

## The Force of Minimalism: An Interview with Jean-Claude Milner

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Knox Peden (KP): In your book, *L'Œuvre claire* you describe the *Cahiers pour l'Analyse* as the culmination of the 'first Lacanian classicism'. You describe the project as 'hyperstructuralist', suggesting that it adheres to Lacan's own hyperstructural conjecture, which can be stated: 'Any given structure has distinctive properties.'<sup>1</sup> How did this conjecture inform the *Cahiers pour l'Analyse*? And in what sense would you say that the *Cahiers pour l'Analyse* were 'hyperstructuralist'?

Jean-Claude Milner (JCM): We need to understand that in speaking of a first classicism and hyperstructuralism, I'm proposing my own interpretation, and that I'm proposing it after the fact. These terms did not exist when the *Cahiers pour l'Analyse* was constituted. It is certainly possible that some or even most of the participants would not recognize themselves in my interpretation of the enterprise. This is a first remark.

I think that ultimately we cannot grasp this brief venture that was the *Cahiers pour l'Analyse* if we do not consider the structuralist movement unfolding at the time and, moreover, the two-fold interpretation that one can make of structuralism. There are two possibilities. Either a given structure has no properties, which means that a structure only has properties if it is particularized (a particular phonological system, a particular system of kinship, etc.) Or instead, and this seems to me especially explicit in Lacan, a given structure in which no particular term is specified is in fact already endowed with properties. I think that this is what Lacan was presenting in the appendix to 'The Purloined Letter' (E, 54-61/41-48). Drawing on mathematical frameworks, Lacan tries to show that, given a minimum of possible particularizations, we can produce distinctive properties [*des propriétés non-quelconques*]. Hyperstructuralism seeks to establish the non-arbitrary properties of an arbitrary structure. These non-arbitrary properties are born from the very functioning of an arbitrary structure. The latter is thus a creator of properties; it is, in a certain sense, active. It seems to me that this is the program that dominates the first part of Lacan's work, what I call the first classicism. I was very responsive to this program; it can be seen in my article 'The Point of the Signifier' (CpA 3.5), and I would suggest in Jacques-Alain Miller's text 'Action of the Structure' (CpA 9.6) as well.

Let us consider the idea that the structure can have an action; what is this action? This action consists in making singularities emerge via the play of the structure alone. Whence the active character of the structure. Whence the active character as well of the signifier, the present active participle [*le signifiant*] opposing the perfect passive

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<sup>1</sup> Jean-Claude Milner, *L'Œuvre claire: Lacan, la science, la philosophie* (Paris: Seuil, 1995), 104. The French reads: '*La structure quelconque a des propriétés non-quelconques.*'

participle of the signified [*le signifié*]. Now, Lacan rarely considered the signified. In privileging the active participle, the signifier, he in fact extracted it from the signifier/signified pairing. He broke this pairing. He chose to distribute [*verser*] the signifier on the side of what he calls pure action, pure in that it is posited in itself and not in inverse symmetry with passivity. In making a theory of the action of the structure, one makes a theory of the signifier as active; one demonstrates that the signifier is only a signifier insofar as it generates a structure and that the structure is only a structure to the extent that it generates the relation of a signifier to another signifier.

KP: In the Foreword to the first volume Jacques-Alain Miller says that 'Epistemology in our sense is defined as the history and theory of the discourse of science (its advent justifies the singular)' (CpA 1.Introduction). Two things are striking in this passage: the precision of the singular – *la science* – but also the emphasis on the history and theory of the *discourse* of science and not the history and theory of science per se. Why this emphasis on singularity? And, moreover, how did you and your colleagues conceive this relationship between science and its own discourse?

JCM: Here again I am not sure that I can speak for all those who participated in the *Cahiers pour l'Analyse*. The reflections on the plural and the singular with regard to science came up very early on because this had been an objection against the *Cahiers* from the outset, most notably from the Althusserians. François Regnault wrote an article bearing precisely on the varying approaches to epistemology according to whether science is considered as either one or plural (CpA 9.4). In my view, science in the singular only makes sense if it is reduced to the cut or break [*coupure*] between science and non-science. And yet, from the point of view of science, there is fundamentally only one single structure of the cut; and *this* is what grounds the singularity of science. This doesn't exclude the possibility that there could effectively be many different sciences, but if we keep to structure, the cut between science and non-science is found in all the sciences. Under different forms, eventually, but it is the same structure of cut. This is how I grasped the question of epistemology at the moment of the *Cahiers pour l'Analyse*.

