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UFD008 Jean Cavaillès Albert Lautman Robin Mackay (trans.)

Mathematical Thought

In this record of a 1939 meeting, two great philosophers of mathematics, Jean Cavaillès and Albert Lautman, attempt to define what constitutes the 'life of mathematics', between historical contingency and internal necessity, describe their respective projects, which attempt to think mathematics as an experimental science and as an ideal dialectics, and respond to interventions from some eminent mathematicians and philosophers.

On 4 February 1939, the Societé française de Philosophie invited Jean Cavaillès and Albert Lautman to come and 'discuss' together the results of their respective theses. Like Cavaillès, Albert Lautman had just published, in 1938, his two theses: 'Essay on the Notions of Structure and Existence in Mathematics' and 'Essay on the Unity of the Mathematical Sciences in their Current Stage of Development'. The two philosophers, bound by a common friendship, were to meet an identical tragic and heroic destiny, since Albert Lautman would also be executed by the Germans in 1944 for his Resistance activities.[1]

Two very important theses have recently been defended before the University of Paris's Faculty of Letters, on philosophy of mathematics considered from the point of view of the current stage of development of mathematics. The Societé de Philosophie considered it would be of interest to discuss them together; we thank their authors for having been amenable to doing so.

Cavaillès begins from the problem of the foundation of mathematics as it is currently posed and has, in part, been resolved. The result of the crisis of set theory, following the work of Russell and Hilbert, was to transform the epistemological problem into a mathematical one subject to the usual technical conditions. Thus two conceptions of mathematics are today considered untenable:

(1) Logicism ('Mathematics is a part of logic') is untenable, for the effective formalization of mathematics makes it clear:

(a) that in reality, mathematics does not appeal to purely logical notions or operations (leaving aside the problem of what we might mean by such 'pure' notions and operations); but that the considerations involved, which are all homogeneous, belong to combinatory calculus or other mathematical theories (the meaning of a symbol is the way it is used in a formal system);

(b) that it is impossible, given Gödel's theorem, to incorporate mathematics into any one single formal system. Every system containing elementary arithmetic is necessarily non-saturated (that is to say, it is possible to construct within it a proposition that can neither be proven nor refuted within the system);

1. The text is translated from 'La Pensée Mathématique', in J. Cavaillès, *Oeuvres Complètes de Philosophie des Sciences* (Paris: Hermann, 1994), 593–630.

(2) The hypothetico-deductive conception, presented with maximum precision in von Neumann's radical formalism, is also untenable. For one cannot characterise a mathematical theory—an arbitrarily posited system of axioms and rules (according to this conception)—as a deductive system without using mathematical theories that are already constituted, rather than being priorly defined in this way (in number theory, for example Gentzen's proof of non-contradiction has to appeal to transfinite recurrence). In other words, there is an essential solidarity between the various parts of mathematics, which means that it is impossible to trace them back so as eventually to reach an absolute beginning.

Mathematics is a singular becoming. There is no eternal definition

This leads Cavaillès to make the following claims:

(1) Mathematics is a singular becoming. Not only is it impossible to reduce mathematics to something other than itself, but every definition, within a given epoch, is relative to that epoch—that is to say, to the history that gives rise to it. There is no eternal definition. To speak of mathematics is always to remake mathematics. This becoming seems to be autonomous; it seems possible for the epistemologist to find beneath the historical accidents a necessary consecution: the notions introduced were necessitated by the solution to a problem—and by virtue of their sole presence amongst the notions that already existed, they pose, in their turn, new problems. There really is a becoming: the mathematician embarks upon an adventure which can be arrested only arbitrarily, every moment of which endows it with a radical novelty.

(2) The resolution of a mathematical problem bears all the hallmarks of an experiment: a construction subject to possible failure, but carried out in conformity with a rule (that is to say *reproducible*, and thus *non-evental* [*non-événement*]), and which, ultimately, takes place within the sensible. Operations and rules only have meaning relative to an anterior mathematical system: any effectively-thought representation (as opposed to pure lived experience) is a mathematical system in so far as it is thought—that is to say, an organization governed by the sensible (in virtue of the continuity between mathematical gestures, from the most elementary ones to the most sophisticated).

(3) The existence of objects is correlated with the actualization of a method and, as such, is non-categorical, but is always dependent upon the fundamental experience of an effective thought. The illusion of the possibility of exhaustive description (or *ex nihilo* generation) by axioms, foiled by Skolem's paradox, can be explained by the necessary discrepancy between exposition and authentic thought. The latter, the central intuition of a method, in order to be expressed, necessitates completed mathematics (explication of all the successive necessary steps). The objects figure projections in the representation of the steps of a dialectical argument: for them, in each case, there is a criterion of self-evidence conditioned by the method itself (for example: the self-evidence of transfinite induction). They are thus neither an in-itself, nor are they within the world of lived experience—they are the very reality of the act of knowing.

Lautman is entirely in agreement with Cavaillès as regards the solidarity that unites the nature of the mathematical object with the singular experiment of its elaboration through time. There is no determination of true and false except within mathematics itself, and truth is immanent to rigorous proof. But after this common starting point, Lautman diverges from Cavaillès. If we admit that the manifestation of an existent in act only really takes on its full sense as the response to an anterior problem concerning the possibility of this existent, then the establishing of effective mathematical relations would seem to be rationally posterior to the problem of the possibility of such liaisons in general. Moreover, the study of the development of contemporary mathematics shows how the results obtained are organized in terms of the unity of certain themes, which Lautman interprets in terms of possible liaisons between the notions of an ideal dialectic: the penetration of topological methods into differential geometry responds to the problem of the relations between local and global, whole and part; the theorems of duality in topology are concerned with the reduction of the extrinsic properties of a situation to intrinsic structural properties; the calculus of variations determines the existence of a mathematical being through the exceptional properties that allow them to be picked out; analytic number theory demonstrates the role of the continuous in the study of the discontinuous, etc.

It therefore turns out that different mathematical theories can be grouped together as a function of affinities in logical structure, in virtue of the fact that each of them sketches out a different solution to one and the same dialectical problem. This is how, for example, field theory wherein a system of axioms is realized, in mathematical logic, and representation theory for abstract groups, are both able to observe how, in mathematics, the passage takes place between a formal system and its material realizations. There is therefore a sense in which one can speak of the 'participation' of distinct mathematical theories in a common dialectic that governs them.

