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MATHEMATICAL THINKING

Jean Cavaillès & Albert Lautman

Introduction and translation[1]
by
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On February 4, 1939, Jean Cavaillès and Albert Lautman were invited to present their recent works on the philosophy of mathematics to the *Société de Philosophie Française* (SFP), which organized lectures followed by debates in front of a selected panel of scholars.

The *Société française de philosophie*, founded in 1901, was established a few years after the *Revue de métaphysique et de morale* (1893) by its founders, Xavier Léon, Élie Halévy, Léon Brunschvicg, and others, who had also organized the first *World Congress of Philosophy* in Paris in 1900 and were instrumental in founding the *International Institute of Philosophy* in 1937. The simultaneous creation of a national forum and a series of international meetings reflects the open-mindedness of a modern rationalism, which, while remaining faithful to the metaphysical tradition, sought to engage with the contemporary situation marked by the intensification of scientific progress and the widespread socio-political crises. Among many other philosophers Russell, Dewey, Piaget, Husserl, Cassirer, Reichenbach, Lukacs... were invited as well as scientists such as Perrin, Langevin, Einstein, de Broglie and Lichnerowicz.

An examination of the letters from Cavaillès to Lautman[2] reveals that it was on Brunschvicg's initiative that the two philosophers were invited by the *Société française de philosophie*, and that together they chose the title of the session: "La pensée mathématique" ("Mathematical thought").

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(1) The editors would like to express their profound gratitude to Brendan Larvor for his valuable assistance with the translation of Cavaillès and Lautman's intervention before the French Society of Philosophy in 1939.

(2) Lettres inédites de Jean Cavaillès à Albert Lautman, *Revue d'histoire des sciences*, 1987, tome 40, n.1, p.117-129.

The two philosophers defended their theses in the same academic year 1937-1938, despite the 5 years that separated them. Lautman defended his theses entitled *Essai sur les notions de structure et d'existence en mathématiques*(3) (main thesis) and *Essai sur l'unité des sciences mathématiques dans leur développement actuel,*(4) the 18 December 1937, while Cavaillès presented his two theses the 22 January 1938, entitled *Méthode axiomatique et formalisme*(5) (main thesis) and *Remarques sur la formation de la théorie abstraite des ensembles*(6).

Remarkably, Élie Cartan was president of the juries for both theses and took part in the discussion before the SFP.

The text of their presentation to the *Société de Philosophie* is part of the *Grandes Conférences* published online by the *Société*.(7) The one we present here for the first time in an English translation is available in French under the title "*La pensée mathématique*" and contains, in addition to the contributions of the two philosophers, the discussion with the mathematicians Élie Cartan, Paul Lévy, Maurice Fréchet, Charles Ehresmann, Claude Chabauty, Paul Dubreil and the philosophers Jean Hyppolite and Paul Schrecker.

True philosophers of mathematical practice before it was reinvented, these two intellectuals forge their philosophy by attempting to integrate into it a form of irreducibility of mathematics, a key point of convergence between the perspectives of Cavaillès and Lautman that will shape their close relationship to mathematics.

Both agree that mathematics cannot be reduced to its objects, as Lautman summarizes, due to the "interconnection (*solidarité*) that unites the nature of the mathematical object with the singular experience of its elaboration over time". Thus, mathematics can only be understood in its becoming (*devenir*), i.e. through the analysis of mathematical practice (*experience*), conceived as the elaboration over time of structures, procedures and notions for the construction of new theories.

(3) "Essay on the notions of structure and existence in mathematics" *in Mathematics, Ideas and the physical Real.* Albert Lautman. Translated by Simon B. Duffy. Continuum International Publishing Group, 2011.

(4) "Essay on the unity of the mathematical sciences in their current development" *in Mathematics, Ideas and the physical Real.* Albert Lautman. Translated by Simon B. Duffy. Continuum International Publishing Group, 2011.

(5) "Axiomatic method and formalism" (There is no English translation of this text)

(6) "Remarks on the formation of abstract set theory" (There is no English translation of this text)

(7) `https://www.sofrphilo.fr/activites-scientifiques-de-la-sfp/conferences/grandes-conferences-en-telechargement/`

However, the two philosophers analyze the profound nature of this irreducibility in their own ways.

For Cavaillès, irreducibility appears on several levels:

1. Mathematics cannot be reduced to logic, because "in attempting to fully formalize Mathematics, we have come to the conclusion that not all the procedures (*procédés*) we use can reasonably be called logical."

2. It is "impossible to fit all Mathematics into a single formal system", according to Gödel' first incompleteness theorem from 1931.

3. "It is [...] absurd to define Mathematics as a set of hypothetico-deductive systems, since, in order to characterize these formal systems as deductive systems, we must already use mathematics". This point illustrates, again according to Gödel's second incompleteness theorem, that mathematics cannot be reduced to "an assembly of formal systems that are arbitrarily constructed and can be juxtaposed, thus constituting the whole of Mathematics".

4. As a "singular becoming", "it is impossible to reduce [mathematics] to anything other than itself". This is expressed in two ideas at the heart of Cavaillès' thinking: upstream, where "the notions introduced are required by the solution of a problem, and, by virtue of their mere presence among previous notions, they in turn pose new problems"; and downstream, where "there really is becoming: the mathematician is embarked on an adventure that he can only stop arbitrarily, and every moment of which provides him with a radical novelty".

For Lautman, the idea that "the results obtained [in mathematics] are organized under the unity of certain themes" can only be explained by the fact that mathematics "partakes of a common Dialectics that dominates them". This irreducibility stems from the fact that, as far as ideas are concerned, "Dialectics is a pure problematic, a sketch of schemes whose design needs to take shape on a particular mathematical material in order to assert itself". Therefore, Dialectics reveals both its "essential insufficiency" and its "exteriority to the temporal development of scientific concepts". On the other hand, "mathematics presents itself first and foremost as example of incarnations, domains in which the ideal expectation of possible relations is actualized".

Thus, for Lautman, "the problems of Dialectics are conceivable and formulable independently of Mathematics, but any outline of a solution to these problems is necessarily based on some mathematical examples designed to concretely support the dialectical connection under study". Irreducibility is thus established in the articulation between the "Idea" and the mathematical "Reality" to which it gives rise.

Hence, for Cavaillès and Lautman, philosophy of mathematics cannot be done without a detailed knowledge of the mathematics of its time, but also a reflection on the intellectual experience that constitutes it, and which it is the mission of philosophy to clarify.

However, two major points of disagreement emerged throughout the session.

The first concerns the question of the autonomy of mathematics, understood both in the sense of independence from all external influence and as the linear necessity of mathematical progress, the infinite unfolding of problems and the necessary and unique solutions to them.

Cavaillès affirms this autonomy in the sense that epistemological work can show, in the historical sequence of problems and solutions, the autonomy of their elaborations from all contingencies external to mathematics, and in such a way that it is even possible to outline the procedures that preside over this engendering in mathematical experience (i.e., thematization and idealization).

On the contrary, Lautman denies this autonomy, because of the plurality intrinsic to the unfolding of mathematics, which cannot be reduced to a tree-like sequence of solutions determined by sequences of problems. There is in fact a plurality of possible structures that manifest themselves in history, differing in their meaning and the scope of their applications, and calling for a more organic analysis of their structural analogies and connections. In this sense, Lautman's idea that mathematics is not reducible to the activity of problem solving seems interesting for contemporary debate.

The second point of disagreement concerns the question of existence.

For Cavaillès, the existence of mathematical objects can only be asserted in a weak sense: they are merely the correlate, in actual and sensible mathematical experience, of the structures that the mind generates.

For Lautman, they exist in a much stronger, objective sense, as they can be revealed in different analogous structures where their existence is induced by global considerations, with "organic" effects on mathematics as a whole.

The text that follows is divided into three parts.

In the first, summaries of the respective theses of Cavaillès and Lautman are presented in the third person. In the second part, the two authors develop their positions one after the other. The third part is an account of the debate. First, those present address their remarks to one and/or the other speaker. Then Cavaillès and Lautman respond in turn, deepening their dialogue: the aspects that unite and divide them are again taken up by our two authors at the end of their response session.

While the two philosophers' presentations enabled them to clarify the ideas they had developed in their theses, the mathematicians' reactions were just as illuminating in terms of how these scientists perceived the philosophical questions forged in contact with mathematics. The mathematicians' reception of the presentations shows both the proximity of the two fields of mathematics and philosophy, and the differences in the practices of researchers in the two domains. In particular, Lautman's positions were criticized and misunderstood at various points, especially in terms of his "transcendental" dimension, which he saw as enabling "the genesis of the [mathematical] Real from the Idea".

We are convinced that it is useful to circulate this text more widely, and to open with it this double issue of *Annals of mathematics and philosophy*, which we would like to highlight the path of a philosophy inspired by a tradition whose true nature is embodied here by Cavaillès and Lautman.

Our aim is also to show that the positions of Cavaillès and Lautman set out in this 1939 conference have an important current echo, as attested by very recent publications among which the proceedings of the colloquium "Albert Lautman : philosophie, mathématiques, Résistance", published by Éditions Rue d'Ulm.[8]

This text undoubtedly reveals a philosophical debate that marks a new direction compared to the dominant approaches that have spread worldwide. It is time to fully grasp this shift. Contemporary

[8] Eckes, C., Jaëck, F., Mélès, B., & Szczeciniarz, J.-J. (Éds.), *Albert Lautman philosophe, des mathématiques à la Résistance*, Paris, Éditions Rue d'Ulm, à paraître 2025.

philosophy ought now to take advantage of this promising path and engage with it vigorously.

MATHEMATICAL THINKING

February 4, 1939

Two theses of the utmost importance were recently defended before the Faculty of Letters of the University of Paris on the philosophy of Mathematics considered at the point of development Mathematics has now reached. The Société de Philosophie felt it would be useful to discuss them simultaneously, and would like to thank their authors for their willingness to do so.

