a mathematical genius who, at the age of forty, became mad, abandoned mathematics and developed a mystic delirium." If that man happens to be a mathematician, he will probably add: "Before he left the stage he drew attention to himself with some leftist extravagance and the writing of a long, delusional pamphlet in which he 'settled his accounts' with the community: *Récoltes et semailles*."

This is the general idea that uninformed people tend to have. Let us examine what various professionals have to say about him. For example, the science historian Leo Corry, in *Writing the Ultimate Mathematical Textbook: Nicolas Bourbaki's Elements of Mathematics:*[1]

> *As it happened, however, Grothendieck left the group in around 1958–59 while some of the members, above all Serre, Schwartz and Dieudonné, continued to be close friends and collaborators. Later on, in 1970, he completely retired from public scientific life, as he discovered that I.H.É.S. was partly funded by the military.*

More recently, the French scientific magazine *Pour la Science*, read by a wide audience, published an issue devoted to the mathematician's life on the occasion of his passing.[2] In the first lines of the editorial of that issue, the managing editor of the magazine, Maurice Mashaal, says:

> *In the fifties and seventies, in France, an exceptionally brilliant mathematician impressed all of his colleagues.*

followed by the mention of his works in a few lines, and then, in the middle of his presentation:

> *After his sudden withdrawal from the scientific scene in 1970 right up until the last period of his life, which was in total solitude, he wrote tens of thousands of unpublished pages.*

Further, Grothendieck's biography is written by the mathematician Winfried Scharlau, who summarises his article in three points (section entitled "The essentials"):

- Son of a Russian anarchist and a German immigrant, and for a long time stateless, Alexander Grothendieck grows up in various shelters in Germany and then France.
- During the postwar period, he establishes himself as one of the greatest mathematicians of the century. He develops a new vision in algebraic geometry, his main discipline.
- In 1970, he suddenly turns his back on research. He is interested in radical ecology, then leaves permanently to live a reclusive life, limiting all contact with the outside world to a bare minimum.

If I am to believe the spirit of the time when I am writing this article, the "common opinion"– that Grothendieck effectively turned his back on the mathematical community in 1970 – was formed a few years after his disappearance.

However, with a little curiosity, it is easy to see that this vision is *completely false*! Let us search a little.

P. Cartier was a fellow mathematician and friend, both during and after the glorious years. This is what he said in 2000 in *Un pays dont on ne connaîtrait que le nom*. (*Grothendieck et les motifs*)[3]

> *(…) In the meantime, Grothendieck had dropped everything in 1970, after twelve years of unchallenged scientific rule over the I.H.É.S. Until his official retirement in 1988 at the age of sixty, he would only work sporadically, leaving behind a not insignificant "posthumous" work, of which three major writings stand out. The first,* A la poursuite des champs[4]*, written in 1983, is a six hundred-page reflection on multi-dimensional categories. Here, combinatorics, geometry and homological algebra are mixed together in a grandiose project. After fifteen years of effort, only three definitions have been created that are probably equivalent (or almost so) to multidimensional categories (in a broad sense*[5]*. Its stake is not just pure mathematics, since a good theory of assemblies has many potential applications (theoretical computing, statistical physics, etc.).*
> *The second,* Esquisse d'un programme*, is a text written in 1984 in support of a CNRS job application. Grothendieck sketched the construction of a tower (or a game of Lego), describing the deformations of algebraic curves. And finally,* La longue marche à travers la théorie de Galois*, written in 1981, gives partial indications on the constructions claimed in the Sketch.*

[1] Handbook of the History of Mathematics, Oxford University Press 2009.

[2] *Pour la Science*, September 2016, no 467.

[3] Preprint IHES/M/00/75, November 2000 and (written slightly differently) INFERENCE, International revue of science, http://inference-review.com/article/un-pays-dont-on-ne-connaitrait-que-le-nom. My quote is from the second document.

[4] Footnote of P. Cartier: The mathematician Ronald Brown explains the complex story of this document in the paper "The origins of Alexander Grothendieck's *Pursuing Stacks*" (…).

[5] In a footnote P. Cartier explains what the issues are.

Perhaps one will object that even though Grothendieck continued to produce mathematics, as Cartier testifies, he had completely broken with the community. However, by inquiring a little more, one can quickly learn that from 1972 to 1984 Grothendieck was professor at Montpellier, further from 84 to 88 a researcher at CNRS. The 1984 text *Sketch of a programme* has been published, it is cited by contemporary works and a whole colloquium[6] was devoted to it. Moreover, during this same period he had several PhD students at Montpellier, of which at least two, to my knowledge (maybe more), became university professors.