Dialectics, qua delimitation of the field of the possible, is a pure problematics, an outlining of the schemata which, in order to be drawn out, must be embodied in some particular mathematical matter

The ideas of this dialectic must be conceived as ideas of possible relations between abstract notions, and in recognizing them we make no claims as to any effective situation. Dialectics, qua delimitation of the field of the possible, is a pure problematics, an outlining of the schemata which, in order to be drawn out, must be embodied in some particular mathematical matter. But at the same time, it is this indeterminacy of the dialectic, a manifestation of its essential insufficiency, that guarantees its exteriority in relation to the temporal becoming of scientific concepts.

In conclusion, we can clarify the links between dialectics and mathematics. Mathematics primarily appears to consist in examples of incarnation, domains where the ideal-in-waiting of possible relations is actualized; but these are privileged examples, whose emergence seems as if necessary. For every effort to deepen our knowledge of ideas will naturally play out—and this alone justifies an analysis of such efforts—within effective mathematical constructions. Mathematical thinking thus plays the eminent role of presenting to the philosopher the constantly-recommenced spectacle of the genesis of the real from the idea.

Report of the Meeting

JEAN CAVAILLÈS: The reflections I should like to present are situated at a particular given moment in the development of mathematics—that is to say, the present moment. Owing to the very singularity of this moment, they include two parts, which I have distinguished elsewhere in the summary I gave to you: the first part comprises the results that mathematics itself provides as to the philosophical problem of the essence of mathematical thought; this first part needs only to be conveyed, made explicit; one might dispute the importance of the results described, but I believe that therein lies the incontestable part of my proposal.

This incontestable part, however, turns out to be negative. So, having summed them up, I propose to go on to introduce certain positive reflections that apply to the results obtained, and to the current developments of mathematics we see taking place before our eyes.

I will not insist too much on the first part. In particular, I will not link it as precisely as might be appropriate with earlier stages in mathematical philosophy, especially that of the nineteenth century. Let me just point out very briefly that, in nineteenth-century mathematics, owing to the very development of different branches of mathematics and the necessity of abandoning the intuitive self-evidence upon which one had previously relied, one was led to place a greater emphasis on the notion of proof. Self-evidence gave way to demonstrability. Whence the idea, widespread among almost all mathematicians, and which is found in researchers as different as Frege and Dedekind, that mathematics is a part of logic; that what guarantees mathematical results is the rigorous nature of the chains of reasoning by which they are established.

During this era, then, we see an effort to reduce not only all the initiatives of different mathematicians, but the very notions to which they appealed, to purely logical procedures; an effort which found itself aided by the development of set theory, and which indeed partly stimulated the development of the latter.

We can see how this rapprochement was possible, given that the notion of set itself seemed as far as could be from any intuition whatsoever, and given, on the other hand, that it lent itself to being conflated with the notion of class or extension. As late as 1907, Zermelo, at the beginning of his *Axiomatization of Set Theory*, wrote: 'Set theory is the branch of mathematics whose task is to study mathematically the fundamental concepts of number, order and function in their original simplicity and, for this reason, to develop the logical foundations of arithmetic and analysis.'

So we can see how, even in 1907—that is to say, after the appearance of the great paradoxes—a set-theoretician such as Zermelo still held out the hope of founding mathematics (that is to say, arithmetic and analysis) upon purely logical notions.

This hope was to be disappointed not so much by the difficulties encountered by set theory as it discovered those antinomies, but by the effort mathematicians themselves made in order to decide whether or not this hope could be realized—that is to say, through the efforts by which they transformed a philosophical conception of mathematics into a technical problem for mathematicians.

For when it was required to set out precisely the notion of a set, and the theory that follows from it, one was faced with the need to axiomatize this theory—that is to say, to exhibit the fundamental notions and procedures that it uses. One therefore found oneself in the presence of technical problems that admit of a precise response. This is the work that was accomplished by the school that came together around Russell, and in the Hilbert school, one of whose initiators in France was the tremendously energetic Jacques Herbrand—his absence, for those who knew him, philosophers and mathematicians both, is still felt cruelly every day.

I have indicated the outcome of all this in my summary: since we were dealing with a problem that could be resolved mathematically, two fundamental conceptions of mathematics had to be rejected:

(1) The conception I cited at the outset, the famous hope of reducing mathematics to logic; logicism is eliminated. I will not detail all the reasons for this, I have noted them in my summary and I also direct you, for these details, to my book *Formalism and the Axiomatic Method*.[2]

In trying to formalize mathematics in its entirety, we arrived at the result that the procedures to which one appeals cannot reasonably be called 'logical'. I believe that it would be imprudent to get into the debate on the essence of logical thought itself, which would take us too far afield; but I should like to indicate at least that, if arithmetic is formalized, then the principle of complete induction must be brought in, and it is difficult to reduce the latter to a set of logical notions.

(2) It is impossible to incorporate all of mathematics into one unique formal system. This is the result of the theorem given in the paper Gödel published in 1931.

There remains another possible conception: the famous old conception of a hypothetico-deductive system. This is no longer a question of a single formal system, but of an assemblage of formal systems that are arbitrarily constructed and can be juxtaposed, and which together constitute the ensemble of mathematics.

It turns out that this hypothetico-deductive conception is rendered equally impossible by another theorem published by Gödel in the same paper: 'The non-contradiction of a formal mathematical system containing the theory of numbers cannot be proved by mathematical means non-representable in that system.' As a consequence, 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 would already have to use mathematics.

In particular, keep in mind that, if we consider the formal system of 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 one cannot prove everything in it—it is to prove its non-contradiction. We now have such

2. *Méthode axiomatique et formalisme* (Paris: Hermann, 1938).

a proof, due to Gentzen, which uses transfinite induction—that is to say, a mathematical procedure external to number theory.

I mentioned that the most precise conception of the hypothetico-deductive representation was that of von Neumann. The idea of the Hilbert school was as follows: obviously, we need mathematical notions to characterize a formal system, but these notions are quite elementary. In the hypothetico-deductive system of Hilbert's axioms, the notions necessary to define Euclidean geometry were very simple: whole finite number, and placing into correspondence. But this is illusory, for the non-contradiction of the Hilbert axioms in Euclidean geometry could only be proved by the construction of a system taken from number theory. And for this, in turn, we are obliged to appeal to transfinite induction.

Those are the results, then. And the philosopher may ask, now also bringing in current developments in mathematics, what positive conclusions he can draw.

I will indicate straight away that I do not claim that these conclusions are in their definitive form; it is very difficult work, upon which I present, for the moment, only certain reflections, which I submit to you—reflections which are still a little marked by the effort of the work itself. I will indicate here only those points that I have established with a maximum of certainty.