But I can be more precise. Already at this time, and here again I'm still speaking as an individual, I was troubled by the fact that in Lacan's writings science in the singular was essentially mathematical physics. Now, let's consider, alongside structuralism and structural linguistics, biology, which in the 1960s was beginning to be referred to as the epistemological model for structural linguistics; this is the moment when the notion of the genetic code began to become the dominant paradigm. I was struck by the fact that I saw no direct resemblance between mathematical physics from the point of view of its paradigm and the paradigm of structural linguistics or that of biology. I accorded much importance to the fact that it was necessary to preserve this diversity, on the one hand, and on the other to maintain the notion of science in the singular. My response was as follows: science in the singular is the cut between science and non-science; and yet, the cut between science and non-science recurs under diverse forms in mathematical physics, in linguistics, in genetics. A key historical point: I remind you that all of this must be reinterpreted in the light of an intellectual world where Popper didn't exist.

KP: Related to the notion of *coupure*, this is something you discuss in *Le Périple structurale* and *L'Œuvre claire*, is the signal importance of Alexandre Koyré and this notion of what you call 'extended Galileism' [*galiléisme étendu*].<sup>2</sup> Could you say more about the reading of Koyré at this time? He was obviously important for Lacan. How did this notion of Galileism as a definition or a way to talk about modernity and modern science figure in your thinking at the time?

JCM: We should distinguish between what I thought at the time and what I say today. The term Galileism is one that I forged rather late; I'm not saying that it didn't exist before me, but if it did, I never encountered it. I used it in *L'Œuvre claire* and *Le Périple structural*, but this was not a term that was used in the *Cahiers pour l'Analyse*; Galilean science, yes, Galileism, no.

Why did I speak of Galileism in *Le Périple structural*? Essentially so that I could introduce the notion of an 'extended Galileism'. In fact, if I did not have to introduce extended Galileism, I would not need to talk about Galileism. And the notion of Galileism is not Koyréan at all. But in the *Cahiers pour l'Analyse* Koyré was clearly the reference. If you like, I would say that, it is not exactly a paradox, but from the point of view of the presentation of terms, there has been a displacement in the notion of the epistemological break. The epistemological break is a term that was invented by Bachelard, and Koyré did not really use this notion. What he describes is the emergence of a new figure of science, marked by Galileo. Something was at stake, a genuine reversal with respect to what had preceded it. For the first time in history, mathematical entities did not serve as the means to think the eternal but rather the transient [*le passage*]. For an Aristotelian, there is a difference in nature between the world of celestial beings, which follow mathematical laws since these beings are eternal, and the sublunary world, the earthly world, which is subject to generation and corruption and to which mathematical entities do not apply in an explicative way. But, Koyré says, with Galileo something absolutely particular happens: firstly, the celestial beings are not perfect beings – the problem of sunspots, etc. – and, even so, this does not prevent mathematical laws from being defined for them; secondly, mathematical laws can be defined for the sublunary world.

Under the influence of Althusser, we thought that the Bachelardian notion of the epistemological break or cut [*coupure*] allowed us to describe the rupture [*rupture*] that Koyré described. I am not sure that Koyré himself would have accepted this formulation. All the same, you can see how we were led to think of this notion of cut in itself as the fundamental notion – I was, at any rate. To think the notion of cut itself is to think it as a structural notion; thus it is effectively the same notion that permits us to understand the epistemological break as Bachelard described it and the discursive displacement that Koyré described.

KP: At one point in *L'Œuvre claire* you suggest that the ideal informing the *Cahiers* was an ideal of analysis rather than an ideal of science per se,<sup>3</sup> which raises a question about this relationship between the two terms, *la science* and *l'analyse*. How was this relation

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<sup>2</sup> Milner, *L'Œuvre claire*, 95. On Koyré, see 33-76. Cf. Jean-Claude Milner, *Le Périple structurale: figures et paradigme*, 2<sup>nd</sup> edition (Paris: Verdier, 2008), 205, 275-376.

<sup>3</sup> Milner, *L'Œuvre claire*, 37.

understood? Was psychoanalysis to be understood as one variant of a more general concept of analysis? Or was it the fundamental analysis in question?

JCM: What I said in *L'Œuvre claire* and what I say later in *Le Périple structural* turns around the question of mathematization. Science here, with the definite singular article (*la science*), is mathematical physics. It makes use of mathematics in the fullest sense of the term; Descartes was at the same time a mathematician and a physicist – I'm thinking of the *Dioptrics*. Einstein was not a very good mathematician, but that's of little consequence. There is no doubt that what he used in the theory of relativity were quadratic equations, perfectly defined in mathematics. By contrast, structuralist linguistics uses very little mathematics in the strict sense. In fact, it does not use it at all.