Récoltes et semailles is a very long text (about 1000 pages) by Grothendieck, which was mimeographed by the university of Montpellier (1985–86); it is a partly autobiographical memoir describing Grothendieck's life as a mathematician. A persistant rumour says that this writing is only a settlement of accounts with his former students, which would explain why it never found a publisher.[7] But, in the same *Preprint IHES/M/00/75* containing the paper of P. Cartier quoted above, we find a different analysis in *Découvrir et transmettre* by the historian of mathematics Alain Herreman. You will read in the conclusion of *Découvrir et transmettre*:

> *Whether it is the influence of his elders, the reception of his work and its transmission by his students, or the process of discovery,* Récoltes et semailles *is on all levels a confrontation with the collective dimension of mathematics and at the same time an attempt to elaborate it conceptually.(…)*

At this stage, now warned that this text is very different from just a "gunfight at the O.K. corral", you might want to examine it more closely and you will find many passages where Grothendieck evokes his professional activity from 1970 onwards:

> *As you know, I left the mathematical "great world"[8] in 1970 as the result of an issue of military funds in my home institution (the I.H.É.S.). After a few years of anti-militaristic and ecological activism in a "cultural revolution" style, of which you have probably had some form of echo here and there, I practically disappeared from the traffic, lost in a provincial university God knows where. Rumor has it that I spent my time tending sheep and drilling wells. The truth is that, apart from many other occupations, I was bravely going, like everyone else, to do my lecture courses in the faculty (that was my unoriginal bread and butter). It even happened to me, here and there, for a few days, even a few weeks or a few months, that I did maths again at "brin de zinc"[9] – I have boxes full of my scribblings, which I must be the only one who can decipher.*
> *R&S, p. 75*[10]

or:

> *(…) for more than ten years*[11]*, my friend*[12] *remained for me (in a self-evident way) my main discussion partner in mathematics; or more precisely, between 1970 and 1981, he was the only discussion partner (except during one episode) to whom I wanted to speak during the periods of my sporadic mathematical activity, when I was in need of an interlocutor.*
> *R&S, p. 307*

If we recall the work output that Grothendieck was capable of, it goes without saying that his sporadic activity is certainly equivalent to the activity of one or even more ordinary mathematicians!

To conclude this point, here is the testimony of Yves Ladegaillerie[13] who was his student at Montpellier:

> *As a professor in the Faculty of Science, he did the same work as others; with care, availability and dedication. He typed on his old machine the mimeographied texts which were generously distributed to everyone (…). As soon as he arrived in Montpellier in 1973, I taught with Grothendieck, and he quickly offered me to do some work in research with him (…). In the 80s, we had a small working seminar with him, Malgoire and Contou-Carrère, at Montpellier.*

These testimonies are enough to show that the assertion of Grothendieck's break with the mathematical world is inaccurate. If there is a break, it is not with the totality of the mathematical community but only with a part of it, perhaps the one he calls "great world" in the quotation above.

The story of Grothendieck's break with the mathematical community is therefore clearly a myth. However, this myth is currently being revised, as shown by at least

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6 CIRM, Luminy, conference on *Les dessins d'enfants*, April 19–24, 1993. See the book *The Grothendieck theory of dessins d'enfants*, Cambridge University Press (1994).

7 See note 10.

8 In French it is "grand monde"; it is something like "high society" with an ironic connotation. I come back later to what Grothendieck means with "grand monde".

9 Grothendieck's style is very literary, sometimes poetic, sometimes slangy, sometimes difficult to understand. Here the term "brin de zinc" could be a different writing of the slang "brindezingue" (the prononciation is the same) which means "completely drunk" or a deformation of "a tout berzingue" which means "very fast". R&S contains eleven ocurrences of "brin de zinc" in different settings. I give up any attempt to translate it.

10 *Récoltes et semailles* is not yet published but available on the web. My references (denoted by R&S) are from the version online at: https://www.quarante-deux.org/archives/klein/prefaces/Romans1965–1969=Recoltesetsemailles:pdf. The numbering of the pages corresponds to that of the pdf document, not of the original document. To my knowledge R&S is not translated. The translations of the quotes are mine.

11 After his departure from I.H.É.S. My note.

12 Pierre Deligne. My note.

13 *Grothendieck after 1970*, online at http://www.grothendieck circle.org

two recent texts.[14] On the one hand, Jean-Paul Allouche in his review of the book *Alexandre Grothendieck: A mathematical portrait*[15] in the *Newsletter of the European Mathematical Society* of March 2015, questions the so-called madness of Grothendieck:

> *What strikes me in the view that mathematicians have of Grothendieck's work and life is the huge gap between the interest and fascination for his mathematical work and – at least for a large majority of mathematicians – the fact that they are rejecting the rest of Grothendieck's thoughts (should we accept military grants, the unbearable explicit hierarchy that mathematicians build among themselves, the urgency of working in ecology and the like instead of dealing with mathematics, etc.). It is so easy to declare that he was "a bit mad" or "deeply depressed" or even "suffering of psychosis" rather than to think that he could well have been very much in advance also in these subjects.*

There is also a similar reflection in D. Nordon's "Bloc-note" in the journal *Pour la Science*[16]

> *When Grothendieck began to criticize the scientific institution in the 1960s and 1970s, some discredited his objections by saying that they where fuelled solely by his frustration. He had not been able to demonstrate Weil's conjectures and consoled himself by vilifying the institution. The problem with such psychological explanations is that they are always plausible, and never interesting.*
> *Frustration is the most shared thing. We have all been – and more than once – the fox facing the inaccessible grapes.*[17] *So there is not a whistleblower that you can't accuse of being frustrated, but it doesn't say anything about the value of what he is saying. If only arguments from authors virgins of any frustration had a chance to be judicious, no wise argument would ever have been considered since the world began! To disqualify an argument by interpreting it as the disguised expression of bitterness by its author is an avatar of an old process, as unfair as it is effective: to discredit an individual is easier than to refute his arguments. Especially if they are relevant.*

A myth is an imaginary construction developed to ensure a certain social cohesion around the defense of particular interests. My hypothesis is that the myth of the break was coined by this mathematical "great world" that Grothendieck left in 1970, which poses several questions:

- What is the "great world" actually?
- If there is indeed a mathematical "great world", is it actually responsible for the myth?
- If a group had indeed created the myth, what were its motives? What noble or less noble interests did it try to defend?