First point: it seems to me that the idea of defining mathematics is to be rejected, because of the results I have just reported and as a result of a reflection on the work of the mathematician.

Mathematics constitutes a becoming—that is to say, a reality irreducible to anything other than itself. So what could it mean, this enterprise of 'defining' mathematics?

Mathematics constitutes a becoming—that is to say, a reality irreducible to anything other than itself. So what could it mean, this enterprise of 'defining' mathematics? It amounts either to saying that mathematics are that which is not mathematical, which is absurd; or to compiling an inventory of all of the procedures that mathematicians use.

I leave aside the first solution, even if it may still have its advocates. Only the second remains. And I don't think any mathematician would accept that it could be possible to definitively and exhaustively enumerate all of the procedures he uses. It might be possible for some given moment, but it is absurd to say: it is this that is uniquely mathematical, and unless we use these particular procedures we are no longer doing mathematics. I believe that here I am in agreement, on one hand, with the established results—for example, the necessarily non-saturated character of every mathematical theory, which proves that new rules of reasoning must necessarily be brought in every time a new theory is developed; and on the other hand, with the conception of mathematics that we find in intuitionism—and Heyting, for example, has recently written that mathematics constitutes a developing organic system to which we cannot assign any limits.

Mathematics is a becoming. All we can do is try to understand its history; that is to say, to situate mathematics in relation to other intellectual activities, to discover certain characteristics of this becoming. I will enumerate two such characteristics:

(1) This becoming is autonomous. That is to say that, while it is impossible to locate it outside of itself, by studying the contingent historical development of mathematics such as we see it, we can perceive necessities beneath the sequence of notions and procedures. Here, obviously, the word 'necessity' cannot be made more precise in another way. We note the problems, and we perceive that these problems demand the appearance of a new notion. This is all we can do—and certainly the use of the word 'demand' here is too quick, since we are on the other side of things, i.e. we see only the successes. But we can say that the notions that have appeared did truly bring solutions to problems that had effectively posed themselves.

I believe that it is possible, within the picturesque contingency of the sequence of theories, to achieve this work. I have tried, for my part, to do so for set theory; I do not claim to have succeeded, but I do think that I was able to perceive, in the development of that theory which seems the very example of a theory of genius, and which was constructed through radically unforeseeable interventions, an internal necessity: certain problems in analysis gave rise to the essential notions, generated certain procedures already guessed at by Bolzano and Lejeune-Dirichlet, and which became fundamental procedures refined by Cantor. Autonomy, therefore necessity.

This is what we might call the fundamental dialectic of mathematics: although new notions appear as demanded by the problems posed, their novelty itself is truly a complete novelty

(2) This becoming develops like a true becoming. That is to say that it is unpredictable. It is perhaps not unpredictable for the intuitions of a mathematician in full flow, who divines whereabouts he should direct his inquiries, but it is originarily unforeseeable, authentically so. This is what we might call the fundamental dialectic of mathematics: although new notions appear as demanded by the problems posed, their novelty itself is truly a complete novelty. That is to say that one cannot, through the mere analysis of notions already in use, discover the new notions already within them: the generalizations, for instance, that have generated new procedures.

I would characterize this novelty by way of the second point of my conclusion: namely, that the activity of mathematicians is an experimental activity.

By experiment, I understand a system of gestures, rule-governed and subject to conditions independent of these gestures. I recognize that this definition may seem vague, but I believe it is impossible to remedy this wholly without taking up specific examples. I mean by this that each mathematical procedure is defined in relation to a foregoing mathematical situation upon which it partly depends, but from which also it boasts an independence such that the result of this gesture is to be found in its completion. This, I think, is how mathematical experiment might be defined.

Does this mean to say that this experiment bears some relation to what we usually call an experiment? I would rather reserve for it alone the very word 'experiment'; in particular, the experiment in physics seems to be a complex of many heterogeneous elements, something I will not go into today (this would take us too far afield). Specifically, though, unlike a mathematical experiment, in a physical experiment the gestures are not necessarily accomplished according to a rule, nor, on the other hand, does the result have a significance within the system itself, which is always the case for the mathematical experiment. That is to say, given the mathematical situation, the completed gesture gives us a result which, by virtue of its appearance, takes up its place within a mathematical system that extends the anterior system (comprising it as a particular case).

How are such experiments carried out? I have tried to show this, in my book on the axiomatic method, in a very incomplete manner, which I hope to make more precise later; I have indicated several of the procedures that mathematicians use. It is, of course, a description that is bound to be simplifying since, at any given moment, there are certain procedures that are situated in a mathematical atmosphere, a current state of mathematics, and which may not be transferable to another. I have, however, indicated several of these procedures, taking my inspiration both from Hilbert's analyses and those of Dedekind, in his paper delivered before Gauss in 1857, which was approved by Gauss and was recently published, in 1931, by Mlle Noether.

A first procedure is what I call, in general, *thematization*: whereby the gestures carried out on a model or a field of individuals can, in turn, be considered as individuals upon which the mathematician can work, considering them as a new field. The topology of topological transformations, for example (and there are many other examples). This procedure allows mathematical reflections to be superimposed upon one another, and it also has the interest of showing us that there is an unbroken link between the concrete activity of the mathematician from the first moments of his development— placing two symmetrical objects side-by-side, swapping them around—and the most abstract operations; for each time this linkage is found in the fact that the system of objects under consideration is a system of operations which, themselves, are operations upon other operations which, ultimately, are founded upon concrete objects.

A second procedure is what Hilbert calls the *idealization* or *adjunction* of ideal elements. It consists simply in demanding that an operation that is limited, accidentally, by certain circumstances extrinsic to the carrying out of this operation itself, should be freed from this extrinsic limitation, and this through the positing of a system of objects that no longer coincide with the objects of intuition. For example, this is how different generalizations of the notion of number have been made.

What are the consequences of this for the very notion of the mathematical object? I have tried to show this, perhaps unsatisfactorily, I realise—I myself am not entirely satisfied with it, but it is a first approximation.

The mathematical object, in my view, is always correlated with gestures effectively carried out by the mathematician in a given situation

The mathematical object, in my view, is in this way always correlated with gestures effectively carried out by the mathematician in a given situation. Does this mean that such an object possesses a particular mode of existence? Are there, for example, ideal objects, existent in themselves? In the properly mathematical discussions that took place between the advocates of the Vienna School and the Hilbert School, it was asked whether there could be what was called a Platonism (although I don't think this expression is quite right here, but it's not the word that matters)—whether there is a region of ideal objects to which mathematics might refer; this is what Gentzen, in an article that appeared this summer, called 'the mathematical in-itself'.