Thus, the first question: if we consider structuralist linguistics as a variant of Galilean science, does this not mean that we understand mathematics differently or that, in the Galilean operation, strict mathematization is only a variant among others within a larger operation? With Lacan, you find attempts at strict mathematization. I take the example of the appendix of the 'Purloined Letter' – but it's not true, if you take the definition of the signifier as 'the signifier represents the subject for another signifier', that this formula is from the outset mathematical or even could be mathematized. If we turn to Marxism, it served as one of the variants, one of the forms of the epistemological break during the time of the *Cahiers pour l'Analyse*; with respect to the political economy that had preceded it, it was thought of as a Galilean science of economy and of social formations more generally. And yet, the dimension of strict mathematization is practically absent here. The literalized [*littérialisé*] formulas of *Capital* are not mathematical formulas in the strict sense of the term. We could not carry out any mathematical operation with them, whether simple or complex. We could even say that for Marx the notion of surplus value, were it to confront mathematics, appears precisely as irreducible to a calculation. One is tempted to say that economists can calculate everything *except* for surplus value.

We encountered this paradox very early. For my part, I feel like it was not clearly resolved until much later on, even if I now think that the resolution was already latent in the notion of analysis. The resolution itself however, in my case, appeared after the fact, after many years. The notion of analysis means that what is essential in Galilean science is not the use of mathematics. Mathematics is only one of the possible variants of what is essential and what at the time of the *Cahiers pour l'Analyse* we called, precisely, *analysis*. Later, in *Le Périple structural*, I called it – and this clearly represents a displacement – literalization [*littérialisation*].<sup>4</sup> Galilean science exists the moment that there is literalization; mathematization is one of the possible forms of literalization, but it is not the only one. Here we have a general point of view on the notion of Galilean science; I developed it in a particular way with respect to linguistics in my *Introduction à une science du langage*.<sup>5</sup> We could say that with the notion of literalization the relation between science and analysis becomes clear and precise. But since this notion is not yet clear in the *Cahiers*, the relation between science and analysis remains obscure there.

To respond to your last question, psychoanalysis was understood as one of the forms of analysis. The hypothesis of the *Cahiers pour l'Analyse* was that you could find

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<sup>4</sup> Milner, *Le Périple structurale*, 338.

<sup>5</sup> Milner, *Introduction à une science du langage* (Paris: Seuil, 1989).

exactly the same analysis in Freud that you would find in, let's say, Spinoza and that you would find in Marx. I don't think that this position can be fully supported.

KP: I'd like to move now from epistemology to ontology and ask about a remark you make near the end of *Le Périple Structurale*. I quote: 'The most obvious mark of structuralist ontology resides in the inseparability of being and position.'<sup>6</sup> This is a striking, clear, and succinct definition of structuralist ontology. Would you describe the *Cahiers pour l'Analyse* as an ontological project in these terms?

JCM: I think that it was an ontological project, most certainly. You cite *Le Périple structural*; I remark there that in Saussure there was an overturning with respect to the western ontological tradition, since he separates being and unicity.<sup>7</sup> In classical ontology, being and the one went together. Whereas what we find in the *Course in General Linguistics* is the definition of a type of being that is fundamentally not one. And this is in my view what Lacan grasped in defining the signifier as representing the subject for another signifier. This means that the alterity 'for an other' is at the heart of the one of the signifier. Somewhere, I believe it is in 'L'Étourdit', but I am not sure, Lacan posits that the signifier only represents 'for' [*le signifiant ne représente que 'pour'*].<sup>8</sup> This means that the signifier in its being [*en tant qu'être*] cannot be one. It is impossible to think it at the same time as being and as one. There is an ontological project and I think this can be seen in the *Cahiers*. Perhaps I'm mistaken, but it seems to me that the text where this appears most clearly is 'The Point of the Signifier' (CpA 3.5). What I tried to show in this text, however tentatively, is the way in which Plato – who, Lord knows, is the figure most associated, in the philosophical tradition, with the effort to tie being and the one together – proceeds to their disjunction in the *Sophist*. Which is to say that being is under the form of a chain.

KP: It's fascinating to see in 'The Point of the Signifier' the use of a new concept in its earliest development – i.e. 'suture' – in an analysis of Plato. Why this return to Plato, and the texts of *Sophist* and *Parmenides* in particular, in the *Cahiers*?

JCM: On this point, I think that, though the article itself was personal, many shared the interest for what I would call texts of pure logic, whether it was Plato's 'logical' texts, in other words the *Sophist* and the *Parmenides*, or Hegel's *Logic*, or mathematical logic, all the texts of this kind. Our interest in Plato was not a 'return' to Plato. It is not to be separated from other interests: why Russell on Gödel? Why Frege? What interested us in these texts at the time was the possibility of reasoning about terms without any particular substance and making particular or even singular properties appear. Since they didn't depend on a particular substance, these properties must depend on the necessity of the structure itself. This supposes that the structure is itself productive of properties. Without

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<sup>6</sup> Milner, *Le Périple structurale*, 360.