I am not claiming to know the answer to these questions, which go far beyond my competence and, I believe, that of any individual. Only a collective work involving competent mathematicians, historians and sociologists could enlighten us in a relevant way. My only ambition is to convince the reader that an open-minded reading of R&S could open up interesting research paths on the function and evolution of the French mathematical community between the immediate post-war period and the present day.

A short account of more or less-known facts

The hurried reader who knows the history of Grothendieck from before 1970 can skip this paragraph.

Montpellier, the arrival in Paris, the thesis in Nancy

Grothendieck studied mathematics at the University of Montpellier, where he did not find enough to satisfy his attraction to mathematics.

> *He passed his bachelor's degree at the faculty of sciences in Montpellier without his professors taking notice of him (nor did he notice his professors). He then went to Paris to become a mathematician.*[18]

Schwartz's slightly scornful opinion in referring to his colleagues at Montpellier does not seem completely justified. Indeed, Grothendieck did have a certain affection for "Monsieur Soula", who was trying to convince him that since Lebesgue there was nothing more to do in mathematics:

> *Mr. Soula, my "differential calculus" teacher, was a kind and willing man towards me. But I do not think he convinced me. There must have already been within me the preconceived idea that mathematics is an unlimited thing in length and depth.*
> *R&S, p. 34*

One can also think that Mr. Soula was aware of the Montpellierian shortcommings because he helped Grothendieck to "go up" to Paris and to meet Henri Cartan by providing him with a letter of recommendation.[19] More-

[14] See also on the website of the Collège de France, as part of the symposium for the start of the 2016 academic year, *Migrations, refugies, exil*, a reading by Alain Connes of several pages of R & S, as well as *La clef des songes* that will make the listener want to read more.

[15] Leila Schneps (ed.), International Press of Boston.

[16] March 2016.

[17] This is a reference to Jean de Lafontaine's fable *The fox and the grapes* where the fox covets grapes that are inaccessible.

[18] Laurent Schwartz, *Un mathématicien aux prises avec le siècle*, Editions Odile Jacob, Paris (1997) p. 292.

[19] Here is a testimony of J-P. Kahane who knew Mr. Soula: One day, with Soula and Turrière, the conversation turned to Grothendieck, whom they had as a student. They took out license exam copies for me, and all of a sudden my respect for them took a huge leap forward. These copies were illegible. An examiner might have refused to read them. But these old gentlemen, Soula at first I think, had felt what was behind Grothendieck, and he passed his examinations. Jean-Pierre Kahane, *Grothendieck et Montpellier*. Images des Mathematiques, CNRS, 2014..

over Grothendieck, if he says that Montpellier could not satisfy his thirst for mathematics, also says that he does not have a totally negative memory of it:

> *Yet now, thinking back to those three years, I realise that they were not wasted. Without even knowing it, I learned in solitude what the essence of the mathematician's job is – what no teacher can really teach. (…)*
> *To put it another way: I learned in those crucial years to be alone. By this I mean conversing with my own mind about the things I want to know rather than relying on the ideas and consensus, whether expressed or implied, that come to me from a larger or smaller group that I feel myself to be a member of, or that for any other reason would be invested by me with some authority.*
> *R&S, pp. 34–35*

In Paris, Grothendieck discovers the world of mathematics "in the making", follows a course by Leray, attends the Cartan seminar:

> *In the year that followed, I hosted a Cartan course at "L'École"*[20] *(on the differential formalism on manifolds; also that of the "Séminaire Cartan", to which I clung firmly, amazed to witness the discussions between him and Serre, with big shots of "spectral sequences" (brr!) and drawings (called "diagrams") full of arrows covering the whole picture. (…) I had been to see Mr. Leray at the Collège de France to ask him (if I remember correctly) what his course would entail. I do not remember any explanations he gave me, nor did I understand anything about it (…)*
> *R&S, p.140*

Conscious of the value of Grothendieck, A. Weil and H. Cartan sent him to…

> *(…) Nancy, which at that moment was a bit like Bourbaki's headquarters; with Delsarte, Dieudonné, Schwartz, Godement (and a little later also Serre) teaching at the University.*
> *R&S, p. 145*

He wrote his thesis, which he defended in 1953, under the direction of L. Schwartz who tells us:

> *It was the most beautiful of "my" theses. (…) The collaboration with this talented young man was a fascinating and enriching experience.*[21]

From 1953 to 1956 he took multiple trips to universities abroad.