I believe that on this matter I can go further than Gentzen, who attempts to reconcile mathematics-in-itself with the constructivist demands of intuitionism; I do not believe that a conception of systems of mathematical objects existing in themselves is at all necessary in order to guarantee mathematical reasoning. For example, in regard to the continuous, this conception of mathematical objects must be rejected for a quite simple reason: that it is completely useless, both for the development of the mathematics itself and for an understanding of this development.

For if this 'Platonism' really meant anything precise, it would be to say that, if the objects to which the mathematician refers cannot be grasped in any intuition whatsoever, at least their properties, the simultaneous presence of their properties, must have been necessary at some point in the mathematician's reasoning. Not only has this not taken place, but even if we wish to clarify what it might mean, we run up against difficulties which oblige us to resist such a conception; I am alluding here to Skolem's paradox.

I do not wish to expound on this paradox, all the more so because, to explicate it precisely, we would have to use a formalization. But in broad outline, it says the following: If we have a model which we suppose to satisfy a system of axioms, it is always possible to construct a denumerable model satisfying this same system of axioms. In particular, one can satisfy a system of set-theoretical axioms with a denumerable model.

This paradox, upon which Skolem himself and many others (this summer, Gentzen) have long reflected, amounts to saying that it turns out to be impossible to exhaustively characterize a model satisfying a system of axioms. If we suppose the axioms—that is to say, the enumerations of the properties we need for the objects—to be posited, we cannot expect these axioms at the same time to generate the objects. Rather, we are obliged to suppose 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 cannot say is that our field of objects can be characterized in a uniform fashion by our system of axioms.

What is interesting here is that this not only eliminates such an idealist conception, so to speak, of the existence of mathematical objects, but that it also indicates the intimate solidarity by which the moments of mathematical development are linked together.

There is no starting from zero. We can say that mathematics emerges within history; but if we wish to make precise what we understand by this—either through the activity of enumeration, which already implies what Poincaré called the intuition of pure number, or through the beginnings of elementary geometry—then we are obliged, in reality, to argue through all of mathematics; we can of course stop arbitrarily at some point, saying: this state here satisfies us—but, if we are faithful to the very exigency that presided over the birth of these notions and their development, then we must raise problems that are born, for example, of the refusal to limit ourselves to circumstances external to the problem posed; and as soon as we do so, new notions appear and engender not only mathematics up to the present day, but the exigencies of development, the unresolved problems that provoke the transformations they are undergoing today.

The very notion of an existence of mathematical objects interests us, we other philosophers, because it poses the problem of the very notion of the existence of the objects of thought

In conclusion, I would say that the very notion of an existence of mathematical objects interests us, we other philosophers, because it poses the problem of the very notion of the existence of the objects of thought.

What is it for an object to exist? Here we find ourselves in the presence of the fact that the very type of certain, rigorous knowledge, which is precisely mathematical knowledge, does not allow us to posit objects as existing independently of a necessary sequence that starts with the very beginning of human activity itself.

Which means that we can neither posit them in-themselves, nor say, with exactitude: here is the world—a world that we describe. In each case, we are obliged to say: these are the correlates of a certain activity. All that we think in themselves are the rules of mathematical reasoning that are demanded by the problems that pose themselves; and there is even an overflowing, a demand for excess made by unresolved problems, and which obliges us to posit yet other objects, or to transform the definition of objects that were posited at the start.

Such are the reflections I wish to present; I have not hidden their partial, insufficient character, which is striking even to my own eyes. But I believe that the current state of mathematics requires something of the sort.

ALBERT LAUTMAN: Having heard M. Cavaillès speak, I am even more convinced that I do not agree with him; and I shall try, during the time I have to speak, to detail the points at which our conceptions diverge. In what he calls the mathematical experiment, M. Cavaillès seems to me to attribute a considerable role to an activity of the mind, which determines, in time, the object of its experiment. According to him, then, there are no general characteristics constitutive of mathematical reality; on the contrary, this reality is affirmed at every moment of the history of mathematics, whereas Platonism is identified with a theory of the 'in-itself' existence of mathematics.

The objectivity of mathematical beings, which manifests itself most manifestly in the complexity of their nature, only reveals its true meaning within a theory of the participation of mathematics in a higher and more hidden reality

Like M. Cavaillès, I recognize the impossibility of such a conception of an immutable universe of ideal mathematical beings. It is an extremely seductive vision, but one of far too weak consistency. The properties of a mathematical being depend essentially upon the axioms of the theory within which that being appears; and this dependency strips them of the immutability that supposedly characterizes an intelligible universe. Just like him, I consider numbers and figures to possess an objectivity as certain as that met with by the mind in the observation of physical nature; but this objectivity of mathematical beings, which manifests itself most manifestly in the complexity of their nature, only reveals its true meaning within a theory of the participation of mathematics in a higher and more hidden reality—a reality which, in my view, constitutes the true world of ideas.

To understand properly how the study of recent developments in mathematics might justify the Platonic interpretation I am proposing, I must insist first of all upon what is called the structural aspect of contemporary mathematics. It is a question of mathematical structures; but as we shall see subsequently, it is easy to trace these mathematical structures back to the consideration of the dialectical structures incarnated in actual mathematical theories.

The structural aspect of contemporary mathematics is manifested by the importance of the role played, in all branches of mathematics, by Cantor's set theory, Galois's group theory, Dedekind's theory of algebraic number fields. What is characteristic of these different theories is that they are abstract theories; they study the possible modes of organization of elements of a nonspecific nature. This is, for example, how it is possible to define the global properties of order, of completeness, of division into classes, of irreducibility, of dimension, of closure, etc., which qualitatively characterize the collections to which they are applied. A new spirit now animates mathematics: lengthy calculations give way to the more intuitive reasoning of topology and algebra. Consider, for example, what mathematicians call existence theorems—that is to say, theorems that establish the existence of certain functions or certain solutions without actually constructing them. In very many cases, the existence of a function that is sought can be deduced from the global topological properties of an appropriately defined surface. In particular, since Riemann, this is how a whole geometrical theory of analytic functions has developed which allows us to deduce the existence of new transcendent beings on the basis of 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 into an affirmation of the existence of the function sought.

If we reflect upon the internal mechanism of the theory to which we have just alluded, we must take account of the fact 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)—that is to say, in short, between the essence of one being and the existence of another. These notions of essence and existence, like those of *form* and *matter*, *whole* and *part*, *container* and *contents*, etc., are not mathematical notions; nevertheless, it is toward them that the consideration of effective mathematical theories leads. I call them dialectical *notions*, and propose to call dialectical *ideas* the problem of the possible liaison between dialectical notions thus defined. The reason of the relations between dialectics and mathematics thus resides in the fact that the problems of dialectics can very well be conceived of and formulated independently of mathematics, but that every sketching out of a proposed solution to these problems will necessarily rest upon some mathematical example designed as a concrete support for the dialectical liaison in question.