<sup>7</sup> *Ibid.*, 15-57.

<sup>8</sup> See also Lacan, 'The Subversion of the Subject and the Dialectic of Desire': 'My definition of the signifier (there is no other) is as follows: a signifier is what represents the subject to another signifier. This latter signifier is therefore the signifier to which all the other signifiers represent the subject – which means that if this signifier is missing, all the other signifiers represent nothing. For something is only represented to' (E, 819/693-694).

being aware of this at the time, this comes down to admitting that analytic judgments can be productive; from that, the importance of Frege, who tried, in explicit opposition to Kant, to base arithmetic in analytic judgments. This was also the source of Plato's importance: we could show that the Socratic dialogues rely on the hypothesis that in analysing one proposition of his interlocutor, Socrates could find entirely new propositions. In the chain of the *Sophist*, it is by analysing one term that we make new terms appear. We thus rediscover, through another path, the word *analysis*. I see there today what Lakatos would have called a hidden lemma, and which I would summarize, in reference to Kant: 'Certain analytic judgments are productive.' We also rediscover here, through another path, what I would today call hyperstructuralism.

It is true that Russell would surely have rejected this lemma. Russell nonetheless shared a common point with Plato; with him too, you withdraw all particular substance from terms. You reason on terms and show the constraints that are not based in experience and which are, for an empiricist like Russell, just as constraining as empirical constraints. Put differently, the empiricist in the English tradition will say: 'You cannot do whatever you like with an empirical object, it resists.' Well Russell said: 'You cannot do whatever you like with a logical object; it resists.'

KP: Another question concerns the relationship between the concept of number and the concept of being, which was a particular concern for the *Cahiers* at this time. For Badiou, it seems, at best the relationship between mathematics and the ontology that Miller was attempting to develop was an analogy, whereas it seems that Miller wanted to insist that the logic that he was drawing from Frege had an ontological bearing. So I wanted to ask about your thoughts on this because your own position in 'The Point of the Signifier' (CpA 3.5) is not totally clear.

JCM: I think that here there is something that few understood at the time, and it is that I'm talking about Plato in making the hypothesis that Plato did not know what number was, or did not have a complete understanding of it. And why? Because he did not have the concept of zero. This means that when Plato reasoned – and I think that this is an extremely important point – when he reasoned on numbers, he reasoned always in reference to the numbers which could have a geometrical representation, and this is tied to the fact that he did not have the concept of zero. Whence the importance of Frege's text on the zero, and the importance of the questions Leibniz poses: is it that the geometrical point is of the order of the zero or the order of the one? At the time, I had an ontological hypothesis; in my view, the signifier exploded classical ontology and the logic of the signifier announced the formal laws of this upheaval. Though it provided the instruments of thought, mathematical logic remained subordinate in relation to this hypothesis. It seems to me that Miller was close to this position. Badiou, at this time, thought on the contrary that ontological discourse must not free itself from the laws proper to mathematical logic.

I am in complete agreement with your diagnostic concerning the past; I cannot speak for the present period. Badiou has developed his own doctrine, very complete but evolving. For example, between *Being and Event* and *Logics of Worlds* there is a displacement and this displacement is deeper still if we think of the period of the *Cahiers*. As for Jacques-Alain Miller, I don't know what he would say on this question today.

For my part, as I said, in 'The Point of the Signifier' (CpA 3.5) I accord much importance to the fact that Plato, having no idea of zero, was unable to have a complete idea of number. And this means that when he constructs his chain with a finite number of terms, we must understand that this is the homonym of a modern chain wherein there would be one, two, three, four, five terms; it is not the same chain since for Plato 'one' has no predecessor, since there is no zero.

KP: For Heidegger as well, *The Sophist* was a key text, and this relationship between being and non-being was a central concern. Was Heidegger in play, as it were, at the time? Were you were familiar with Heidegger's take on *The Sophist*?

JCM: I believe that, on this score, there is a major difference among the various members of the *Cahiers pour l'Analyse*. Personally, I had no familiarity with Heidegger's texts at the time. Don't forget that I did not have a philosophical education. I was a linguist by training. At the end of the 1960s, my knowledge was classical, which is to say that all I knew were the texts of Plato, Descartes, Kant... and by personal choice, I read a lot of Leibniz and Hegel. Heidegger was not at all familiar to me. I have since read and worked on him, but in a completely different context. By contrast, I know that Jacques-Alain Miller had a profound familiarity with Heidegger.