At I.H.É.S. 1958–1970

I.H.É.S. was created in 1948 by the industrialist Léon Motchane based on the model of the Institute for Advanced Studies at Princeton. Jean Dieudonné and Alexandre Grothendieck were the first full professors of the institute. From 1960 to 1969, a seminar was organised by Dieudonné and Grothendieck, the "Séminaire de Géométrie Algébrique", which led to a series of Lecture Notes in Mathematics: the famous S.G.A. This seminar was attended by elite, young researchers, almost exclusively from E.N.S. They would write down the content of Grothendieck's weekly presentations with the help of Dieudonné, and Grothendieck would supervise. It is quite an exceptional situation that a seminar leader does not write his own material by himself, or at least with one or two close associates. Instead, Grothendieck set out the main points of his theories and half a dozen smart students wrote the S.G.A.s. more or less anonymously. This situation is obviously close to that of Bourbaki, but with a one major difference: the founders of Bourbaki – Weil, Cartan, Delsarte, De Possel and Dieudonné … are well-known mathematicians and peers with equal rights and duties (even if some are a little more equal than others), and this collegiality "without a leader" seems to continue with the younger generations (Cartier, Serre,…); while for the S.G.A., it is young people preparing a thesis who write down mathematics that is only partially theirs. At the same time, between 1960 and 1967, Grothendieck wrote, in collaboration with Dieudonné, the *Elements of Algebraic Geometry* (E.G.A.), which appears in the form of eight volumes of I.H.É.S. publications, solely under his name.

In 1966 he refused to go to Moscow to receive the Fields Medal in protest against USSR policy in Central Europe. Soon after, he began getting involved in supporting Vietnam against the United States.

The opening to the world

In the wake of the events of 1968, Grothendieck engaged in political activism. His two main commitments were the denunciation of scientific work for military purposes and the participation in the creation of the movement "Survivre et vivre", the first movement of political ecology in France.

In 1970, discovering that the I.H.É.S. had received some small military funding, he asked that this subsidy cease immediately. Not receiving the support he had hoped from his colleagues, he resigned. In 1971 his course at the College of France, where he had been appointed for two years, was not renewed because of his intention to devote sessions on the theme *Science and Technology in the current evolutionary crisis: are we going to continue scientific research?* in addition to his course on "La théorie de Dieudonné des groupes de Barsotti–Tate". Also on this occasion, he did not receive the expected support from his scientific community.

What is the "great world"?

In my first quote from R&S, Grothendieck talks about his departure from the "great world" of mathematics. In

[20] In the French mathematical community everybody understands that "École" is "École Normale Supérieure de la Rue d'Ulm".

[21] Laurent Schwartz, *Un mathématicien aux prises avec le siècle*, Editions Odile Jacob, Paris (1997) p. 294.

R&S I counted the expression "great world" twenty-nine times, and almost as many times for "beautiful world." He gives a definition in a footnote:

> *There was also, in the background, the thought of a certain spirit in the world of mathematicians, and more particularly in what might be called (without sarcastic or mocking intonation) the "great world" of mathematics: the one which "sets the tone" for deciding what is "important" or "lawful" and what is not, and which also controls publications and, to a large extent, careers.*
> *R&S, p. 388*

but I do not know what to think about it in so far as he specifies that here he uses the expression the "great world" "without a sarcastic or mocking intonation", whereas, for the most part, his intention is, at least, ironic. That is why I am going to specify what I personally mean by "great world", but without claiming that this is precisely what Grothendieck intended.

Promoting the progress of mathematical knowledge is a complex collaborative undertaking led by a community of people. Like any human community of any size, the mathematical community has operating rules that are written in regulatory texts and traditions. Like any community, it has managers: laboratory directors, members of the C.N.U.[22], members of the Academy, leaders of scientific societies, etc. They are responsible for making the institution work at its best by exercising the power they are invested in. They are leaders but they are not necessarily part of what I call the "great world".

I believe that the "great world" is to be sought elsewhere. In addition to the transmitting of "well-established knowledge", the mission of the mathematical community is, as for any scientific community, to distinguish what precisely has the vocation to become this "well-established knowledge" among the mass of accumulated results. This essential mission cannot be assumed by the political power and the state, as Lysenkoism has shown in the USSR, or as anti-Darwinism has shown in the USA. In Western-style democracies, it is entrusted to what is known as "peer judgment". "Peer judgment" is exercised on two crucial occasions.

The first one is, as is well known, the process of reviewing the articles submitted to mathematical journals and the second occasion is when people are recruited to academic positions where questions of balance between disciplines and teaching requirements can compete with scientific excellence. In theory, the voice of each peer has the same value, but it is natural that the voice of leading mathematicians carries more weight than that of smaller players.