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Consider, for example, the problem of the relations between form and matter. We can ask to what extent a form determines the existence and the properties of the matter to which it can be applied. Here is a capital philosophical problem for every theory of ideas, since it is not enough to posit the duality of the sensible and the intelligible; we must also explain the participation, that is to say, whatever we decide to call it, the deduction, the composition, or the genesis of the sensible from the intelligible. And mathematics precisely gives us, in certain cases, remarkable examples of the determination of matter on the basis of form: the aim of the entire theory of the representation of abstract groups is to define a priori how many different concrete transformations can be carried out on a given abstract group structure. In the same way, contemporary mathematical logic demonstrates the close relation between the intrinsic properties of a formal axiomatic and the extension of the fields of individuals within which this axiomatic is realised. Here we have the spectacle of two theories as distinct as can be, the theory of group representation and mathematical logic, which nevertheless entertain close analogies of dialectical structure; analogies which result from each of them being a particular solution to the same dialectical problem, that of the determination of matter on the basis of form.

One passes insensibly from the comnprehension of a dialectical problem to the genesis of a universe of mathematical notions, and it is the recognition of this moment when the idea gives birth to the real that, in my view, mathematical philosophy must aim at

I have indicated above that the distinction between a dialectics and an effective mathematics must above all be interpreted from the point of view of the genesis of mathematics from dialectics. Which I understand as follows: dialectics, in itself, is a pure problematics, antithetic, fundamental, and relating to couplets of notions which appear, on first sight, to be opposed, but with regard to which nevertheless the problem of a synthesis or conciliation is posed. It is in this way, for example, that I envisaged in my thesis the problem of the relations between local and global, extrinsic and intrinsic, continuous and discontinuous, etc. Then we find, just as in Plato's *Sophist*, that the contrary terms are not opposed, but are susceptible to being composited together, to constitute the mixtures that are mathematics. Whence the necessity of these ever so complicated subtleties, of that unforeseeable vividness, of those obstacles that one sometimes overcomes, sometimes circumvents, of that whole historical and contingent becoming that constitutes the life of mathematics, but which to the metaphysician looks like a necessary extension of an initial dialectic. One passes insensibly from the comnprehension of a dialectical problem to the genesis of a universe of mathematical notions, and it is the recognition of this moment when the idea gives birth to the real that, in my view, mathematical philosophy must aim at.

I have tried to show, in a booklet published by Librairie Hermann since my thesis,[3] the analogy of these conceptions with those of Heidegger. The extension of the dialectic into mathematics corresponds, it seems to me, to what Heidegger calls the genesis of ontic reality from the ontological analysis of the Idea. One thus introduces, at the level of Ideas, an order of before and after which is not that of 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 relation 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; here is a purely philosophical problem, which we could say that Kant posed without resolving it, in the third category of relation. It will nevertheless be found that, as soon as the mind tries to think what the coexistence of several bodies in space might be, it is necessarily caught up in the still unsurmounted difficulties of the *n*-body problem. Consider also the problem of the relation between *movement* and *rest*. One might posit abstractly the problem of knowing whether the notion of movement has no meaning except in relation to some change or other; but every effort to resolve such difficulties gives rise to the subtleties of the Theory of Special Relativity. One can equally ask to which of the two notions of movement and rest one should attach a physical meaning, and this is a point upon which classical mechanics and wave mechanics part ways. The latter envisages the wave as a real physical movement; for the former, on the contrary, the wave equation no longer figures as an artifice designed to bring to light the physical invariance of certain expressions in relation to certain transformations. It thus seems that the theories of Hamilton, Einstein, and Louis de Broglie all articulate their meaning in reference to notions of movement and rest whose veritable dialectic they constitute. It may even be that what physicists call a crisis of contemporary physics, as it struggles with the difficulties of the relations between continuous and discontinuous, is a crisis only in relation to a certain, fairly sterile conception of the life of the mind whereby the rational is identified with unity. It seems on the contrary more fruitful to ask whether reason in the sciences does not rather have as its aim to discern, in the complexity of the real, in mathematics and physics alike, a mixture the nature of which can be explained only by tracing it back to the ideas within which this real participates.

3. *Nouvelles recherches sur la structure dialectique des mathématiques* (Paris: Hermann, 1939).

It remains, finally, to remake the Timaeus—that is, to show, within ideas themselves, the reasons for their applicability to the sensible universe

We thus see what the task of mathematical philosophy, and even of the philosophy of science in general, must be. A theory of Ideas is to be constructed, and this necessitates three types of research: that which belongs to what Husserl calls descriptive eidetics—that is to say the description of these ideal structures, incarnated in mathematics, whose riches are inexhaustible. The spectacle of each of these structures is, in every case, more than just a new example added to support the same thesis, for there is no saying that it might not be possible—and here is the second of the tasks we assign to mathematical philosophy—to establish a hierarchy of ideas, and a theory of the genesis of ideas from out of each other, as Plato envisaged. It remains, finally, and this is the third of the tasks I spoke of, to remake the *Timaeus*—that is, to show, within ideas themselves, the reasons for their applicability to the sensible universe.

These seem to me to be the principal aims of mathematical philosophy.

Discussion

ELIE CARTAN: I am rather embarrassed, since I am in a somewhat similar situation to M. Jourdain, who spoke in prose without knowing it. Mathematicians—at least some of them, among whom I number myself—are not at all accustomed to reflect upon the philosophical principles of their science; when they hear a philosopher speak in such a way, it interests them, no doubt, but they do not really know how to respond to the considerations the philosopher introduces.

Obviously, I know M. Cavaillès's thesis and M. Lautman's thesis, since I was on the jury of both, but my situation is different: beforehand, I was on the right side of the barricades, whereas today, I am on the other side....

I did not really understand what opposition there was between the two points of view of M. Cavaillès and M. Lautman, points of view that seemed to me different rather than opposed. I had the impression that M. Cavaillès's concerns bear on the very ground of mathematical thought, whereas M. Lautman's concerns bear rather upon the current state, not of the whole of mathematics, but of a certain number of mathematical theories—and, in this regard, there are obviously a number of M. Lautman's affirmations that particularly interested me: Certainly, these relations are present in a large part of mathematics. The theory of functions, in particular functions of a real variable, such as it has been conceived for fifty years now, cannot pose the problem of the relation between the local and the global; the functions envisaged are too general for one to be able to deduce their global properties from their local properties; something analogous goes for quasi-analytic functions, which have recently been introduced: when we know at a point the values of the function and those of its successive derivatives, it is completely determined in its whole field of existence.