Mr. Cavaillès takes as his starting point the problem of the foundations of Mathematics as it is currently posed and partly resolved. The result of the Crisis in Set Theory, following on from the work of Bertrand Russell and Hilbert, is to transform the epistemological problem into a mathematical one, subject to the usual technical sanctions. Two conceptions of Mathematics have thus been eliminated:

1. Logicism ("Mathematics is a part of Logic"), because the effective formalization of Mathematics has brought about:
   (a) That in reality, no purely logical notions or operations were involved (the problem of the meaning of such notions and operations being left aside), but that the approaches employed [*considérations utilisées*] are all homogeneous and belong to combinatorial calculus or other mathematical theories (the meaning of a symbol is its mode of use in a formal system);
   (b) That it is impossible, by virtue of Gödel's theorem, to fit Mathematics into a single formal system: any system containing Arithmetic is necessarily unsaturated (i.e. it is possible to construct a proposition that is neither provable nor refutable in the system).
2. The hypothetico-deductive conception, presented with maximum precision by von Neumann's radical formalism. Indeed, it is impossible to characterize a mathematical theory — a system of axioms and arbitrary rules (according to this conception) — as a deductive system without using established mathematical theories not previously characterized in this way (e.g., for Number theory, Gentzen's proof of

non-contradiction using transfinite induction (*recurrence transfinie*). In other words: essential interconnexion [*solidarité*[9]] between the parts of Mathematics, with the impossibility of a regression providing an absolute beginning.

Mr. Cavaillès is then led to the following statements:

1° Mathematics constitutes a singular becoming [*devenir singulier*]. Not only is it impossible to reduce Mathematics to anything other than itself, but any definition at a given time is relative to that time, i.e., to the history of which it is the outcome: there is no eternal definition. To speak of Mathematics can only be to remake it. This becoming seems autonomous: it seems possible for epistemologists to find a necessary sequence behind the historical accidents; the notions introduced are required for the solution of a problem, and, by virtue of their mere presence among previous notions, they in turn pose new problems. There really is becoming: the mathematician is embarked on an adventure that can only be stopped arbitrarily, and every moment of which is a radical novelty.

2° The resolution of a problem has all the characteristics of an *experience*: a construction subject to the sanction of a possible failure, but accomplished in accordance with a rule (i.e. a *reproducible* construction, therefore *not an event*), and finally taking place in the sensible. Operations and rules only make sense in relation to an earlier mathematical system: there is no thought-out representation (distinct from mere lived experience [*pur vécu*]) that is not a mathematical system insofar as it is thought, — i.e., a regulated organization of the sensible (by virtue of the continuity between mathematical gestures beginning with the most elementary).

3° The existence of objects is correlative to the actualization of a method, and as such, not categorical, but always dependent on the fundamental experience of effective thought. The illusion of the possibility of exhaustive description (or generation *ex nihilo*) through axioms, unmasked by Skolem's paradox, is explained by the necessary gap between exposition and authentic thought. To express the latter, that is the central intuition of a method, would require completed Mathematics (making explicit all the successive requirements). Objects are projections in the representation of the

[9] Editors' note: "Solidarity" in French will be used 6 times throughout the text, and is best interpreted in its linguistic dimension, as one element cannot be conceived without the other.

stages of a dialectical development: for each of them, there is a criterion of evidence conditioned by the method itself (e.g. the evidence proper to transfinite induction). They are therefore neither objects in themselves [*ni en soi*] nor located in the world of lived experience [*ni dans le monde du vécu*], but are rather the very reality of the act of knowing.

Mr. Lautman fully agrees with Mr. Cavaillès on the interconnexion that unites the nature of the mathematical object with the singular experience of its elaboration over time. True and false can only be determined in the sense of effective Mathematics, and truth is immanent to rigorous proof. But at this point, Mr. Lautman departs from Mr. Cavaillès. If we accept that the manifestation of an actual existent [*un existant en acte*] only takes on its full meaning as an answer to a prior problem concerning the possibility of this existent, then the establishment of effective mathematical relations appears, in fact, as rationally posterior to the problem of the possibility of such connections in general. A study of the development of contemporary Mathematics shows, moreover, how the results obtained are organized under the unity of certain themes, which philosophers interpret in terms of possible links between the notions of an ideal Dialectics: the penetration of topological methods in differential geometry addresses the problem of the relationship between the local and the global, the *whole* and the *part*; duality theorems in topology study the reduction of *extrinsic* properties of situation into *intrinsic* properties of structure; the calculus of variations determines the *existence of* a mathematical being by the exceptional properties that allow its *selection*; analytic Number Theory shows the role of the *continuous* in the study of the *discontinuous*, etc.

It so happens that affinities of logical structure enable different mathematical theories to be brought closer together, as they each provide a different outline of a solution to the same dialectical problem. Thus, for example, Field Theory, where a system of axioms is realized in mathematical logic, and the theory of the Representation theory of abstract groups, both enable us to observe how, in Mathematics, the passage from a *formal* system to its *material* realizations takes place. In this sense, we can speak of the partaking of distinct mathematical theories in a common Dialectics that dominates them.

The Ideas of such a Dialectics must be conceived as Ideas of possible relations between abstract notions, and their knowledge is not affirmative of any actual situation. As a delimitation of the field of the possible, Dialectics is pure problematization [*problématique pure*[10]], a sketch of schemes whose design needs to take shape on a particular mathematical material in order to assert itself. Only this indeterminacy of the Dialectics, which reveals its essential inadequacy [*insuffisance essentielle*], at the same time ensures its exteriority to the temporal becoming of scientific concepts.

In conclusion, we can specify the links between Dialectics and Mathematics. First and foremost, Mathematics presents itself as an example of incarnation, a domain in which the ideal expectation of possible relations is actualized, but these are privileged examples whose appearance is, as it were, necessary. Indeed, any effort to deepen our knowledge of the Ideas naturally extends into effective mathematical constructions, by the very fact that this effort is concerned with analysis. Mathematical thinking thus has the eminent role of offering philosophers the constantly renewed spectacle of the genesis of the Real from the Idea.

MEETING MINUTES

Mr. Cavaillès. — The reflections I'd like to present take place at a given moment in the development of Mathematics, i.e., at the present time. Because of the very singularity of this moment, they comprise two parts, which I have distinguished in the summary that has been sent to you: the first part includes the results that Mathematics itself has given us on the philosophical problem of the essence of mathematical thinking; all we have to do is translate and explain this first part; we may perhaps discuss the scope of the results, but I believe that this is the indisputable part that I am proposing.

But this indisputable part is negative, and so I propose, after briefly summarizing it, to introduce some positive reflections that stem from the results obtained, as well as onto the current development of Mathematics as we see it unfolding before our eyes.

I won't dwell too much on the first part: in particular, I don't want now to link it, as precisely as I should, with the earlier stages of Mathematical Philosophy [*Philosophie mathématique*], especially

[10] Editors' note: Lautman seems to use the French noun "*problématique*" in the sense used by Bachelard, in *Le rationnalisme appliqué* (1941), third edition 1966, in particular pp 68-73. As in English the word "problematic" is an adjective and has a pejorative sense, we have chosen to translate "*problématique*" by "problematization", conceived as the rational art of posing empirical or theoretical problems in order to develop our knowledge.

in the XIX$^{\text{th}}$ century. I'll just say that, in the Mathematics of the XIX$^{\text{th}}$ century, the very development of the various branches of Mathematics and the need to abandon the intuitive evidence to which we had previously resorted led us to emphasize the notion of proof. Evidence gave way to provability. Hence the idea, widespread among almost all mathematicians and found in researchers as different as Frege and Dedekind, that Mathematics is a part of Logic. Indeed, what guarantees results is the rigorous nature of the chain of reasoning by which they are established.

At this time, therefore, there was an effort to reduce not only all mathematicians' procedures, but also the notions they used, to purely logical procedures and notions; an effort that was helped by the development of Set Theory, and which, moreover, partly provoked it.

We can see how this rapprochement was possible, since the notion of set itself seemed the furthest from any intuition, and since, on the other hand, it could be confused with the notion of class, or extension. Still in 1907, Zermelo, at the beginning of his "Axiomatization of Set Theory", wrote: "Set Theory is the branch of Mathematics to which it falls to study mathematically the fundamental concepts of number, order and function in their primitive simplicity, and, by so doing, to develop the logical foundations of the whole of Arithmetic and Analysis."

Here again, we can see how, until 1907, i.e., after the appearance of the greatest paradoxes, a Set theorist like Zermelo still hoped to base Mathematics — i.e., Arithmetic and Analysis — on a purely logical notion.

This hope was dashed, not so much as a result of the difficulties that Set Theory encountered at the time with the discovery of antinomies, but as a result of the effort that mathematicians themselves made to decide whether or not this hope could be realized, i.e., the effort by which they transformed a philosophical conception of Mathematics into a technical problem for mathematicians.

In fact, when we set out to define the notion of set and the subsequent theory, we came up against the need to axiomatize this theory, i.e., to list the fundamental notions and procedures employed. We thus found ourselves in the presence of technical problems to which a precise answer could be found. This work was carried out by Russell's and Hilbert's schools, and in France, one of its initiators was Jacques Herbrand with his outstanding vigorous work. For those who knew him, both philosophers and mathematicians, his absence is still sorely felt today.

In my summary, I set out the results. Since we were dealing with a problem that could be solved mathematically, two fundamental conceptions of Mathematics were rejected:

1° The conception I mentioned at the beginning, the famous hope of reducing Mathematics to Logic. Logicism is eliminated. I won't insist on the reasons, I'll note them in my summary, and I'll also take the liberty of referring you, for the details, to my book: *Méthode axiomatique et formalisme*.