It happens that some mathematicians or groups of mathematicians have such a high opinion of their excellence that they think that their vision of mathematics must prevail at all costs. It is much more than a mere corporatist attitude, where you defend your discipline and try to "fit in" your friends. It is a much more disturbing attitude that is close to fanatism. They feel personally invested with the mission to defend what they consider to be mathematics ("La mathématique", as Bourbaki says), to propagate the "true faith" and behave like real crusaders as Arnaud Denjoy says of Bourbaki.[23]

> *I fear your absolutism, your certainty of holding the true faith in mathematics, your mechanical gesture of drawing the sword to exterminate the infidel to the Bourbaki Qur'an. […] We are many to judge you as despotic, capricious, sectarian.*

It is these mathematicians, often of great renown, but not always, and certainly with an oversized ego, that I call the "great world" or the "beautiful world". So my "great world" is not what Alain Herreman calls (in Grothendieck's words) the "microcosm", made up of about twenty colleagues or students who have had professional relations with the mathematician, even if some could be part of it. Nor do I seek, as I have already said, to demonstrate here that this is indeed what Grothendieck means by "great / beautiful world" but simply to use this concept a little vaguely to read some passages of R&S.

Before 1970: Grothendieck in the "great world"…

The partly fanatical behaviour that I attribute to individuals in my concept of "great world" does not enable me to decide lightly if Grothendieck was a member of it or not; and I will not do that. I do not know if he was willing to cover up bad cases in the name of mathematics' defense, all I know is that, before 1970, his whole life was devoted to mathematics, to his own mathematics, which is a respectable choice. But it is indisputable that his privileged position at I.H.É.S. made him very close to the "great world" and adopt in a more or less conscious way certain of its attitudes. In R&S he severely criticises this behaviour, which he now considers (after 1983) as harmful. What is interesting here is that this criticism, which includes a large part of self-criticism, cannot be swept away on the pretext that it is only a vulgar "settlement of accounts". Thus he explains at length some of his attitudes as his "contempt", that retrospectively he severely judges:

> *I did not say to myself back then that if the pupil*[24] *was indeed unproductive, it was a reason to advise him to do something else and to stop working with him, but not to treat him with contempt. I had identified myself with being "strong at maths" such as this prestigious elder, at the expense of "nobodies" that it would be lawful to despise.*
> *R&S, p. 146*

[22] The national organism that rules careers in France.

[23] Arnaud Denjoy to Henri Cartan (May 22, 1954). Archives de l'Académie des sciences, fonds Montel, carton 1. Quoted in: Anne-Sandrine Paumier, David Aubin, *Polycephalic Euclid?: Collective Practices in Bourbaki's History of Mathematics,* 2013. ¡hal-00871784v3¿.

[24] Grothendieck relates the attitude of one of his prestigious elders towards one of his students.

This question of respect for the person and the importance of respect in the process of mathematical creation holds a huge place in R&S but this is not the point I want to address.[25] Beyond the necessarily subjective appreciation of the moral attitudes of the "great world", there are more objectively appreciable practices of which here is an example:

> *Towards 1960 or 1961 I offered Verdier the development of new foundations of homological algebra as a possible thesis work (…) His work on foundations continued satisfactorily, materialising in 1963 with a "State 0" on the derived and triangulated categories, mimeographed by the I.H.É.S. (…)*
> *If its defense[26] did not take place in 1963, but in 1967, it is because it was unthinkable that this 50-page text, the embryo of a foundation work still to come, could constitute a doctoral[27] dissertation – and of course the question did not even arise. For the same reason, at the thesis defense on 14 June 1967 (in front of a jury consisting of C. Chevalley, R. Godement and myself, who presided), it was out of the question to present this work as a thesis. The text submitted to the jury of 17 pages (+ bibliography) was presented as an introduction to a large-scale work in progress. (…)*
> *R&S, p. 352*

We are very far from the tradition which at the time required, for the "thèse de doctorat d'état", the defense of a "second thesis" which consisted of the oral presentation of a work far from the specialty of the candidate! Grothendieck recognises this:

> *If the title of doctor of science was awarded to J.-L. Verdier on the basis of this 17-page text, sketching some ideas that he himself admitted had not all come from him, then it was clearly a goodwill contract between the jury and himself: he committed himself to completing and making available to the public this work of which he presented a brilliant introduction.*
> *R&S, p. 352*

That this testimony by Grothendieck is made in the context of his "settling of accounts" with his former student does not detract from the objectivity of facts that are nevertheless important because they concern the successor of H. Cartan as Director of study at E.N.S. That Verdier was a high-quality mathematician at that time is probably not doubtful to the specialists of the discipline – I do not know – but was that reason enough to go over the rules and traditions and just accept that he gets away with minimal drafting work? Grothendieck, the author of R&S, therefore the Grothendieck after the break, actually considers that on this occasion he has shown lightness:

> *I accept the entire responsibility as J.-L. Verdier's thesis supervisor and president of the jury, for my folly in having awarded him (together with C. Chevalley and R. Godement, both trusting the guarantee I gave) the title of doctor for a work which had not yet been entirely completed (…) To this responsibility I should also add not having ensured during the two years that followed (before my departure from the mathematical scene) that Verdier did indeed fulfil the contract he had made.*
> *R&S, p. 353*

Grothendieck is certainly at fault but less, it seems to me, than Chevalley and Godement, and more generally than the microcosm of the S.G.A., who let it go. Since he lived only for the mathematics that he identified with the activity of the S.G.A., he was excusable of this contempt of tradition; a tradition that he did not know very well because of his atypical career.