In geometry—it is above all geometry that M. Lautman was thinking about—there are also extremely important problems for which the relation between the local and the global is posited: for example, given a space, if we take a small section of that space, is it possible, through knowledge of that little section, to deduce knowledge about the whole space? Of course, we must suppose this space to have fairly simple global properties, otherwise the problem would be meaningless. These are, apparently, problems of pure geometry, yet in reality they are also problems of analysis. Take, for example, a portion of Riemannian space: if you suppose that the functions used to define this space are analytic, you will have an extremely interesting problem, which is as follows: with knowledge of a small section of Riemannian space defined analytically by its differential form, to what point can one deduce the global properties of that space? It may happen that this small section cannot be extended so as to form a complete space; in general, this is the case. If it can be extended in such a way as to form a complete space, this can be done in one way only, with certain restrictions.

Thus here is a problem of the relations of global and local which is not defined simply by stating it geometrically, but is linked to the existence of purely analytic properties in the definition of a section of space.

One might develop analogous considerations on the subject of the relations between the intrinsic and the extrinsic. Given a surface immersed in a certain space, do the supposed known intrinsic properties of the surface imply limitations of the properties of the space that contains it? These are all extremely interesting problems; but one must remark that they depend not only upon the geometrical posing of the problem, but its being posed in analytic form.

Lautman gave a certain number of other examples of such problems: form and matter, group theory. All very interesting, but I'm not sure to what extent this justifies M. Lautman's general thesis, for I don't understand very well what the dialectic is, and I am obliged to limit myself to purely technical terrain.

I don't get the impression that M. Lautman's concerns are in contradiction with M. Cavaillès's. I get the impression that M. Lautman considers certain particular problems of current mathematics, and a certain number of philosophical problems. I believe I am, on the whole, in agreement with him, but, unfortunately, I am incapable of discussing with him on his terrain.

There have been in the history of mathematics—which I know, which I have lived—certain predictions of what was to come

In any case, as far as mathematics' boasting an autonomous and unpredictable character, I don't think one can deny this claim. However, history teaches us that there have been in the history of mathematics—which I know, which I have lived—certain predictions of what was to come: in 1900, Hilbert gave a paper on the future problems of mathematics, an extraordinarily remarkable paper because, precisely, he put his finger on the problems that would come to be posed in the development of mathematics over the next fifty years at least; and he foresaw correctly the most important problems that were in fact posed. On the contrary, we might find papers by eminent scholars on the future of this or that branch of mathematics, in which these scholars did not at all manage to foresee how things would develop.

Certainly, the development of mathematics has something of the unforeseeable about it, and when one reaches a certain age, one realizes how certain theories, after twenty, thirty, or forty years, take entirely unforeseen paths, and that the point of view from which one comes to see them from is entirely different from the initial point of view. And yet one is indeed obliged to recognize that it is internal necessities that are revealed in the ulterior development of these theories. I am thinking, for example, of topology, a science that has existed for barely half a century, but which every day takes on a new aspect and gives rise to entirely unexpected developments, penetrating deeper and deeper into all branches of mathematics.

M. PAUL LEVY: First of all, I could repeat what M. Cartan has just said: I am a little disconcerted when I hear philosophers speaking of the science that I study, in a language to which I am little accustomed. I follow them with a little effort and I am not sure of understanding all that they say. I believe myself to be fairly sure of understanding some of it; but I am equally sure of not having understood certain things.

So I cannot give an opinion on all of the questions that have been set out; I can only present some reflections that were suggested to me in particular by M. Cavaillès's paper, and I believe that they are not on beside the point; if I am wrong, you will have to excuse me.

I think I am somewhat in opposition to M. Cavaillès; however, his conclusion reassured me, when he said that in the becoming of mathematics, certain internal necessities revealed themselves.

I believe that the development of mathematics—while subject to great contingency, that goes without saying—supposes far more profound internal necessities

I believe that the development of mathematics—while subject to great contingency, that goes without saying—supposes far more profound internal 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 great role, and there are theorems of which I could say to you: if this scholar hadn't discovered this theorem at this time, and if this theorem hadn't been demonstrated in this year, it would have been discovered within the following five or six years. As proof of this, I remind you that a great number of theorems have been, in quick succession, discovered separately by different thinkers, because they respond to a necessity in the development of mathematical thought at the time.

Consequently, this allows me to think that, when a certain mathematical theory has been initiated, a superior mind can foresee a little in what direction it is going to develop. I take as a concrete example one of the mathematical theories whose philosophical aspect has drawn the most attention: the theory of the integral, such as modern set theory allows us to construct it. It is M. Lebesgue who gave the notion of integral its definitive form and, as you all know, currently the integral is an essential mathematical tool. It is indispensible to the extent that, without any doubt whatever, if M. Lebesgue hadn't existed, his integral would all the same have been discovered before long. I don't mean to diminish the merit of M. Lebesgue—I think, on the contrary, that I can only add to it, in saying that he brought to light a notion that was necessary for the further progress of science. Would M. Émile Borel, who was already working in this order of ideas, have brought this theory to completion? Would it have fallen to another of his students to do so? I don't know. But after the work of Jordan and M. Borel, given the current state attained by the whole of humanity and the number of specialized researchers in the domain of mathematics, I believe it was necessary and predestined that, within ten or fifteen years, Lebesgue's theory of the integral would have come about. And, thinking about it this way I do believe, to a certain extent, that the development of mathematics is predictable.

Of course, one must not deny that, on the other hand, certain discoveries constitute an unforeseeable leap in the development of science; coming before their time, it sometimes happens that their importance is recognized only after a more or less lengthy gap. On the other hand, it is certain that there are, among mathematicians, geometers and algebraists; the first evolve in one branch of mathematics, the others in another; it's conceivable that the human species could have contained only geometers, and no algebraists; or inversely. in the same way, it is possible that a later development of humanity will permit certain brains to dedicate themselves to branches of mathematics of which we can have no conception today.

It is possible that a later development of humanity will permit certain brains to dedicate themselves to branches of mathematics of which we can have no conception today

On the other hand, there is a point upon which our two speakers found themselves in agreement and, in so far as I understood them, I am a little surprised. For me, there would be no reason for mathematics to exist were its object considered to be inexistent. When I say that the product of two numbers is independent of their order, it is something that is true, independently of the fact that I say it; it is not true only within my thinking.