In attempting to fully formalize Mathematics, we have come to the conclusion that not all the procedures [*proceeds*] we use can reasonably be called logical. I think it would be imprudent to enter into a debate on the very essence of logical thinking, as this would take us too far. I can at least point out that, if we formalize arithmetic, we have to bring in the principle of complete induction, which can hardly be reduced to a system of logical notions.

2° It is impossible to fit all Mathematics into a single formal system. This is the result of a theorem in Gödel's 1931 memoir.

There's another possible conception: the famous old conception of the hypothetico-deductive system. This is no longer a single formal system, but an assembly of formal systems that are arbitrarily constructed and can be juxtaposed to form the whole of Mathematics.

This hypothetico-deductive conception is also rendered impossible by another theorem published by Gödel in the same memoir: "The non-contradiction of a formal mathematical system containing the Number Theory can only be proved by mathematical means not representable in this system." It is therefore absurd to define Mathematics as a set of hypothetico-deductive systems, since in order to characterize these formal systems as deductive systems, Mathematics must already be employed.

In particular, if we consider the formal system representing Number Theory, we have a characterization of this system as a deductive system. To characterize a system as a deductive system is to show that not everything in it can be proved, that is, to prove its non-contradiction. We now have a proof of it by Gentzen, using transfinite induction, a mathematical procedure outside number theory.

I mentioned that the most precise conception of a hypothetico-deductive representation was that of von Neumann. The idea of

Hilbert's school was the following: obviously, we need mathematical notions to characterize a formal system, but these notions are very elementary. In Hilbert's hypothetico-deductive system of axioms for Euclidean Geometry, the notions are very simple: finite whole numbers, mappings. This is illusory, because the non-contradiction of Hilbert's axioms in Euclidean geometry could only be proved by the construction of a system borrowed from Number Theory, and for the latter in turn, we are obliged to appeal to such a transfinite induction.

These are the results. The philosopher can now ask himself, also in the presence of the current development of Mathematics, what positive conclusions he can draw.

I'd like to make it clear from the outset that I don't claim to be giving these conclusions a definitive form. It's a very difficult work, and I'm only offering you some reflections for the time being, reflections that are still somewhat impregnated with the effort of work, and I'm only indicating now the points on which I believe I've arrived at the maximum degree of certainty.

First point: the idea of defining Mathematics seems to me to be rejected, both because of the results I've just pointed out, and because of the very reflection on works of mathematicians.

Mathematics is a becoming, a reality that cannot be reduced to anything other than itself. What does defining Mathematics mean? It's either to say that Mathematics is that which is not mathematical, which is absurd, or to list the procedures used by mathematicians.

I'll leave out the first solution, although it has had, and still has, its supporters. The second remains. I don't think any mathematician would agree to a definitive, exhaustive list of the procedures he employs. We can list them at a given moment, but it's absurd to say: this is all Mathematics, and if we don't use these procedures, we won't be doing any more Mathematics. I believe that I am in agreement here, on the one hand, with the results obtained, such as the necessarily unsaturated character of any mathematical theory, which proves the necessity of new rules of reasoning each time theories develop, and, on the other hand, with the conception of Mathematics as it is found in intuitionism. And Heiting, for example, recently wrote that Mathematics constitutes an organic system in full development, to which it is inadmissible to assign limits.

Mathematics is a becoming. All we can do is try to understand its history, that is, to situate Mathematics among other intellectual activities, to find certain characteristics of this becoming. I would like to mention two of them:

1° This becoming is autonomous, i.e., if it is impossible to place oneself outside it, we can, by studying the historical, contingent development of Mathematics as it presents itself to us, glimpse necessities beneath the sequence of notions and procedures. Here, of course, the word "necessity" cannot be defined in any other way. We note problems, and realize that these problems demanded the appearance of a new notion. That's all we can do, and it's certain that this use of the word "demand" comes too easily to us, since we're on the other side, seeing the successes. We can say, however, that the notions that have appeared have really provided a solution to problems that actually arose.

I believe that it is possible, under the vivid contingency of the sequence of theories, to engage in this work. I personally tried to do it for Set Theory. I don't claim to have succeeded, but precisely in the development of this theory, which would seem to be the epitome of a brilliant theory made up of radically unforeseeable inventions, I seem to have perceived an internal necessity: it was certain problems in Analysis that gave rise to the essential notions, and engendered certain procedures already guessed at by Bolzano or Lejeune-Dirichlet, and which became the fundamental procedures perfected by Cantor. So, autonomy and therefore necessity.

2° This becoming develops as a true becoming, i.e., it is unpredictable. It may not be unpredictable for the intuitions of a mathematician in full activity, who guesses which way to look, but it is originally unpredictable, in an authentic way. This is what we might call the fundamental Dialectics of Mathematics. If new notions appear to be necessitated by the problems posed, this very novelty is truly a complete novelty. In other words, we can't simply analyze the notions we've already used to find the new notions within them: for example, the generalizations, that have given rise to new procedures.

I'll characterize this novelty by the second point of my conclusion: that the activity of mathematicians is an experimental one.

By experience, I mean a system of gestures, governed by a rule and subject to conditions independent of these gestures. I recognize the vagueness of such a definition, but I don't think it's possible to completely overcome it without taking actual examples. By this, I mean that each mathematical procedure is defined in relation to a previous mathematical situation, on which it partly depends, and in relation to which it also maintains such independence that the result of such a gesture must be observed in its accomplishment. This, I believe, is how one can define mathematical experience.

Does this experience have anything to do with what we usually call it? I believe it is preferable to reserve the very word "experience" for it. In particular, physical experience seems to me to be a complex of many heterogeneous elements, which I don't want to insist on today, since it would take us too far. But, in physical experience gestures are not performed in accordance with a rule, nor are their results meaningful in the system itself. On the contrary, this is the case with mathematical experience. In other words, given a specific mathematical situation, the gesture performed gives us a result which, by the very fact that it appears, takes its place in a mathematical system extending the previous system (containing it as a particular case).

How can these experiences be carried out? In my book on the axiomatic method, I tried explain it in a very incomplete way, but one that I hope to clarify later. I have indicated some of the methods used by mathematicians. This is, of course, a crude description, because, at any given moment, there are certain procedures that are situated in a mathematical environment a state of Mathematics at a given moment that may not be transportable. However, I have indicated some of these processes, drawing on both Hilbert's and Dedekind's analyses, in Dedekind's 1857 speech to Gauss, which was approved by Gauss and recently published by Miss Nœther, in 1931.

I've called a first process thematization, i.e., the gestures made on a model or a field of individuals can, in turn, be considered as individuals on which the mathematician works, considering them as a new field. The topology of topological transformations is an example of thematisation, but many other examples could be found. This procedure enables mathematical reflections to be superimposed. It also has the advantage of showing us that the link between the mathematician's concrete activity from the very first moments of his development — putting two symmetrical objects next to each other, making them change places — and the most abstract operations never ceases. Each time such a link is found in the fact that the system of objects considered is a system of operations which, themselves, are operations on other operations which, in the end, are operations on concrete objects.

The second procedure is named by Hilbert idealization or addition of ideal elements. It consists simply in requiring that an operation, which was accidentally limited to certain circumstances extrinsic to its accomplishment, be freed from this extrinsic limitation, and this by setting up a system of objects that no longer coincides with the objects of intuition. This is how, for example, the various generalizations of the notion of number came about.

What does this mean for the very notion of the mathematical object? I've tried to indicate this, in a way that may not be satisfactory — I admit, it doesn't completely satisfy me — but it's an approximation.

The mathematical object is thus, in my view, always correlative to gestures actually performed by a mathematician in a given situation. Does this mean that mathematical objects have a particular mode of existence? Are there, for example, ideal objects existing in themselves? In the purely mathematical discussions that took place between the proponents of the Vienna School and the Hilbert School, the question was raised as to whether there was a region of ideal objects to which Mathematics could refer — they called it Platonism, I think the expression doesn't quite fit the thing, but never mind the word. This is what, in an article that appeared this summer, Gentzen calls mathematization in itself.

From this point of view, I think I can go further than Gentzen, who tries to reconcile Mathematics per se with the constructivistic demands of intuitionism. I believe that a conception of systems of mathematical objects existing per se is in no way necessary to guarantee mathematical reasoning. For example, when it comes to the continuous, this conception of mathematical objects must be rejected, for one simple reason: it is totally useless, both for the very development of Mathematics and for an understanding of this development.

Indeed, if this conception were to correspond to something precise, it would mean that, if these objects to which the mathematician refers cannot be grasped in any intuition, at least their properties, their simultaneous presence, are required at some point in the mathematician's reasoning. Not only does this not happen, but if we want to clarify what it means, we come up against difficulties that force us to reject this conception. I'm referring here to Skolem's paradox.

I don't want to elaborate on this paradox, especially as it would require a formalization to explain it precisely. Roughly speaking, it means this: if we have a model, which we assume satisfies a system of axioms, it is always possible to construct a countable model satisfying this same system of axioms. In particular, we can satisfy the axiom system of Set Theory with a countable model.

This paradox, on which Skolem himself, and many others (Gentzen, during this last summer) have reflected at length, boils down to this: an exhaustive characterization of a model satisfying a system of axioms turns out to be impossible. If we assume the axioms to have been laid down, i.e., the enumeration of the properties we need for the objects, we cannot demand that these axioms at the same time generate the objects; we are obliged to assume the existence of a field of objects, and then from the properties of these objects in this field we can deduce other properties. What we can't say is that our field of objects can be characterized in a uniform way, by our system of axioms.

This has the advantage not only of eliminating this, so to speak, idealistic conception of the existence of mathematical objects, but also of marking the intimate interconnexion between the moments of mathematical development.