KP : In your work in the past decades you've held a critical position on the *Cahiers pour l'Analyse*. In *Le Périple structural*, you describe your own personal encounter with Noam Chomsky's work as 'a total reversal of perspective.'<sup>9</sup> You emphasize in particular his abrogation of epistemological minimalism in favour of 'a hypothesis about structure rich enough to account for linguistic acquisition.'<sup>10</sup>

For you, did Chomsky's project, which insists on the physiological and biological components of speech and language, supervene on the efforts of the *Cahiers pour l'Analyse*? Did the reintroduction of *phusis* in Chomsky's project compromise the 'logic of the signifier' developed in the *Cahiers*?

JCM: This was not the point of the reversal. The point of the reversal was precisely to consider that a theory should advance the most complex hypotheses possible. Obviously this sends us back to a Popperian epistemology. From the point of view of the history of ideas, it would be very interesting if someone would investigate the history of Popper's reception. I think that it was in 1960 that things really got underway. In 1960, Popper begins to be known in the English speaking world and it is at this same moment that a separation gets made between Francophone and Anglophone epistemology. Francophone epistemology had been extremely important, even internationally. Koyré's works had exerted a great influence; in particular, they were at the basis of Kuhn's works. In truth, Kuhn was a product of Francophone epistemology. This epistemology had several characteristics and one very important one: its total indifference to logic. Anglophone epistemology on the contrary accorded much importance to the logical form of scientific reasoning. Nonetheless, they shared a common conviction: theoretical minimalism. A scientific theory must rest on the smallest possible number of axioms.

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<sup>9</sup> Milner, *Le Périple structurale*, 375n19.

<sup>10</sup> Ibid., 350.

The translation of Popper into English modified this approach. But, at the time, Popper remained unread in French. A separation was thus established. Anglophone epistemology, in particular in the United States, would take the Popperian path, totally separated from Francophone epistemology, which would persist in its ignorance of Popper. Without knowing it, we were – and here I speak in a general sense, not only of the *Cahiers pour l'Analyse*, but also the Althusserians – we were inscribed in this moment during which Popper was in the process of transforming the horizon of epistemology in the Anglophone world, without us having any real awareness of it at the time.

The book of Chomsky's that I alluded to is *Aspects of the Theory of Syntax*; I read it on the airplane on the way to MIT in September 1966 and I translated it later into French.<sup>11</sup> Chomsky developed a Popperian style epistemology in this book. It is through him and not Popper that I discovered this epistemology and this was the occasion for my total intellectual reversal. Chomsky actually did not mention Popper. No one knows exactly why. Did he not know who he was? In which case, he seems to have rediscovered Popperian theses for himself. Another enigma for the history of ideas. In any event, it was clear that Chomsky had accomplished an important epistemological operation at the heart of linguistics. He withdrew from linguistic structuralism the evidence it had enjoyed up to that point; one of the ways he did this was in referring linguistic structuralism – and I think he was right on this point – to an epistemology of a minimalist variety.

Having done this, Chomsky was able to point out two facts: (a) that structuralism availed itself of an epistemology without knowing it, (b) that this epistemology was not self-evident. Then he argued that in truth one can construct an epistemology that was exactly the inverse, maximalist and not minimalist. Here, from the point of view of the maximalist epistemologist, structuralism appears as a weak, meagre enterprise. Since it is not really falsifiable! This was the case above all if linguistic structuralism was not hyperstructuralist; for as long as one considered that a given structure [*une structure quelconque*] did not have properties, then one was incapable of defining the properties of a structure in general. And this meant that the notion of structure, in itself, became an empty notion.

This was the true reversal for me, what I talked about in *Le Périple structural*. I was educated as a minimalist epistemologist, I discovered that this did not go without saying. After this, Chomskyan linguistics witnessed a development that was more and more naturalist, but this was not the reversal for me. Let's be clear. For me, I had been very sceptical of these developments when I was working in the Chomskyan framework. Not for reasons of right or legitimacy because, after all, sure, why not? Why not say that linguistic structures are inscribed in the body? That's not what shocked me. My objection – I've put this in writing – was that to the extent that we cannot give experimental proofs, then, to say that it is physiological is simply to baptize objects. To say that the transformations – the linguistic transformations that Chomsky will ultimately abandon, but that's another story – represent, in the last instance, neurobiological processes, why not? But when we don't have observations or admissible proofs from neurobiology, this is just talk. And this is the source of the extreme scepticism that, beyond Chomsky, I have sustained against cognitivism, which seems to me to be a way of avoiding the question of empirical proof. It so happens that I've attended discussions between the representatives

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<sup>11</sup> Noam Chomsky, *Aspects de la théorie syntaxique*, trans. Jean-Claude Milner (Paris: Seuil, 1971).

of neurosciences and cognitivists. The representatives of neuroscience say to the cognitivists: 'Come on, you're just telling stories! We want to see some observations that neuroscience can accept.' The cognitivists then say, 'No! Since what we are constituting is the set of rules that allows to account for observable behaviours.'