…and then on the other side: after 1970

A large part of R&S is devoted to a "case" in which Grothendieck accuses several members of what he calls the "great world" to have, first, ignored the work of a mathematician who was not part of the brotherhood, and then, in a second instance, to have looted it. There are certainly some interesting things to understand in this testimony of Grothendieck, but that presupposes a very good understanding of the mathematics involved and the truth of the facts asserted, which is not at all my case. This is the part where Grothendieck engages most in personal attacks, whose foundation can only be appreciated by sharp specialists. I will not talk about it. On the other hand, the following testimonies, which concern his work as an "ordinary mathematician" in Montpellier, speak for themselves.

Despite the fact that he has decided to leave the "great world", Grothendieck still intends to continue to do mathematics. During his stay in Montpellier he is interested in a few young mathematicians and experiences the ordinary mathematician's life. Before going into detail, let's say that these are instances where he can see that giving his approval to the work of young mathematicians no longer has any effect on the work's reception by the "great world", since he is no longer a member of it.

As the works in question are related to his own body of work (naturally, since it concerns his testimony on their value), he attributes their rejection to the "burial" enterprise of which he feels the victim. But there is another reading, less centered on Grothendieck and more simply factual, that we can try here.

[25] In *Découvrir et transmettre*, cited above, A. Herreman offers a fascinating analysis of Grothendieck's entire reflection on the collective process of mathematical creation, and thus of this particular point.

[26] of Verdier's thesis

[27] Grothendieck says "thèse de doctorat d'état": in the sixties, in France, there were two thesis: a small one (two years of research) called "thèse de troisième cycle") which is more or less equivalent to our present "mastere thesis" and a "big thesis", the "thèse de doctorat d'état" (approximately 5 years of research) which is roughly equivalent to our present "habilitation thesis".

The difficulty in publishing

In the 1970s, Olivier Leroy was a young man from Montpellier who impressed Grothendieck with his skills:

> *He was a young man of maybe twenty who must have had just a smattering of diagrams, a bit of topology and topos, and had handled a lot of infinite discrete groups, I think… It was three times nothing, to be honest, and yet with that he managed to fill in all the blanks anyway and to "feel" without any effort what I, an old veteran, told him at full speed for two or three hours, based on a fifteen-year familiarity with the subject. I had never seen anything like it, or at most with Deligne, and perhaps with Cartier, who was also quite extraordinary in that respect at such a young age.*
> *R&S, p 406*

O. Leroy wrote some of his work in the form of a note to the C.R.A.S.[28] The note was rejected. Grothendieck has an opinion on this rejection:

> *One of my friends and companions of yesteryear gently explained to me recently that, in the course of time, alas, and with the immeasurable increase of mathematical output that we are aware of, "we" are absolutely obliged, whether we like it or not, to make a strict selection of the papers that are written and submitted for publication, and to publish only a small part. He said it with a sincere, desolate look, as if he himself were also victim of this inevitable fatality – the same look that he had when saying that he was himself also a member, yes it's unfortunate but that's how it is!, of the "six or seven people in France" who decide which articles are going to be published, and which ones are not (…)*
> *R&S, p. 187*

In this passage, Grothendieck offers us a fairly accurate portrait of a member of the "great world". He returns to this character:

> *One or two months later I learned that this colleague had refused a few years ago to recommend the publication of a certain note to the "comptes rendus", whose author as well as theme (one that I had proposed to him roughly seven or eight years before) were dear to me. (…)*
> *I think he did an excellent job (presented as a "thèse de 3e cycle). I had never been this young researcher's boss, brilliantly gifted as he is (I do not know if he will continue to apply his gifts to mathematics, given his welcome…), and he completed his work without any form of contact with me. But it is also true that the origin of the theme that was developed could not be doubted; he was in big trouble the poor guy, and possibly without suspecting anything! This colleague said that in a civilized manner, there is at least that and I would not have expected less from him, "sincerely sorry but you understand…". Two years of work by a highly motivated researcher just starting out versus the three-page CR cost – how much public money would it have cost?*
> *R&S, pp. 188–189*

and understands that for some time now this work has struggled to be recognised. He then gives us a juicy but totally imagined description of the censor's behaviour:

> *This same draft of notes to the "Comptes Rendus" had the honour of being submitted to another one of the "six or seven people in France…", who sent it back to the author's "boss" because these mathematics "did not entertain him" (textual!). (The boss, revolted but cautious, himself in a rather precarious position, preferred to say nothing rather than saying anything displeasing…) Having had the opportunity to discuss it with this colleague and ex-student, I learned that he had taken the trouble to read the note carefully and consider it (it must have brought back many memories), and that he had found that some of the statements could have been presented in a more helpful way for the user. He did not deign, however, to waste his precious time submitting his comments to the person in question: fifteen minutes of the renowned man's time versus two years of work by an unknown young researcher! Maths amused him enough to seize this opportunity to reconnect with the situation studied in the note (which could not fail to generate in him, as well as in myself, a rich tapestry of various geometric associations), to assimilate the given description, then, with very little effort given his experience and his means, to detect some clumsiness or deficiencies. He did not waste his time: his knowledge of a certain mathematical situation clarified and enriched thanks to two years of conscientious work by a researcher taking his first steps; work that the Master would certainly have been able to do (in broad outline and without demonstrations) within a few days. This being gained, we remember who we are – the cause is judged, two years of work by Mr. Nobody are good for the rubbish bin…*
> *R&S, p. 188*