There's no starting at zero. Historically, we can see Mathematics appearing in the group of displacements of Elementary Geometry. Nevertheless, if we want to specify what we mean by this — either through the activity of counting, which already involves what Poincaré called the intuition of pure number, or the beginning of Elementary Geometry — we are bound, in reality, to develop all of Mathematics. We can of course stop arbitrarily and say: "We're satisfied with this state of affairs". Nevertheless, if we are faithful to the very requirement that presided over the birth of these notions and their development, then we have to raise problems that arise, for example, from the refusal to stop in circumstances that are external to the problem posed. At that point, new notions will appear, and not only Mathematics, up to the present day, will be recovered, but also the requirements for future developments and the unsolved problems that give rise to its current transformations.

In conclusion, I would say that the very notion of the existence of mathematical objects interests us as philosophers, because it raises the problem of the very notion of the existence of objects of thought.

What does it mean for an object to exist? Here, we are confronted with the fact that the very type of certain, rigorous knowledge that is mathematical knowledge prevents us from positing objects as existing independently of the system performed on them, and even independently of a necessary sequence from the very beginning of human activity.

So, we can never posit them per se, nor say exactly: this is the world — a world we would describe. Each time, we are obliged to say: these are the correlates of an activity. All we can think of in them are the rules of mathematical reasoning that are demanded by the problems that arise. There's even an overflow, a demand for surpassing that lies in the unsolved problems, which compel us to pose other objects anew, or to transform the definition of the objects primitively posed.

These are the ideas I wanted to share with you. I don't hide their incomplete and insufficient character, obvious to me, but I believe that the current state of Mathematics requires at least the essential part of them.

COMMUNICATION OF MR. LAUTMAN

After listening to Mr. Cavaillès, I'm even more convinced that I don't agree with him, and I'm going to try, in the few moments I have to speak, to clarify the points on which our conceptions diverge. It seems to me that, in what he calls mathematical experience, Mr. Cavaillès attributes a considerable role to an activity of the mind, determining in time the object of its experience. According to him, therefore, there would be no general feature of mathematical reality. On the contrary, at every moment in the history of Mathematics, mathematical reality would assert itself as an event both necessary and singular. Hence Mr. Cavaillès's criticism of Platonism in Mathematics, in the sense that Platonism is identified, in his view, with a theory of the existence "in itself" of Mathematics.

I agree with Mr. Cavaillès that such a conception of an immutable Universe of ideal mathematical beings is impossible. It's an extremely seductive vision, but it's far too weak in its consistency. The properties of mathematical beings depend essentially on the axioms of the theory in which they appear, and this dependence robs them of the immutability that should characterize an intelligible Universe. Nevertheless, I regard numbers and figures as possessing an objectivity as certain as that which the mind encounters in the observation of physical nature. Nevertheless, this objectivity of mathematical beings, which manifests itself noticeably in the complexity of their nature, reveals its true meaning only in a theory of the partaking of

Mathematics of a higher and more hidden reality, which constitutes, in my opinion, a true world of Ideas.

To make it clear how the study of the recent development of Mathematics can justify the Platonic interpretation I have proposed, I must first insist on what has been called the structural aspect of contemporary Mathematics. It refers to mathematical structures, but we shall then see how easy it is to trace back from these mathematical structures to the consideration of dialectical structures embodied in actual mathematical theories.

The structural aspect of contemporary Mathematics can be seen in the important role played by Cantor's Set Theory, Galois's Group Theory and Dedekind's Theory of Algebraic Number Fields in all part of Mathematics. What characterizes these different theories is that they are *abstract* theories. They study possible ways of organizing elements whose nature is indifferent. In this way, for example, it is possible to define global properties of ordering, completion, division into classes, irreducibility, dimension, closure, etc., which qualitatively characterize the collections to which they apply. A new spirit animated Mathematics: long calculations gave way to the more intuitive reasoning of Topology and Algebra. Consider, for example, what mathematicians call existence theorems, i.e., theorems that establish, without constructing, the existence of certain functions or solutions. In a very large number of cases, the existence of the function sought can be deduced from the global topological properties of a suitably defined surface. In particular, since Riemann, a whole geometric theory of analytic functions has been developed, enabling us to deduce the existence of new transcendental beings from the almost intuitive consideration of the topological structure of certain *Riemann surfaces*. In this case, knowledge of the mathematical structure of the surface is extended to an assertion of existence relative to the function sought.

If we reflect on the internal mechanism of the theory we've just alluded to, we realize that it establishes a link between the degree of completion of the internal structure of a certain mathematical being (a surface) and the existence of another mathematical being (a function), i.e., in short, between the *essence* of one being and the *existence* of another being. These notions of *essence* and *existence*, like those of *form* and *matter*, *whole* and *part*, *container* and *contents*, etc., are not mathematical notions, yet it is to them that the consideration of actual mathematical theories leads. I call them dialectical *Notions*, and propose to call the problem of the possible connection between dialectical notions thus defined dialectical *Ideas*. The reason for the relationship between Dialectics and Mathematics lies in the fact that problems of Dialectics can be conceived and formulated independently of Mathematics, but that any tentative solution to these problems must necessarily be based on some mathematical example that concretely supports the dialectical connection under study.

Consider, for example, the problem of the relationship between form and matter. It is possible to ask to what extent a form determines the existence and properties of the matter to which it can be applied. This is a key philosophical problem for any theory of Ideas, since it is not enough to posit the duality of the sensible and the intelligible. We must also explain partaking, i.e., by whatever name we call it, the deduction, composition or genesis of the sensible from the intelligible. In certain cases, Mathematics provides remarkable examples of the determination of matter from form: the whole theory of the representation of abstract groups aims to determine *a priori* the number of different concrete transformations capable of effectively realizing an abstract group of a given structure. Similarly, contemporary Mathematical Logic shows the close connection between the intrinsic properties of a formal axiomatic and the extension of the fields of individuals in which this axiomatic is realized. Here, then, we have the spectacle of two theories that are as distinct as possible from one another — Group Representation Theory and Mathematical Logic — but which nonetheless present close analogies of dialectical structure: analogies that come from their both being particular solutions to the same dialectical problem, that of determining matter from form.

I mentioned earlier that the distinction between an ideal Dialectics and an effective Mathematics must be interpreted above all from the point of view of the genesis of Mathematics from Dialectics. Here's what I mean by this: Dialectics, in itself, is pure problematization [*problématique pure*], a fundamental opposition [*antithétique fondamentale*] concerning pairs of notions that appear, at first glance, to be opposed, and in relation to which the problem of a possible synthesis or conciliation nevertheless arises. In my thesis, for example, I considered the problem of the relationship between the local and the global, the extrinsic and the intrinsic, the continuous and the discontinuous, and so on. As in Plato's *Sophist*, it turns out that opposites are not opposites, but that they can be combined to form the mixtures [*mixtes*] that are Mathematics. Hence the need for these intricate subtleties, this unpredictable peculiarity [*ce pittoresque imprévisible*], these obstacles that are sometimes overcome and sometimes circumvented, this whole historical and contingent becoming that constitutes the life of Mathematics, and which nevertheless presents itself to the metaphysician as the necessary extension of an initial Dialectics. We pass imperceptibly from the understanding of a dialectical problem to the genesis of a universe of mathematical notions. It is to the recognition of this moment, when the Idea gives birth to the real, that Mathematical Philosophy must, in my opinion, aim. In a booklet published by *Librairie Hermann* since my thesis, I have tried to show the analogy between these conceptions and those of Heidegger. The extension of Dialectics into Mathematics corresponds, it seems to me, to what Heidegger calls the genesis of ontic reality from the ontological analysis of the idea. This introduces, at the level of the ideas, an order of before and after that is not time, but rather an eternal model of time, the schema of a genesis constantly in the making, the necessary order of creation.

It seems to me that the problem of the relationship between the theory of Ideas and Physics could be studied in the same way. Consider, for example, the problem of the *coexistence* of two or more bodies. This is a purely philosophical problem, which we'd say Kant posed rather than solved in the third category of relation. Nevertheless, as soon as the mind tries to think what the coexistence of several bodies in space might be, it necessarily gets involved in the as yet unsolved difficulties of the n-body problem. Consider again the problem of the relationship between *motion* and *rest*. We can abstractly pose the problem of whether the notion of motion only makes sense in relation to absolute rest, or whether, on the contrary, there is rest only in relation to certain changes; but any effort to resolve such difficulties gives rise to the subtleties of the Theory of Special Relativity. The question also arises as to which of the two notions of motion and rest should be given a physical meaning, and this is a point on which classical Mechanics and wave Mechanics clash. The former considers the wave as a physical movement. For the latter, on the contrary, the wave equation appears to be no more than an artifice designed to highlight the physical invariance of certain expressions with respect to certain transformations. It thus appears that the theories of Hamilton, Einstein and Louis de Broglie take on their full meaning with reference to the notions of motion and rest, of which they seem to represent the true dialectics[11]. It may even be that what physicists call a crisis in contemporary Physics, grappling with the difficulties of the relationship between the continuous and the discontinuous, is a crisis only in relation to a certain rather sterile conception of the life of the mind, where the rational is identified with unity. On the contrary, it seems more fruitful to ask whether the purpose of reason in the sciences is not rather to see in the complexity of the real, in Mathematics as in Physics, a mixture whose nature can only be explained by going back to the Ideas of which this real partakes.

This shows what the task of Mathematical Philosophy, and indeed of the Philosophy of science in general, must be. The Theory of Ideas needs to be built up, and this requires three kinds of research. First, the research of what Husserl calls descriptive eidetics, i.e., the description of those ideal structures embodied in Mathematics, whose richness is inexhaustible. The spectacle of each of these structures is each time more than a new example in support of the same thesis, for it is not excluded that it is possible — and this is the second of the tasks assignable to Mathematical Philosophy — to establish a hierarchy of Ideas and a theory of the genesis of ideas from one another, as Plato had envisaged. Lastly, and this is the third of the announced tasks, one has to remake the *Timaeus*, i.e., to show, within the Ideas themselves, the reasons for their application to the sensible Universe.