KP: 'The conditions of possibility.'

JCM: That's it. I said to myself: this is Cartesian reasoning. It simply led Descartes to construct a physics that revealed itself inconsistent and a physiology that revealed itself inconsistent. But I come back to maximalist epistemology. Just as a neurophysiological or neurobiological approach has little effect with regard to the approach found in the *Cahiers pour l'Analyse* – it was not our concern – so too does the emergence of a maximalist epistemology constitute a valid objection. In other words, there is a question to be asked: is the 'Analysis' such as it appeared in the title of *Cahiers pour l'Analyse* an analysis that was going to shoot for minimalism, that is to say the smallest possible numbers of principles for obtaining the largest number of consequences, or ought it be maximalist, with the largest possible number of hypothesis so that falsification would be possible? Historically, it was the first path that was chosen. It could not have been otherwise since Popper was unknown to us.

KP: I think that it's true that the dominant theme in the *Cahiers pour l'Analyse* was an effort toward a minimal number of suppositions for the maximal application. But it seems like there was some resistance, even within the journal itself and I'm thinking in particular of André Green's contribution (CpA 3.2), where he tries to reintroduce a notion of affect, which has some sort of physiological element. But it does seem that there's a firmer move toward formalization in later issues, that this effort to introduce affect goes away, and that Leclaire effectively wins over Green. Do you have any comments on Green's contribution?

JCM: I think that your description is right. That was what happened. Leclaire had certainly sought not to lose the finesse of empirical analysis of psychic phenomena, but at the same time he did not want to lose the horizon of epistemological minimalism. This is what made the thing interesting and at the same time, even if it was exterior circumstances that brought the *Cahiers pour l'Analyse* to a halt, I think that the tensions between the two sides would have become insurmountable very quickly.

Today, I would distinguish two types of figures: either there is an instance of falsification or there is not. I would say that when there is no instance of falsification, the only weapon that we have is minimalism. On the other hand, when falsification is possible, we should reason in Popperian terms. Now, there are domains where falsification is impossible, where there is no instance of falsification. I'm thinking for example of the Darwinian hypothesis of natural selection. I doubt that it is falsifiable, in the strict sense of the term. Its force lies in its minimalism; in other words, minimalism reclaims its rights.

If you look at the work of Freud, which Popper treated rather disdainfully, minimalism is the horizon, for example when in the *Interpretation of Dreams* he settles for a negative denomination for the unconscious; it's that he doesn't have access to an

instance of falsification that would allow him to give a positive characterization of it. That said, we find in Freud a nice example of Popperian reasoning in the *fort/da* game. This is reasoning by falsification, and it is a maximalist epistemology since Freud departs from a system that had one single principle, the pleasure principle, to go toward a system wherein the pleasure principle no longer sufficed. This means that the criterion of the minimum is not sustainable. And why is it not sustainable? Because the child's game falsifies the theory in which the pleasure principle is the only one that exists.

KP: That's a good example. But couldn't we perhaps say that, more than an instance of Popperian reasoning, Freud's reasoning 'beyond the pleasure principle' is almost more Lakatosian. I wanted to ask you something about the relationship between Lakatos and Popper – I know that this takes us away from the *Cahiers pour l'Analyse* a bit, but you've addressed this relation in your work – and Lakatos's critique of Popper's strong falsificationist principle. Lakatos has an image I like. He says it's not that nature shouts 'no!' to the scientist's theorem, but instead 'inconsistent!'<sup>12</sup> The theory is forced to undergo revision and change, but it is not wholly falsified. This is an image which itself evokes Cavallès's insistence on the notion of 'erasure' [*rature*] in science's development.<sup>13</sup> For Lakatos, no theorem will ever be complete or finished; it will always be interrupted.

In your chapter, 'The Doctrine of Science' in *L'Œuvre claire* you present an evocative image that aligns the Lacanian unconscious with the infinite as an intrinsic property of the universe: 'The infinite is that which says no the exception of finitude; the unconscious is that which says no to the privilege of self-consciousness [*la conscience de soi comme privilege*].'<sup>14</sup> For Lakatos, nature, by shouting 'inconsistent,' questions and supports at the same time the self-conscious subject of science. It seems that the project of the *Cahiers pour l'Analyse* sought to show how the subject is at the same time buttressed and tenuous in its own constitution in similar terms.