To take a simple example, which can be verified objectively: I have rectangular cases comprising a certain number of rows and columns; I have a certain number of balls and I want to put one in each case; well, the same number of balls are enough whether I fill the cases row by row or column by column. I use this very simple example because, in others, it would be difficult to find a material interpretation allowing us to verify the exactitude of a theorem.

For me, the theorem is preexistent; when I seek to demonstrate if some statement is true or false, I am convinced that it is true or false in advance, independently of the possibility of my discovering it.

Let's take another problem: Riemann's hypothesis on the $\zeta(s)$ function, is it correct or not? I believe most mathematicians are convinced that it is correct, even though none of them can prove it; and

I think that all mathematicians in this room would agree in saying that we may perhaps never prove it, but that this hypothesis is, in itself, either true or false, even if we never come to know if it is true or false.

If I understand your language properly, you would express my position by saying that I am a Platonist; but I cannot conceive of anything that might make me abandon this point of view.

M. FRÉCHET: I will begin by agreeing with an observation that has been made before me, successively, by Messrs Cartan and Lévy: for a mathematician who dedicates the principal part of his activity to mathematics, it is extremely difficult to follow in all their nuances the expositions of Messrs Lautman and Cavaillès, which were nevertheless most instructive. The difficulty in discussing them is perhaps not so much owing to what they have said as in the prior necessity of understanding exactly what they wished to say.

Before entering into some details, I would however 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, ourselves, immersed in mathematics and—speaking for myself, at least—entirely ignorant of the subtleties of philosophical language and the nuances that differentiate certain philosophical theories; whereas our distinguished colleagues seem, on the contrary, to advance with great ease not only in philosophy but also in mathematics. Finally, they know, with regard to technical points and results from certain parts of mathematics, many things which personally I know nothing of.

Precisely for the reasons I have indicated, I do not want to take up one by one the different subjects they spoke of. But there are two or three points upon which I have perhaps understood their thesis, and upon which I should like to say a word.

To justify the existence of mathematics, it is indispensable to see it as a set of instruments that was invented to help man to know nature

It is a matter, firstly, of two questions that are connected, at least in my mind, and to which I may be able to respond: M. Cavaillès has indicated that, in his view, mathematics is an autonomous science. Personally, I do not think so. It all depends, firstly, of course, on what we are calling 'mathematics'; many people call 'mathematics' the set of deductive theories that allow us to pass from a set of properties and axioms to certain theorems. No doubt this is the most specific part of mathematics; but it seems that, if one stops there, not only will mathematics be reduced to a machine for transformations (in which case their role will still be very important) but that they will be limited to transforming, so to speak, one empty proposition to another. I think that, to justify the existence of mathematics, it is indispensable to see it as a set of instruments that was invented to help man to know nature, to understand it and to foresee the course of phenomena. The notions that seem to me the most fundamental in mathematics are all the notions that do not, in my opinion, come from our own intelligence, from our minds, but that are imposed upon us by the external world.

I would cite, for example, the whole number, the straight line, the plane, the ideas of speed, of force, certain transformations such as symmetry, similarity. These are notions that were not present in our mind, but which were imposed upon us by the consideration of the world that surrounds us. We have translated these external realities into the words of axioms, definitions—which only represent them approximately, of course, which are more simple, so as to be more manipulable—but which, all the same, originated in the external world.

To these fundamental notions, which we find at the origin of mathematics, others are constantly added, introduced by the development of the physical sciences. The notions of work, of the moment of a force, for example, were not defined, to my knowledge, until two or three centuries ago. Many other notions that I could indicate, such as differential equations, were not introduced until the modern epoch, owing to the development of physics, of mechanics, of astronomy, etc.

Alongside these notions whose study is, so to speak, imposed upon us, other notions of a different nature have been introduced into mathematics: those that owe their existence to the 'internal activity' of this science. They seem to me far less fundamental than the others, having been imagined so as to facilitate the task of the mathematician, in view of the resolution of problems posed from without.

To give some elementary examples, I would cite transformation by inversion, transformation by reciprocal polars; here are two transformations which, as far as I know, were not imposed upon us by examples taken from nature, but are artifices of mathematicians which give us a means of investigation.

In the same way, I think that the introduction of complex numbers furnished an extremely powerful instrument that allowed us to obtain far more rapidly certain propositions concerning real numbers.

We could cite many other examples: in elementary geometry, one has introduced the consideration of supplementary triads. Here again, I do not believe that there was a real phenomenon that imposed upon us the consideration of these supplementary triads, but they furnish a commodious tool to be used in elementary geometry to transform one proposition into another.

Consequently, in the examples I have just cited, I can see two categories of notion: some of them do indeed enter into the framework of an autonomous mathematics, and others, on the contrary, I do not think can be reconciled with the idea of an autonomy of mathematics.

And this leads me, on the contrary, to find myself in agreement with Mr Cavaillès, although for reasons different to his, as to the unpredictability of mathematics, upon which I take a point of view that is moreover entirely congruent with that presented by M. Paul Lévy, but which would seem to lead to the contrary conclusion.

Lévy indicated numerous examples where problems cannot help but be resolved by mathematics; and said that, in this sense, mathematics is predictable, because it is a matter of problems that mathematicians posed themselves *for the internal development of* mathematics.

We do not know, we cannot even imagine, what will be the nature of the problems that, in fifty years, technology or physics may pose to mathematicians

But in the development of sciences external to mathematics, there are constantly problems that pose themselves, that impose themselves upon mathematicians, that mathematicians are asked to resolve, and which give them new ideas, leading them to introduce new notions. And those notions are unforeseeable. We do not know, we cannot even imagine, what will be the nature of the problems that, in fifty years, technology or physics may pose to mathematicians; perhaps we shall have the means to resolve these problems by drawing on the existing arsenal of mathematical theories; but here there is an impulse that comes from outside, and whose interventions are by their nature unforeseeable.

That's what I want to say on the subject of the autonomy and unforeseeability of mathematics.[4]

As to M. Lautman's thesis, I am a little hesitant to comment on the most of it, for I find it subject to different possible interpretations. Some of them seem to me immediate and acceptable, but I cannot reconcile them with his conclusion. This probably owes to the face that I have not properly understood.

I see, at the beginning, phrases such as: 'The establishment of effective mathematical relations appears to me to be rationally posterior to the problem of the possibility of such liaisons in general.'