These seem to me to be the main aims of Mathematical Philosophy.

DISCUSSION

Mr. Cartan. — I'm rather embarrassed, because I'm a bit in the position of Mr. Jourdain, who used to speak in prose without realizing it. Mathematicians — at least a certain number of them, including myself — are not in the habit of reflecting on the philosophical principles of their science. When they hear a philosopher talk about them, they are interested, but they don't really know how to respond to the considerations he develops.

Obviously, I'm familiar with both Mr. Cavaillès's and Mr. Lautman's theses, since I was on the jury for both, but my situation

[11] We have retained the lowercase here, as in the original text, to distinguish Dialectics as a formal field of thought or method from dialectics in a more flexible sense, referring to any process of interaction between opposing forces or ideas without necessarily invoking the formal philosophical notion.

is different: I used to be on the right side of the barricade, whereas today I'm on the other side...

I didn't quite understand the opposition between the two points of view of Mr. Cavaillès and Mr. Lautman, which seem to me to be different rather than opposed. I have the impression that Mr. Cavaillès's considerations concern the very basis of mathematical thought, whereas Mr. Lautman's are more concerned with the current state, not of Mathematics as a whole, but of a certain number of mathematical theories and, in this respect, there are obviously a number of statements by Mr. Lautman that particularly interest me: those concerning the relationship between the local and the global, for example. Certainly, these relationships arise in an important part of Mathematics. The theory of functions, in particular of functions of real variables, as it has been conceived for the last fifty years, cannot address the problem of the relationship between the local and the global. The functions considered are too general to be able to deduce their global properties from their local properties. But there is a class of functions for which the relationship between the local and the global is basically the essential part of the problem: these are analytic functions of complex variables whose global properties are determined by their local properties. For quasi-analytic functions, which have been introduced recently, something analogous happens: when the values of the function and those of its successive derivatives are known at a point, it is completely determined in its entire field of existence.

In Geometry — and it's especially Geometry that Mr. Lautman was thinking of — there are also extremely important problems in which the relationship between the local and the global arises: if we take, for example, a small piece of a space, is it possible, through knowledge of this small piece, to deduce knowledge of the whole space? Of course, we have to assume that this space has fairly simple global properties, without which this problem wouldn't make sense. On the face of it, these are problems of pure Geometry, but in reality they are also problems of Analysis. Let's take, for example, a portion of Riemannian space. If you assume that the functions used to define this space are analytic, you'll have an extremely interesting problem: given a small piece of Riemannian space defined analytically by its differential form, to what extent can we deduce the global properties of this space? It may happen that this small piece cannot be extended to form a complete space. In general, this is what happens. If it can be extended to form a complete space, it can be extended in only one way, with certain restrictions.

Here, then, is a problem of the relationship between the global and the local that is not defined simply by its geometric statement, but is linked to the existence of purely analytic properties in the definition of the piece of space.

Similar considerations could be developed for the relationship between the intrinsic and the extrinsic. Given a surface immersed in a certain space, do the supposedly known intrinsic properties of the surface entail limitations on the properties of the space containing it? These are extremely interesting problems, but it should be noted that they depend not only on the geometric definition of the problem, but also on its analytical definition.

Mr. Lautman gave a number of other examples of such problems: Form and Matter and Group Theory. It's all very interesting, but I don't know to what extent it justifies Mr. Lautman's general thesis, because I don't quite understand what Dialectics is, and I have to stay on purely technical ground.

I don't have the impression that Mr. Lautman's considerations contradict those of Mr. Cavaillès. I have the impression that Mr. Lautman is considering certain particular problems of current Mathematics, and a certain number of philosophical problems. On the whole, I think I agree with him, but, unfortunately, I'm unable to argue with him on this ground.

In any case, I don't think there can be any objection to the character of Mathematics as an autonomous and unpredictable development. Nevertheless, history teaches us that in the history of Mathematics — which I know and have experienced — there have been certain forecasts of the future. In 1900, Hilbert gave a lecture on the future problems of Mathematics, an extraordinarily remarkable lecture, precisely because he put his finger on the problems that were to arise in the development of Mathematics over the next fifty years at least, and he foresaw precisely the most important problems that actually arose.

On the other hand, we could find lectures by eminent scientists on the future of this or that branch of Mathematics, in which these scientists did not foresee at all what was going to happen.

Certainly, the development of Mathematics is in itself somewhat unpredictable, and when you reach a certain age, you realize that certain theories, after twenty, thirty or forty years, take on a completely unexpected development, and that the point of view from which you come to consider them is completely different from the initial point of view. However, we are obliged to admit that it is certain internal necessities that have emerged as these theories continue to evolve. I'm thinking, for example, of Topology, a science which barely existed half a century ago, and which is taking on a new aspect and a completely unexpected development every day, penetrating ever more deeply into all branches of Mathematics.

Mr. Paul Lévy. — First of all, I could repeat what Mr. Cartan said earlier: I'm a bit disconcerted when I hear philosophers talking about the science I'm studying in a language I'm not used to. I follow them with a bit of effort and I'm not sure I understand everything they say. I think I'm pretty sure I've understood some of it, but I'm also sure I haven't quite understood some of it.

So, I can't give an opinion on all the issues that have been raised. I can only offer a few thoughts that were suggested to me by Mr. Cavaillès's lecture, and I believe they are not out of line with the question; if I'm wrong, you'll have to excuse me.

I think I'm a little at odds with Mr. Cavaillès, but his conclusion reassured me, when he said that there were some inner necessities revealed in the becoming of Mathematics.

I believe that the development of Mathematics — while having a great deal of contingency, it goes without saying — presupposes much deeper inner necessities. Naturally, it was impossible to foresee that such and such a theorem would appear at such and such a date in history, but internal necessities play a very large role, and there are theorems of which I can tell you: if such and such a scientist had not found such and such a theory at such and such a time, and if such and such a theorem had not been proved in such and such a year, it would have been discovered in the following five or ten years. As proof, I give that a very large number of theorems were, at very short intervals, discovered separately by different scientists, because they responded to a necessity in the development of mathematical thinking at the time.

This leads me to believe that, once a certain mathematical theory has begun, a superior mind can foresee in what direction it will develop. Let me take, as a concrete example, one of the mathematical theories whose philosophical aspect has attracted the most attention: the Theory of the Integral, as constructed by modern Set Theory. It was Mr. Lebesgue who gave the notion of the integral its definitive form, and today you all know that the integral is an essential tool in Mathematics. It is so essential that, without a doubt, if Mr. Lebesgue hadn't existed, his integral would still have been discovered today, long ago. I don't think I'm diminishing Mr. Lebesgue's merit; on the contrary, I think I'm only enhancing it, by saying that he brought to light a notion that was necessary for the further progress of science. Would Émile Borel, who was already working along these lines, have developed this theory? Would another of his students have been given the chance? I don't know. But, after the work of Jordan and Mr. Borel, given the current level reached by humanity as a whole and the number of researchers specializing in the field of Mathematics, I believe it was necessary and inevitable that, within ten or fifteen years, the theory of the Lebesgue Integral should be put on the bridge. And, in this vein, I believe, to a certain extent, that the development of Mathematics is predictable.

Of course, it must not be denied that, on the other hand, certain discoveries constitute an unforeseeable leap in the development of science: coming before their time, their importance is sometimes only recognized after a more or less long time. On the other hand, it is certain that among mathematicians, there are geometers and algebraists. The former evolve in one branch of Mathematics, the latter in another. It would have been conceivable for the human species to contain only geometers, and not algebraists, or vice versa. Likewise, it is possible that a later development of humanity will allow certain brains to devote themselves to certain branches of Mathematics that we cannot conceive of at present.

On the other hand, there was one point on which both speakers agreed, and to the extent that I understood them, I'm a little surprised. For me, Mathematics would have no raison d'être if its object were considered non-existent. When I say that the product of two numbers is independent of their order, it's something that's true regardless of the fact that I say it: it's not true only in my mind.

Let's take a simple example that can be verified objectively: I have rectangular squares with a certain number of rows and columns; I have a certain number of marbles, and I want to put one in each square. Well, the same number of marbles will suffice, depending on whether I fill the squares by rows or by columns. I'm using this very simple example, because in other cases it would be difficult to find a material interpretation to verify the accuracy of a theorem.

For me, the theorem pre-exists: when I try to demonstrate whether a statement is true or false, I'm convinced that it's true or false in advance, regardless of the chances I have of discovering it.

Let's take another problem: is Riemann's hypothesis about his ζ function right or wrong? I think most mathematicians are convinced that it is correct, although no one has proved it, and I think all the mathematicians in this room will agree that we may never get there, but that this hypothesis is in itself true or false, even if we can't work out whether it is true or false.

If I understand your language correctly, you'll express my position by saying that I'm a Platonist, but I can't imagine what could make me abandon this point of view.

Mr. Fréchet. — I'd like to begin by associating myself with an observation that has just been made before me by Mr. Cartan and Mr. Lévy: for a mathematician who devotes most of his time to Mathematics, it is extremely difficult to follow in all their nuances the presentations, however instructive, of Mr. Lautman and Mr. Cavaillès. Perhaps the difficulty in discussing them lies not so much in what they said as in the need to understand exactly what they meant.

Before going into detail, however, I'd like to say that, in any case, I admire the virtuosity with which they handle not only philosophical language, but also mathematical language. We are immersed in Mathematics, and — at least as far as I'm concerned — totally ignorant of the subtleties of philosophical language and the nuances that differentiate certain philosophical theories: whereas our distinguished colleagues seem, on the contrary, to move with ease, not only in Philosophy, but also in Mathematics. Last but not least, they know a great deal about technique and the results of certain parts of Mathematics that I personally know nothing about.