JCM: Yes, I would not be against this distant echo, since at that very moment, if we could have had access to Lakatos, that would have excited us. But these are the misfires of history. I am not against this echo, but I would like to remind you that *L'Œuvre claire* is not in the *Cahiers pour l'Analyse*. Reciprocally, my object in *L'Œuvre claire* was not to re-examine the *Cahiers*. So what you say, yes, I do not disagree... with a simple addition. I'll present it in my own terms, but don't think I'm betraying what was presented under other pens than mine in the project. This is what I would like to add: the convocation that was at work at the time, was that the moment of what says 'no' is itself sutured. This is where you got the mode of reading, for example, which consisted in searching a text for the point where something was in some way passed over in silence, and to affirm that this is where the essential occurred. So in effect there is a relation with Lakatosian epistemology, except that the point of inconsistency that one discovers and that was hidden is also immediately the condition of the apparent consistency of what was visible.

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<sup>12</sup> See Imre Lakatos, 'Falsification and the Methodology of Scientific Research Programmes' [1970] in Idem., *The Methodology of Scientific Research Programmes*, eds. John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978), 8-101.

<sup>13</sup> Cf. Jean Cavallès, *Sur la logique et la théorie de la science* (Paris: PUF, 1946).

<sup>14</sup> Milner, *L'Œuvre claire*, 66.

KP: You've said that *L'Œuvre claire* is not about the *Cahiers pour l'Analyse*, but it does have the sense of a balance sheet, a looking back. And I would say that there are lines of continuity between the two, for example, in this notion of contingency becoming necessity and the subject as being the site where that happens. It seems to me that that's also in 'The Point of the Signifier' (CpA 3.5), that that's something you're trying to work through as a very young man and that you come back to in *L'Œuvre claire*. And again, you bring in Popper, and you have this wonderful phrase where you say he presents us with a version of science wherein 'the referent must be able to be – logically or materially – other than it is. But that's contingency. [...] The set of contingencies as science grasps them, in theory and practice, is the universe.'<sup>15</sup> So from there, I want to ask you if you think that this project of the *Cahiers* has anything contemporary about it. Are these questions still in play today in French intellectual life? And is there a way to read your efforts then with your current work and develop some new questions out of this project?

JCM: I would say that this is true for the *L'Œuvre claire*, since you alluded to it, but this is true for many of the things that I have written. When I wrote *L'Œuvre claire*, I had studied a lot of Lakatos, Holton, Feyerabend, Quine, Duhem; this literature was much discussed in the milieu of Chomskyan linguistics. But in a more particular manner, I was very impressed with the book of Ernst Mach on the physics of Newton.<sup>16</sup> In sum, what I wanted to do with *L'Œuvre claire* was a rereading of Lacan in the manner that Ernst Mach had reread Newton, in isolating the axioms, in examining if they are inconsistent etc. Why did I do this? Simply because I asked myself the question: Does this stand up to it? Does something remain of Lacan if we submit it to a questioning of the Ernst Mach sort? Can we formulate a finite number of fundamental propositions? Are these propositions consistent within themselves? Is there one or a number of inconsistent points? In fact, *L'Œuvre claire* was an attempt to submit Lacan to examination, but this is an examination that Lacan, I dare to say, passed. I concluded that I was not mistaken at the time of the *Cahiers pour l'Analyse* in giving importance to Lacan.

If I make this remark, it is because not all the texts that were important to me at the time of the *Cahiers pour l'Analyse* were submitted to the test in a positive way. For example, it is clear to me, rereading the texts of Althusser, that if I passed them through the Ernst Mach test they would fall apart. This is also true, in my eyes, for Canguilhem. I would say the same thing for Koyré. There is a historical part that is absolutely important, but the general propositions seem to me today to be extremely weak. This is to say that not all the points of references you find in the *Cahiers pour l'Analyse* necessarily stand up to re-examination. Lacan, yes, very certainly. Marx, very certainly also, even if I interpret Marx in a manner totally different from mine at the time. So, to respond to your question, you are right in judging that much that I have done in recent years is a re-examination of questions that were already present for me at the time of the *Cahiers pour l'Analyse*, but the response is not always of the same nature. In certain cases, it stands up to examination and in certain cases the results are unfavourable.

KP: So would you say that we need to 'find new answers for old questions'?

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<sup>15</sup> Ibid., 61.

<sup>16</sup> Ernst Mach, *The Science of Mechanics* [1883] (La Salle, IL: Open Court, 1989).

JCM: Maybe, or that from old questions, we should build new questions. Since you pose the question of intellectual life as it is today, I think that it has turned away from the questions posed at the time of the *Cahiers pour l'Analyse*. For example, I believe that all ontological questions are totally out of play. I think that the will to pose questions on the productive character of a structure, all these kinds of question no longer command attention. I even feel that the general mode of questioning which was that of the *Cahiers pour l'Analyse* is a mode of questioning that has become very distant. I said that we were in a world where Popper did not exist and now Popper does. I do not mean that he has the last word, but he exists. We must also not forget that at the moment we were writing – this is also true for Lacan and Foucault – French was a vibrant intellectual language and still had a certain audience. 'French Theory' existed. I think this is no longer true today. I think that what is said in French, by the fact alone of being said in French, is inaudible. To say it another way, I think that French is a dead language.