In the following case, Grothendieck is more personally concerned, since it refers to one of his students:

> *Yves Ladegaillerie started working with me in 1974. (…) and he grasped it more or less, until the day when it ended up "tilt" with him, I do not know when and why. (…) From the moment Yves had grasped it, he did his thesis in a year, a year and a half, results, writing, everything, and moreover dressed to the nines. It was a brilliant thesis, less thick than most of those which had been written with me, but as substantial as any of those eleven theses. The defense was done in May 1976. The thesis is still not published today. (…) The central result will finally appear, nine or ten years later and reduced to the bare bones, in a short article of Topology (hush – I have an accomplice in the editorial board of this estimable journal).*
> *R&S, pp. 399–400*

[28] Comptes Rendus de l'Académie des Sciences.

The ordinary mathematician of my generation who has not rubbed shoulders with the "great world" will not be surprised by this testimony, which he will probably have read with jubilation. It's somewhat the "sprinkler sprinkled". Grothendieck, whose opinion was to be solicited as the divine word when he was the guru of the "great world", does not even manage to make a poor little "note" now that he is no longer recognised!

Admittedly, it was not always easy to publish, especially if one was not part of the batch of a great feodalism having some power, but I would not want to give the impression that in these years it was not possible to publish a note to the Academy outside the servile allegiance. Allow me a little personal testimony. At the beginning of the 1980s the little mathematics school I belonged to was disputed and had trouble getting anything published. I was in contact with R. Thom, whose hostility to the ideas of the school in question I knew, but whose listening qualities I appreciated. On my advice, a young colleague asked him to present a note to the "Comptes Rendus", which he did. Surprised, I took the opportunity to tell him that I was delighted to see that he was convinced by the point of view of my school of thought: "Not at all…", he answers me.

– "But did you not send the note! Then surely you agree? I do not understand."

– "It's very simple, though. Mr. X seems to me to be an entirely competent mathematician. He did not convince me, it's a fact, but others than me could be convinced. By what right would I deprive the community of appreciating those ideas that may be relevant?"

This is not the supposed style of Grothendieck's "companion of yesteryear".

Professionnal instances

Contou-Carrère is a mathematician with a non-traditional background. As a holder of a foreign doctorate he is the only candidate for a position as professor at the university of Perpignan [29] and supported by local mathematicians. The "Comité Consultatif des Universités" (C.C.U.) rejects the proposition.

> *The fact remains that Contou-Carrère's candidacy was ruled inadmissible by the "Comité Consultatif des Universités" and the file was returned. The thing that baffled me was that, in the absence of an official explanation, neither the President of the CCU (the national board that made the decision) nor any of the members had the minimum of respect to write personally, either to Contou-Carrère himself, or at least to the director of the Perpignan Institute of Mathematics, to give a few words of explanation as to the meaning of this vote, which could only be perceived as a stinging disavowal of the choice of Perpignan's colleagues, as well as a disavowal of their sole candidate as being capable of honourably filling the post he had been offered.*
> *R&S, p. 400*

[29] A small town in the south of France.

The contempt displayed by the C.C.U. is indicative of the spirit of the "great world". Of course the C.C.U. may have been right regarding the case's substance, but it is totally unacceptable to not explain the reasons for the rejection. Finally, let me show a last incident that humiliated Grothendieck, being reduced to the rank of standard mathematician.

In 1983 there was a "Commission des Thèses de Mathématiques des Universités Parisiennes", without an institutional existence, whose members were not known; the mathematical community agreed to have to obtain its approval for a defense to take place in a Parisian university. One of the peculiarities of this commission is that it did not motivate its decisions.[30]

In 1983, Grothendieck wanted Contou-Carrère's thesis to be defended.

> *It is all the more remarkable [Grothendieck refers to Verdier's own defense] that J.-L. Verdier refused my proposal to be part of the Contou-Carrère Thesis Jury in December 1983, with J. Giraud and myself having the role of research director, estimating that the thesis (entirely written and yet carefully read by J. Giraud) and the jury would not offer the guarantee of sufficient seriousness without referring to the control of the "Commission des Thèses de Mathématiques des Universités Parisiennes" (Sic).*
> *R&S, p. 353*

In R&S, Grothendieck mainly attributes his new situation after 1970 to a degeneration of mores. He attributes to his elders, Weil, Cartan, Dieudonné and Leray…, all sorts of moral virtues that are absent in his contemporaries and his pupils, who would indulge in the worst turpitudes. In fact, what has mainly changed for Grothendieck between the sixties and the seventies is not so much the mathematical world as his position in the world. But an evolution of this world is not excluded. Anyway, once again his testimony deserves attention.

Who benefits from the myth?