Precisely for the reasons I've given above, I don't want to go into the various subjects they dealt with one by one. But there are two or three points on which I may have understood their thesis, and on which I'd like to say a word.

First of all, there are two related questions, at least in my mind, to which I might be able to provide an answer: Mr. Cavaillès indicated that, in his opinion, Mathematics is an autonomous science. Personally, I don't think so. It all depends, of course, on what we call "Mathematics". Many people call "Mathematics" the set of deductive theories that enable us to go from a set of properties and axioms to certain theorems. This is undoubtedly the most specific part of Mathematics, but it seems that, if we were to stop there, not only would Mathematics be reduced to a machine for transformations, and in that case its role would still be very useful, but it would be limited to transforming, so to speak, emptiness into emptiness. I believe that, in order to justify the existence of Mathematics, it is essential to show that it is an instrument invented to help mankind understand nature and predict the course of phenomena. The notions that seem to me to be the most fundamental in Mathematics are all notions that do not, in my opinion, originate from our intelligence, from our mind, but are imposed on us by the external world.

I'll mention, for example, the whole number, the straight line, the plane, the ideas of velocity and force, and certain transformations such as symmetry and similarity. These are notions that were not present in our minds, but were imposed on us by consideration of the world around us. We translated these external realities into words, axioms and definitions — which only represented them approximately, of course, and were simpler, to be more manageable — but which nonetheless had their source in the external world.

In addition to these fundamental notions, which are at the origin of Mathematics, others are constantly being added, introduced by the development of the physical sciences. The notions of work, moment of force, for example, have only been defined, to my knowledge, in the last two or three centuries. Many other notions I could mention, such as differential equations, were only introduced in modern times, as a result of the development of physics, mechanics, astronomy and so on.

Alongside these notions, the study of which is, so to speak, imposed upon us, other notions of a different nature have been introduced into Mathematics. These are those that are due to the "internal activity" of this science. They seem to me to be much less fundamental than the others, having been devised to facilitate the mathematician's task, with a view to solving problems posed from the outside.

Elementary examples include inversion transformation and reciprocal polar transformation. As far as I know, these two transformations have not been imposed by examples taken from nature. They are mathematicians' devices that provide a means of investigation.

Similarly, I believe that the introduction of complex numbers has provided an extremely powerful tool for obtaining certain propositions about real numbers much more quickly.

We could cite many other examples: in Elementary Geometry, we introduce the consideration of supplementary trihedra. Here again, I don't think there's any real phenomenon that requires us to consider these supplementary trihedra, but it does provide a convenient way of transforming one proposition into another in Elementary Geometry.

In the examples I've just cited, I see two categories of notions: some that fit well within the framework of an autonomous Mathematics, and others, on the contrary, that don't seem to me to be reconcilable with the idea of an autonomy of Mathematics.

And this leads me, on the contrary, to agree with Mr. Cavaillès — for reasons different from his own, it's true — on the unpredictable nature of Mathematics, from a point of view which, incidentally, is entirely reconcilable with that presented by Mr. Paul Lévy and which would seem to lead to the opposite conclusion.

Mr. Lévy pointed to numerous examples where problems could not fail to be solved by Mathematics. In this sense, Mathematics was predictable, because these were problems that mathematicians had set themselves *for the internal development of* Mathematics.

But in the development of non-mathematical sciences, there are always problems that arise and impose themselves on mathematicians, that mathematicians are asked to solve and that give them new ideas, forcing them to introduce new notions. And these are unpredictable. We don't know, we can't even imagine what kind of problems technology or physics will pose for mathematicians in fifty years' time. Perhaps we'll have the means to solve these problems by drawing on the existing arsenal of mathematical theories, perhaps we'll need to create new mathematical tools. There's an impulse coming from outside, and its interventions are of an unpredictable nature.

This is what I wanted to say about the autonomy and unpredictability of Mathematics[(12)].

As for Mr. Lautman's thesis, I'm a little reluctant to comment on most of it, as I find different interpretations possible: some seem

[(12)] I developed these two points, among others, in a report presented in Zurich in December 1938 on "The Question of *the Foundations of Mathematics* and General Analysis" at a symposium organized by the *International Institute for Intellectual Cooperation*, the proceedings of which were published by the Institute.

quite immediate and acceptable to me, but don't seem reconcilable with the conclusion. This is probably because I haven't quite understood.

I see, at the beginning, sentences like this: "The establishment of effective mathematical relations appears to me, in fact, as rationally posterior to the problem of the possibility of such connections in general."

Mr. Lautman is careful to point out that, for him, this is not a historical point of view. And indeed, from a historical point of view, the answer is not in doubt: on the contrary, the establishment of effective mathematical relationships certainly *predates* the problem of the possibility of such connections.

So, what exactly does "Rationally posterior" mean? I ask the same question of the sentence: "We can see in what sense we can speak of the partaking of distinct mathematical theories in a common dialectics that dominates them."

Considering these two sentences and the surrounding text, it seems to me that there is an answer to which one would naturally arrive: it is that the various mathematical theories (especially the proofs contained in these theories) consist of reasonings applied to certain particular circumstances, but that they all come under the same general theory, which Mr. Lautman designates, I believe, under the name of the Theory of Ideas, and which mathematicians would probably call Logic.

If it were so, I think everyone would agree, but it would be so obvious that I don't think Mr. Lautman meant precisely that. In any case, it would be irreconcilable with the end of his presentation: "Mathematical thinking thus has the eminent role of offering philosophers the constantly renewed spectacle of the genesis of the Real from the Idea."

I don't know exactly what this means, but it seems to me, from the reflections I made earlier, that it was reality that generated the idea, at least as far as Mathematics is concerned. It is the requirements of reality that have led to mathematical problems, that led mathematicians to use logic and formulate certain definitions, certain axioms.

I can see, therefore, the genesis of the Idea from the real, but I confess I don't understand the opposite position. Perhaps further discussion will clarify this point[13] ?

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(13) As I correct the shorthand of my speech, I realize that the main difficulty for me was to understand Mr. Lautman's language precisely and accurately. As he

Mr. Ehresmann. — I've taken note of a few thoughts relating to Mr. Lautman's thesis. I find it extremely interesting to see in it general problems that can be found in many mathematical theories. But I quote one of the most characteristic sentences: "One of the essential theses of this work affirms the necessity of separating the supra-mathematical conception of the problem of the connections that certain notions support between them, and the mathematical discovery of these effective connections within a theory."

If I have understood correctly, it would not be possible, in this field of supra-mathematical dialectics, to specify and study the nature of these relationships between general ideas. The philosopher could only highlight the urgency of the problem.

It seems to me that, if we're concerned with talking about these general ideas, we're already vaguely conceiving the existence of certain general relationships between these ideas. Therefore, we can't stop halfway, we have to tackle the truly mathematical problem of explicitly formulating these general relationships between the ideas under consideration.

I believe we can give a satisfactory solution to this problem with regard to the relations between the whole and its parts, the global and the local, the intrinsic and the extrinsic, etc.... Thus, the relations between a fundamental set and its parts form precisely the subject of a chapter in the abstract Set Theory. Between the subsets of a set, we have the following relationships: *inclusion* of one subset in another, *intersection* of two subsets, *union* of two subsets, *complementary* set of a subset. In the set of subsets of a fundamental set, these relations give rise to a whole calculus, namely Boolean Algebra. These are a number of general relationships that can be found in any mathematical theory.

Given a fundamental set with a *particular* mathematical structure, such as a group structure or a topological space structure, the relationship between this fundamental Set and one of its subsets is expressed by the *mathematical* notion of structure induced on the subset. I can't go into more detail here, because we'd first have to

pointed out in his reply, what he meant by the real in no way corresponded to the concrete, the sensible, with which I had identified the real.

In the absence of such an identification, my objection falls by the wayside; but it has not been useless in providing yet another clear example of the importance of an unambiguous interpretation of the language used. I've been told by philosophers that this difficulty — which appears more clearly in debates between philosophers and mathematicians — is not absent from discussions where only professional philosophers meet.

define the general notion of mathematical structure. The problem of relations between intrinsic and extrinsic properties, and the problem of the situational properties of a subset in a fundamental set, is nothing other than the problem of relations between the structure of the fundamental set and the structures induced on a subset and on the complementary subset.

As regards the notions of *local* and *global*, it seems to me that the notion of local only makes sense for a topological space structure: as we then have the notion of a point's neighborhood, the notion of a local property at a point can be deduced from the notion of a structure induced on *any* neighborhood of the point. Once again, we arrive at a purely mathematical notion.

The examples could be multiplied. I believe that the general problems raised by Mr. Lautman can be stated in mathematical terms. And this ties in with the thought expressed in the summary of Mr. Cavaillès's thesis: "To speak of Mathematics can only be to remake it."

Mr. Hyppolite. — First of all, I must confess that while I understood Mr. Cavaillès' thesis perfectly, I understood Mr. Lautman's much less well.

What struck me in Mr. Lautman's talk was the ambiguity of the word "dialectics", and the different meanings in which it has been used. It seems to me that — applied to Mathematics — the word "dialectics" has been used in three different senses, or at least I thought I discerned three rather different meanings of the term.

In the first sense of the term, Mr. Lautman would agree with Cavaillès's thesis — their two conceptions, on this point, would be similar: dialectics would be the very experience of the life of Mathematics, reconciling, as it were, the need for development we've already mentioned, and the apparent contingency of this development.

In another sense, Mr. Lautman's dialectics is a kind of art of problematization, in the modern sense of the word, something quite different. I think it's mainly in this sense, in fact, that he uses the word; such a dialectics is a problematization, a kind of opening onto theoretical problems that the mathematician would come to embody in his research.