KP: Though it must give you some hope that a few Anglophones are at least discussing the *Cahiers pour l'Analyse*...

JCM: Yes, yes. I am very happy about this. But this is to say that there is a displacement.

KP: There's clearly a wistful sense at the end of *Le Périple structurale* and you introduce this pessimism where you say it's as if this type of questioning never happened, or a pessimist would think that it never happened.<sup>17</sup> Would it even be desirable in your view for these ontological questions to come back, to be discussed again?

JCM: I'm not fond of 'comebacks.' You know, Busby Berkeley once wrote, before he tried to kill himself in fact, 'there is no comeback for a has been'. In fact there is no 'comeback' *except* for the 'has been'. But what I mean is that what was called 'French Theory' and had enjoyed a certain celebrity in the Anglophone world only represents a portion of the things that were going on. This is the first point. The second point, and here I have deliberately not brought it up since its demonstration would be too long, I now think that among the great authors, the great discursive interventions in French in the latter part of the 20<sup>th</sup> century, that one of the greatest will have been that of Foucault. More important, in my view, than Deleuze.

Now, the world of the *Cahiers pour l'Analyse* was a world in which Foucault and Deleuze were just beginning to exist. They had just published major books, but all the same the essential was still to come. And I believe that Foucault constitutes one of the very important shifts of the 20<sup>th</sup> century. I think that, even on the question of ontology, he could have been a fundamental interlocutor, but things did not happen like that; the *Cahiers pour l'Analyse* were interrupted, and then, from a certain moment, he himself did not want to have an interlocutor in France.

KP: Deleuze is an interesting case, because it seems only now, in the Anglophone context, that people are reading *Difference and Repetition* as a work deeply engaged with structuralism. But I have the sense that Deleuze wasn't really being read by 'structuralists' at the time.

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<sup>17</sup> Milner, *Le Périple structurale*, 368.

JCM: You are right. Deleuze began to be important later on, at a moment when structuralism had begun to decline.

KP: It seems to me that the *Cahiers pour l'Analyse* is a chapter in a much longer French history, the history of a certain rationalist way of thinking that resurges from time to time.

JCM: Yes, I think that there is continuity in which Descartes indeed plays a part. But I would not put it in terms of rationalism. I would say that there is in the French tradition a sort of prose of thought, but we desire or need these writings to encounter questions along the way that make prose explode. I think of Descartes' *Meditations* and Pascal's *Pensées*; I could add Cavaillès's last text. This is not German systematicity, which is architectural; this is on the order of the sequence or chain [*enchaînement*] of reasons, which is linear. This is also not the pleasing coordination of English (I think of Locke, Berkeley or Hume) which avoids, at least in appearance, concatenations and points of explosions. In the continuity that I am thinking of, the effect of the sequence remains, regardless of the length of the chain; it remains even if the chain at one point has an encounter with an element that breaks it. Yes, I think that there is something of this order that appears in the *Cahiers pour l'Analyse*, extending to their interest for the notion of a signifying chain.

Yes then to a continuity, but this must be corrected by taking into account particular historical circumstances. The *Cahiers pour l'Analyse* were produced by very young people who came directly from their university education; they testify to something that happened in certain places in the French university at the time. I described this moment in *Le Périple structural*; theory, which had had Germany and the German language as its privileged place, was in certain respects displaced or errant, with no longer any place nor language. I think that with Althusser, there was the will to make the French language, and in the French language Paris, and in Paris the university, and in the university the École Normale of the rue d'Ulm, and in the École Normale of the rue d'Ulm his own seminar, his own teaching, become the place of reception for this errant theory. We were seized by this moment; even if the *Cahiers pour l'Analyse* depended on Lacan and not Althusser, I believe they were animated by a conviction that came to them from Althusser: the tranquil conviction that France had by then already become the natural language of the concept. This was at once a moment for the French language and a moment for what I am calling the wandering [*errance*] of theory, a wandering both geographic and linguistic, born from the observation that the German language could no longer be the language of the concept, because of 1933, and the conviction that the English language had long since ceded to market forms. Need I add that I now think, as I speak to you today, that these games around languages arise from, or amount to, a mirage?<sup>18</sup>

Translated by Tzuchien Tho.

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<sup>18</sup> The French reads: '*Dois-je préciser que ces jeux autour des langues relèvent, selon moi, au moment où je vous parle, du mirage?*'