In consideration of these testimonies we are obliged to take note of the existence of some serious shortcom-

[30] Here is the response received by the supporters of a candidate who had been denied the defense of his thesis on a subject (unrelated to the work of Grothendieck, let us specify): "I answer you on behalf of the "Commission des Thèses" to the letter addressed to the President of the University of Paris VII concerning the functioning of the "Commission des Thèses". This letter has of course been sent to us. (…) In this letter, you raise the case of the thesis of Mr X for which the "Commission des Thèses" asked the opinion of a second reviewer following a first report which did not allow the Commission to take a final decision. You are asking us to send you this latest report. The Commission was unanimous in finding your request inadmissible. It is a fundamental principle of the functioning of the Commission that the confidential nature of the reports requested, which guarantees the freedom of judgment of the reviewer. (…) In *Et pourtant ils ne remplissent pas* N*!*, C. Lobry, Aléas, 1989).

ings in the implementation of the "judgment of peers" in the French mathematical community. The testimony of Grothendieck is not a revelation and at the time of Grothendieck's diffusion of the 250 copies of R&S mimeographed by the University of Montpellier there were other testimonies of serious dysfunctions of this kind, but the personality of their authors, often modest mathematicians, meant that they did not get much attention. The "great world" had good grounds to challenge them by highlighting the alleged mediocrity of their authors. But with Grothendieck this type of argument was no longer adequate.

So the "great world" invented the myth of the break. Grothendieck's uncompromising personality lent itself remarkably to the operation: his break with the "great world" was confused with a withdrawal from the whole community, his uncompromising analysis of the mathematicians' collective work with a "settling of accounts", the expression of a particular spirituality with a "mystical delirium".

The "great world" who knows well that the community, as a whole, does not approve of Grothendieck's militant provocations:

- it is not well-regarded to challenge the mathematician L.S. Pontryagin at the Congress of Nice on the potential military use of his works,
- one does not transform a chair at the Collège de France into a leftist tribune in the style of sixty-eighters,

thinks, rightly, that not too many questions will be asked. Time will do the rest.

Now let us hold ourselves back from virtuously booing the "great world" that we have just described and which no longer exists in this form. It has indeed become fashionable to distance oneself from it, even to despise it, for example through a critique, *a posteriori* of the "mistakes of Bourbaki" who failed to recognise the importance of mathematical physics, the probability theory and failed to predict the importance of computing. For the current mathematical community it would be the same unfortunate mistake as refusing to take into account the testimony of one of its most distinguished members. We must remember the exceptionally rapid growth of the community between 1950 and 1980. If my recollections are correct, there were about fifteen mathematics teachers at the faculty of sciences of Grenoble in 1960 when I was studying there. When I left it in 1970, there were about one hundred and fifty. Before the war, only a few former students of the École Normale Supérieure, often after a passage at secondary schools, would go to research.

The situation did not change much immediately after the war and then, suddenly, during the sixties, things accelerated. It was normal to worry about the consequences of the "massification" of the profession of mathematician. The arrogance of the "great world" individuals was certainly not the right answer, but it is in this context that it must be analysed.

We must not forget that this "great world" with a high social position is also the one who created the I.H.É.S., this small institute with an annual budget of a few million which housed no less than 11 Fields medals in half a century of existence. It is definitely not nothing! Especially since some disciplines that Bourbaki had "missed" are now justly represented. And if one thinks that my reference to the I.H.É.S. is too elitist to look at, then consider the CIRM, this perfectly democratic instrument at the service of the wider community, which we owe to a great extent to the energy of the most elitist among the elitists: Jean Dieudonné.

The role of the "great world" is to be appreciated in the context of the era which is one of a transition between the feodal world and the capitalist world. In the post-war period and until the 1970s, the French mathematical world functioned as in the feodal world where legitimacy stems from lineage. Twenty years later it had clearly turned to the capitalist world where legitimacy stems from the money one is able to make. This transition took place with the confrontation of the old world, that of pure mathematics, with the new world of applied mathematics and computer science. The decline of the Bourbachic empire saw the expansion of a new form of domination that proposed its new ways of organising research. There were no more large pyramidal organisations supporting the big names of the moment, but small competing teams animated by a single dynamic scientific personality. The INRIA[31], among others, and Jacques-Louis Lions, emblematic figure in the rise of mathematics related to computer sciences, successfully proposed this new model of development in mathematics for their entry into techno-science. New dynamics where put into place, control by the "great world" has given way to that of the "evaluations" and the arrogance of brilliant mathematicians to that of no less brilliant "managers". Who knows if the resistance of the "great world", in spite of its reprehensible excesses, has not partly protected us (temporarily?) from a limitless commercialism which will, for sure, lead to the end of science as we know it since Galileo? There are still too few studies on the evolution of power structures in the French mathematical community at the turn of the 1970s. A good knowledge of the mechanisms that led to the present situation is obviously essential to better apprehend the future and a critical but honest reading from R&S can help. I wrote this paper simply in the hope of arousing some vocation.

The myth of Grothendieck's break with science is useless now that the "great world" which he was supposed to protect no longer exists.

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[31] Institut National de la Recherche en Informatique et Automatique, a French institute devoted to computer sciences.

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