And in a third sense — and this is where the ambiguity seems to me to be strongest — Mr. Lautman uses the word "dialectics" in the sense in which philosophers have most often taken it. It's a dialectics of "form and matter, local and global", and so on. It seems to me, however, that if we were to use the word "dialectics" at all costs in the philosophy of Mathematics, we would have to use it only in the first sense, i.e. in the sense of a life of mathematical experience in the course of its history.

Let me give you an example that really struck me: the development of the theory of Equations, from Viète to Galois. I think that, if there is a necessity — as Mr. Cartan said — in the development of Mathematics, this necessity appears very clearly in the development of this theory from Viète to Descartes, but it no longer appears at all when it comes to Galois's discoveries. It seems that there is something completely new in mathematical theory, something unexpected that has been introduced and that cannot be foreseen exactly by the earlier development of Mathematics. This is something that struck me a lot, when studying the decomposition of a group into invariant subgroups in Galois, and the application of this problem to the algebraic resolution of Equations after studying the problem of the theory of algebraic equations in Descartes. It seems to me that in this case we can see both a necessary development, and then the appearance of a completely new method in the problem, an unforeseeable creation, if not in retrospect.

There's another point I'd like to make about the evolution of Equation theory from Viète to Galois: you could put it crudely and say that we don't know how to undo what we know how to do, or that intellectual activity exceeds itself in what it generates. In a way, the given equations appear to be enigmatic mathematical beings. We know how to construct them, by the products of binomials, as Harriot did. We can thus manage to construct equations of any degree, but we are then incapable — the problem of division after that of multiplication — of undoing any given equation.

To attempt this analysis in general, it was necessary to introduce new notions which, moreover, can be understood in a certain way, such as the imaginary ones foreseen by Descartes. In 1637, Descartes explicitly stated that there were n positive, negative or imaginary roots of the n^e degree equation, a prediction of what was to come much later.

To sum up, I think I'd tend to agree with Mr. Cavaillès, who sees in Mathematics an essential autonomous life. One might also think that the need for mathematical development and historical contingency must be reconciled in this "life of Mathematics".

As for Mr. Lautman's thesis, we might fear that, if we adopted it, mathematical notions would somehow evaporate into purely theoretical problems that go beyond them: such as form and matter, the local and the global. The very originality of "Mathematics" would be in danger of disappearing.

I didn't quite understand in Mr. Lautman's thesis whether the mathematician would end up finding these problems, or whether it was on the contrary an ideal requirement of these problems — and this would be the problematic-, which would first be given shape and then embodied in Mathematics.

There's an ambiguity here, but perhaps I've misunderstood Mr. Lautman's thesis.

Mr. Schrecker. — After so many mathematical considerations, a philosopher may perhaps be allowed to present a few thoughts that do not absolutely respect the autonomy in which mathematicians necessarily confine themselves. They concern the impossibility of defining Mathematics, as asserted by Mr. Cavaillès. According to him, any definition of Mathematics would be absurd, because it would be impossible to define it by something it is not. But it seems to me that this same difficulty can be found in all the sciences: no science is capable of definition by its own means and methods, and it is always necessary to place oneself outside a science in order to arrive at a definition of its domain.

But that doesn't mean we would necessarily define Mathematics by something it is not. Mathematics is a science: that's the first element of a definition, and it's certainly not heteronomous. Mathematics is a hypothetico-deductive science: that's the second element. But it's true that you can't define Mathematics while remaining within mathematical formalism and, in the definition, respecting the autonomy of the mathematical domain. Formalism and autonomy apply to all mathematical problems; however, the definition of Mathematics is not itself a mathematical problem; it's a problem for the theory of science, which is in no way obliged to fit into the coherence of mathematical formalism itself.

So the refutation of the hypothetico-deductive nature of Mathematics seems to me to turn in a circle, because this refutation itself makes use of the hypothetico-deductive method. This refutation is given by means of reasoning which, being deductive, is necessarily also hypothetical, because it assumes the effectiveness of the formalism by which it operates. By denying the hypothetico-deductive character of Mathematics, we turn in a closed circle or in a closed system that has neither entry nor exit...

Mr. Cavaillès. — I never denied this character, I only said that it could only be defined that way, because we have to use mathematical theories.

Mr. Schrecker. — But it's obvious that if we try to define Mathematics by using mathematical theories, we'll never succeed. If, on the other hand, we decide to define Mathematics by other means, emancipating ourselves from formalism and employing historical or philosophical methods, it seems possible to succeed. And all the more so since, without a doubt, we know how to distinguish Mathematics from the other sciences, when we undertake its history or when we consider it as an object of Philosophy.

Some great mathematicians have proposed a definition which, while not absolutely satisfactory, seems to me to be on the right track. Bolzano defined Mathematics as the science of the general laws that all possible things necessarily follow. And H. Weyl has proposed a definition that does not essentially differ from this. It would seem, then, that the philosopher is not obliged, when faced with the problem of defining Mathematics, to the resignation that Mr. Cavaillès demands of him.

Mr. Chabauty returns to Mr. Cartan's remark that the dialectical themes envisaged by Mr. Lautman can only be found in certain parts of modern Mathematics. Few examples can be found in the work of set theorists [*des ensemblistes*]. When we have indeed recognized one of these themes in certain approaches to Mathematics, it might be interesting to see what initial conditions, what axioms imposed on the sets under consideration, have enabled this common character of the theories under consideration.

Mr. Dubreil. — I was particularly interested in what Mr. Cavaillès said about the effort mathematicians made to reflect on their own science, and about one of the difficulties they then encountered: to study the non-contradiction of a system of axioms, you have to involve mathematical theories that are of a higher level. For example, to establish the non-contradiction of arithmetic, transfinite induction is used.

I wonder whether this difficulty is not more apparent than real, and whether the power of the means required to establish the non-contradiction of a system of axioms does not rather highlight the profound nature and true scope of these axioms. Let's take the example of integers again: perhaps it's not too much to say that, if we want to exhaust the *mathematical* content of this notion, we're led to link it to that of a well-ordered set.

Let's focus our attention not on the individual natural numbers, but on the *set of* all these numbers. This set is ordered, even well-ordered; moreover, each element has an antecedent. As the notions of set, order, well order and antecedent are logically independent of the notion of natural integer, let's consider *a priori* well-ordered sets where each element admits an antecedent: two possibilities present themselves, depending on whether or not the set admits a last element; we'll call it finite in the first case, countable in the second. From these definitions, we can easily see that any two countable sets have the same power, and that any finite set has the same power as a certain segment of a countable set. The set of natural integers thus appears as a countable set, chosen once and for all, but arbitrary, to the segments of which we compare finite sets. Operations on the natural integers and their properties follow immediately from the notions of reunion and product of sets.

We can see that a small number of remarkable properties characterize finite sets and countable sets, in particular the set of natural integers, within the more general class of well-ordered sets. We've also highlighted a fact that, if you think about it, seems quite natural: like so many other sets considered in Algebra, the set of integers is really only defined by an isomorphism.

Mr. Cavaillès. — If you don't mind, I'll answer in reverse order.

My answer to Dubreil is quite simple: Dubreil is not the only one to say that what Gödel discovered was bound to be found. Yes, but when Gödel presented his *Mémoire*, nobody suspected that such a thing was possible. Hilbert and von Neumann, whom I've already mentioned, worked for years to demonstrate the non-contradiction of arithmetic with finite means, without resorting to transfinite induction. Von Neumann himself was very surprised by Gödel's result.

As for the priority between the notions of whole numbers and well-ordered or countable sets, that's a mathematician's question, and I won't take the liberty of resolving it myself; my humble opinion is that the notion of integer comes first, and this seems to me to be confirmed by the work of, for example, von Neumann on the axiomatization of set theory, where, prior to the notion of well-ordered set, we find what he calls the notion of numbering, i.e. an extension of the notion of integer, each time by mapping an object onto the system of objects already numbered; extending in this way, we arrive at the notion of transfinite numbering.

This has very little to do with Gödel's result. It was a question of demonstrating whether it was possible, using finite arithmetic, the axiom of ordinary complete induction (and not general complete induction) to make a certain property appear in symbols: arithmetic non-contradiction. Gödel succeeded in demonstrating that this was impossible. This is a considerable achievement. About a month ago, Gödel introduced a considerable new result: the possibility of demonstrating, using the axioms of set theory — without the axiom of choice — the non-contradiction with these axioms of the axiom of choice and even of the continuum hypothesis.

The reason I cite this new example is to show that by extending these meta-mathematical procedures, we can ensure — if we adopt radically new procedures — ever more extensive theories.

As for Mr. Schrecker, I don't know if he's satisfied with his definition of Mathematics, you'd have to ask mathematicians what they think. If someone has never done mathematics and is told it's a deductive science, I don't think they'll get the idea of Mathematics.

What I mean is this: what are we actually thinking when we talk about science, and deductive science at that? There's only one way to think about something deductively, and that's to do Mathematics. Here, I'm touching on the problem I wanted to avoid, and you're going to tell me that the definition of a deductive science is a logical question. I don't want to get into that debate, but if we want to know what a deduction is, there's only one way: to do mathematics; and the logical processes we call deductive are a very elementary mathematical combinatorics.

Let me add that this is very important; I can invoke the testimony of Carnap, who was in favor of reducing every mathematical notion to a logical one; yet he had to clarify, in his *Logische Syntax der Sprache*, that now he was saying: the meaning of a sign is its instructions for use. It's impossible to give a complete meaning to the notion of deduction independently of mathematical development. What's more, if you restrict deduction to the calculus of propositions or of predicates, you won't have the axiom of complete induction, and it will mean nothing to say: "Mathematics is a deductive science", since the axiom of complete induction, as Poincaré said, as Hilbert repeated, is the very essence of mathematical life.

I'm sorry to have to completely disagree with what Mr. Fréchet has told me.

I'm not trying to define Mathematics, but rather, by means of Mathematics, to find out what it means to know, to think; this is basically, very modestly, the problem posed by Kant. Mathematical knowledge is central for knowing what knowledge is.

Mr. Fréchet tells me: “there are notions that are taken from the real world and other notions that are added by the mathematician”. I reply that I don’t understand what he means, because I don’t know what it is to know the real world, other than to do Mathematics on real world.

What do you call the real world? I’m not an idealist, I believe in what’s lived experience [*vécu*]. To think a plan, do you live it? What do I mean when I say I think of this room? Either I’ll talk about lived impressions, rigorously untranslatable, rigorously unusable by means of a ruler, or I’ll do the geometry of this room and do Mathematics. What do you think of when you think of a plane? The geometric properties of this plane, the symmetry?

Our disagreement stems from the fact that I haven’t expressed my thoughts enough, and I feel I’m not good enough.

I spoke of an interdependence based on sensitive gestures. There isn’t, on the one hand, a sensible world that would be given, and, on the other, the mathematician’s world outside. The symmetry of the plane, for example, coincides with the character of permutation, which is one of the properties I experience in the sensible world.

Mr. Fréchet. — This character is revealed to me by the sensible world.

Mr. Cavaillès. — Hilbert said that there can never be mathematical thinking without the use of signs, without sensible work on signs. I’m sorry to say this, but I suppose mathematicians agree with me that they experiment with the signs they have: in a formula, there’s a kind of appeal. “Who could do without the circle with its center, the cross of coordinate axes? Arithmetic signs are written figures, geometric figures are drawn formulas, and it would be as impossible for a mathematician to do without them as it would be to ignore parentheses when writing.”

I quote from memory Hilbert’s very fine article on this subject — it predates the war, it’s early Hilbert. — This article studies unconscious experiments on possible relationships, the possible use of certain signs: I know the use I can make of them, there’s a possibility of experimentation; we can’t exhaustively define the mathematical object independently of the object’s implementation in the sensible world.

I believe that this point of departure is never left, in the sense that there is an internal interdependence, and that every time we substitute a less well-thought-out mathematical object with more well-thought-out objects, i.e. we separate what was united merely accidentally, by the process I've indicated, to that extent all the same, we don't leave the sensible world.

But there is autonomy. In fact: 1° The questions posed by direct practice in its unification (theoretical physics) only take on meaning and form by being transformed into mathematical questions, i.e., by being inserted into the becoming of pure Mathematics. 2° This insertion does not cause ruptures: physics only acts as an occasional revealer. In reality, the problem was latent — internal difficulties, the need to go beyond a system of over-simplified notions — in the fabric of mathematical substance. Here again, I can invoke history: a sufficiently detailed study would always show, for all the examples of services rendered by physics to mathematics, that there is an internal necessity, that physics is merely the occasion. I believe that it is essential, if we are to understand — and on this point it seems to me that the disagreement is complete, but this has at least one advantage, which is that we can decide: of course, we won't do so here — I believe that it is essential to see, in the notions used by the mathematician to solve problems, the result of a requirement that was already present in the previous system.

It's possible that the mathematician is lazy, or for extrinsic reasons, that he doesn't solve certain problems, that he cohabits with difficulties, but I don't think we can deny the role of internal necessity.

It seems to me that Mr. Paul Lévy made much the same objection.

Mr. Paul Lévy. — I wanted to express the idea that something exists *a priori*, regardless of how it's discovered.

Mr. Cavaillès. — Here again, I've expressed myself inadequately: I'm not at all saying that these notions are dependent on a historical order, I believe they are required by the problems.

When we've used integers, it's obvious that we'll be posing the product as commutative; there will be other cases where we'll be using non-commutative products.

As a result, when you say: "Given a problem, there is a solution" — "Seek and you shall find it", as Hilbert used to say — this is what I indicated as the projection of the system of mathematical gestures. The historical, contingent mathematician can stop, get tired, but the necessity of a problem imposes the gesture that will solve it.

This, if you like, is what I meant when I said that this is the reality of knowledge, which, from the very point of view of an anthropology or philosophy of the human constitution, is the extraordinary miracle of human destiny; independently of life in the lived world, problems arise that require solutions and lead outside of what is by a necessary chain.

Here, I wouldn't be too far from Lautman, except for the word "real", which bothers me; it's a question of distinguishing whether it's the sensible real, and here I disagree, or whether it's the actual real of beings, and here I agree with him, and perhaps also with Mr. Paul Lévy. In other words, this solution is obviously required by the problem at hand: you say it's somewhere, for me it's a matter of taste.

Mr. Paul Lévy. — The word "somewhere" indicates that it's not localized.

Mr. Fréchet. — Personally, I completely agree with Mr. Paul Lévy, I see this proposition as existing outside of us.

Mr. Cavaillès. — Lautman separates himself from me; what I find very interesting in what he does is precisely the connections he makes between certain theories. The future will show who's right: personally, I'm very reluctant to posit anything else that would dominate the mathematician's actual thinking; I see a requirement in the problems themselves. Perhaps this is what he calls the Dialectics that dominates; otherwise, I believe that this Dialectics will only lead us to very general relationships, or to relationships like those indicated by Mr. Cartan. There's undoubtedly a point in looking in this direction; but transforming this into a philosophical position doesn't seem possible to me.

Mr. Lautman. — First of all, I would like to thank Mr. Cartan for the kindness with which he has justified the logical interpretation I have given to certain contemporary mathematical theories, some of the finest of which emanate from him. I am also grateful to him for admitting that notions such as local and global, matter and form, are not linked to a specific theory, but can be found in quite different theories, such as analysis or geometry. In short, if Mr. Cartan does not feel the need to refer to a Dialectics for himself, he recognizes the right of philosophers to do so, and no encouragement could be more valuable to them.

I'd be much less inclined to agree with Mr. Fréchet. I spoke of the genesis of the real from the idea. Mr. Fréchet claims to understand only the opposite, i.e. the genesis of the idea from the real, by abstraction of course. It seems to me that we need to distinguish between the historical order of human reflection and the logical or ontological order of dependence of notions. Mathematical theories seem to me to take on their full meaning when interpreted as answers to a dialectical problem or question. Clearly, it is only through an effort of regressive analysis that we can trace the theory back to the Idea it embodies, but it is no less true that it is in the nature of an answer to be an answer to a logically anterior question, even if awareness of the question is posterior to knowledge of the answer. The genesis of which I have spoken is therefore transcendental and not empirical, to use Kant's vocabulary.

As far as Mr. Ehresmann's objections are concerned, I'm convinced that I agree with him, although he doesn't want to admit it. Mr. Ehersmann tells me that the problems I call dialectical remain vague until I specify the statement, at which point they become purely mathematical problems. I myself have written that Dialectics, being affirmative of no actual situation and being pure problematization [*problématique pure*], necessarily extends into actual mathematical theories. The question is whether it is possible to conceive the statement of a logical or metaphysical problem independently of any concrete mathematical solution. The answer to this question lies in the history of philosophy. I'll take just two examples. One is the Leibnizian monad. Is it possible to conceive of all the relationships it maintains with the entire universe as inscribed in the internal properties of a being? This conception of the monad is purely metaphysical, and I believe I showed in my thesis the links between it and current theories of *Analysis situs*, which are also inspired by Leibniz. My second example is the problem of reciprocal action between two or more bodies, a problem that is clearly distinct from Newtonian theory, and which Kant nevertheless believed found its definitive solution in the famous law of universal attraction. History of Philosophy thus demonstrates the autonomy of the conception of structural problems from the contingent elaboration of particular mathematical solutions.

Mr. Chabauty points out that I've attached great importance to theorems that establish the existence of certain functions on certain surfaces or sets, but that this result may seem less surprising if you realize that the sets in question have been "tricked" in such a way that it's not hard to find the functions you're looking for on them. It would seem, then, that you can only find on a set what you have put there beforehand. Such a way of putting things doesn't seem to me to highlight sufficiently the fact that there can be two kinds of "tricking", in Mr. Chabauty's sense: those that are fruitful and those that are not. In terms of properties, a set never possesses more than those given to it *a priori* by axioms, but it may happen that some of these artificial definitions have the consequence of bringing a set or surface to such a state of completion or perfection that this internal perfection blossoms into assertions of the existence of new functions defined on this set. This fruitfulness of certain structural properties, which extends into the genesis of new mathematical beings, seems to me to distinguish, within the possibilities of axiomatic definition, creative conceptions from those that lead to nothing truly new.

Mr. Hyppolite reproaches me for using the term Dialectics in at least three different senses. There's one I don't accept. It's the one according to which there could be a dialectics of the local and the global that would be self-sufficient independently of Mathematics; on the other hand, the other two ways seem to me to complement rather than destroy each other. Mathematics constitutes a genuine Dialectics of the local and the global, of rest and movement, in the sense that Dialectics studies the way in which the abstract notions in question can be composed between them; this does not prevent us from conceiving of a Dialectics prior to Mathematics, conceived as a problematization. Mr. Hyppolite tells me that to pose a problem is to conceive of nothing; I reply, following Heidegger, that it is already to delimit the field of what exists.

Mr. Schrecker has mainly addressed Mr. Cavaillès, but I think we can agree on the legitimacy of a theory of abstract structures, independent of the objects linked together by these structures.

It only remains for me to reply to Mr. Cavaillès. The precise point of our disagreement lies not in the nature of mathematical experience, but in its meaning and scope. That this experience is the *sine qua non* of mathematical thinking is certain, but I believe that we must find in experience something other and more than experience; we must grasp, beyond the temporal circumstances of discovery, the ideal reality that alone is capable of giving meaning and value to mathematical experience. I see this ideal reality as independent of the mind's activity, which in my opinion only comes into play when it comes to creating effective Mathematics; Mathematics does belong to the realm of action, but Dialectics is above all a universe to be contemplated, whose admirable spectacle justifies and rewards the mind's long efforts.