What is more, M. Lautman has taken care to indicate that, for him, it is not a matter of an historical point of view. And, indeed, from the historical point of view there is no doubt as to the response: the establishment of effective mathematical relations is, on the contrary, certainly anterior to the problem of the possibility of such liaisons.

4. I have developed, among others, these two points in a report presented at Zurich in December 1938 on *The Question of the Foundations of Mathematics and General Analysis* at a colloquium organized by the International Institute of Intellectual Cooperation, whose debates are published under the auspices of that Institute.

So, what does it mean exactly to say: 'rationally posterior'? Same question for the phrase: 'We see in what sense one can speak of the participation of distinct mathematical theories in a common dialectic that governs them.'

Considering these two phrases and the text surrounding them, It seems to me that one naturally arrives at this response: different mathematical theories (above all the proofs contained in those theories) consist in reasoned arguments applied to certain particular circumstances, but they all belong to the same general theory, which is, I believe, what M. Lautman calls a theory of ideas, but which mathematicians probably call logic.

If this is the case, I believe that everyone would be in agreement, but it seems so obvious that I can't believe that this is quite what M. Lautman intended to say. In any case, it could hardly be reconciled with the conclusion of his presentation: 'Mathematical thinking thus plays the eminent role of presenting to the philosopher the constantly-recommenced spectacle of the genesis of the real from the idea.'

It is the exigencies of the real that have posed mathematical problems, that have led mathematicians to make use of logic and to formulate certain definitions, certain axioms

I don't know exactly what this might mean, but after my reflections just now, it seems to me that it is the real that has engendered the idea, at least in so far as mathematics are concerned; it is the exigencies of the real that have posed mathematical problems, that have led mathematicians to make use of logic and to formulate certain definitions, certain axioms.

So I can see very well the genesis of the idea from out of the real, but I must say that I don't understand the inverse position. Perhaps the remaining discussion will elucidate this point?[5]

5. At the point when my intervention was being typed up, I remarked that in fact the principal difficulty, for me, was indeed to understand in a precise and exact fashion Mr Lautman's language. As the latter indicated in his response, what he means by the real does not correspond either to the concrete, to the sensible, with which I identified the real.

M. EHRESMANN: I have noted down some reflections relating to M. Lautman's thesis. It seems to me extremely interesting to see being taken up general problems that we find over and again in many mathematical theories. But I would cite one of the most characteristic phrases: 'One of the essential theses of this work affirms the necessity of separating the supra-mathematical conception of the problem of liaisons that certain notions hold between each other, and the mathematical discovery of these effective liaisons within a theory.'

If I have understood correctly, it would not be possible, in this domain of a supra-mathematical dialectic, to specify and to study the nature of these relations between general ideas. The philosopher could only bring to light the urgency of the problem.

We cannot stop midway: we must pose the truly mathematical problem that consists in formulating explicitly the general relations between the ideas in question

It seems to me that, as soon as we care to speak of these general ideas, we already conceive in a vague way the existence of certain general relations between them; meaning that we cannot stop midway: we must pose the truly mathematical problem that consists in formulating explicitly the general relations between the ideas in question.

I believe that one can give a satisfactory solution to this problem as far as the relations between part and whole, global and local, intrinsic and extrinsic, etc. are concerned. The relations between a fundamental set and its parts form are precisely the object of a chapter in abstract set theory. Between the parts of a set, we have the following relations: *inclusion* of one part in another, *intersection* of two parts, *union* of two parts, and *complementary* part of a part. In the set of parts of a fundamental set, these relations give rise to a whole calculus, namely Boolean algebra. Here are a certain number of general relations that we find again in any mathematical theory.

Given a fundamental set endowed with a *particular* mathematical structure, for example the structure of a group or the structure of a topological space, the relation between this fundamental set and one of its parts is expressed in the *mathematical* notion of structure induced on the part. I cannot elaborate further, because one would have to firstly define the general notion of a mathematical structure. The problem of relations between intrinsic and extrinsic properties of a situation of a part in a fundamental set, is nothing other than the problem of the relations between the structure of the fundamental set and the structures induced on a part and on the complementary part.

As far as the notions of *local* and *global* are concerned, it seems to me that the notion of local makes no sense outside of a structure of topological space: as we now have the notion of the neighborhood of a point, the notion of local property at a point can be deduced from the notion of structure induced upon *any neighborhood whatsoever* of the point. And so we arrive once again at a purely mathematical notion.

We could give more examples. I think that the general problems raised by M. Lautman could be stated in mathematical terms, and I would add that one cannot prevent them from being stated in mathematical terms. And this comes back to the thought expressed in the summary of M. Cavaillès's thesis: 'To speak of mathematics can only be once more to do mathematics'.

M. HYPPOLITE: I must firstly admit that, although I perfectly understood Mr Cavaillès's thesis, I understood Mr. Lautman's far less well.

What struck me in Mr Lautman's exposition was the ambiguity of the word 'dialectic' and the different senses in which this word was employed. It seems to me that—applied to mathematics—the word 'dialectic' was used in three different senses—or, at least, I believed that I could discern three distinct senses of the term.

With the first sense of the term, M. Lautman rejoins Cavaillès's thesis—their two conceptions, on this point, are very close: the dialectic is the very experience of the life of mathematics; it reconciles, in some way, a necessity of development, of which we have already spoken, and the apparent contingency of this development.

The dialectic is the very experience of the life of mathematics; it reconciles, in some way, a necessity of development, and the apparent contingency of this development

In another sense, M. Lautman's dialectic is a sort of problematic, in the modern sense of the term, which is something entirely different; I believe that it is above all in this sense, moreover, that he uses the word; this dialectic is a problematic, a sort of opening onto theoretical problems that the mathematician comes to incarnate in his researches.

And in a third sense—and precisely here, it seems to me, the ambiguity is the strongest—M. Lautman once more takes up the word 'dialectic' in the sense that philosophers have most often used it. For here it is a matter of a dialectic of 'form and matter, of local and global', etc. For my part, it seems that, if one really wants to use the word 'dialectic' in philosophy of mathematics, one must use it uniquely in the first sense—that is, the sense of a life of the mathematical experience through the course of its history.

To take an example, which struck me greatly: the development of the theory of equations, from Viète to Galois. I would say that if there is a necessity—as M. Cartan insisted—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 when it comes to the discoveries of Galois. It seems that here there is, in mathematical theory, something entirely new, something unexpected that was introduced and that could not have been foreseen exactly on the basis of foregoing developments in mathematics. This is something that has struck me greatly, in studying the decomposition of a group into invariant subgroups in Galois, and the application of this problem to the algebraic resolution of equations, after having studied the problem of the theory of algebraic equations in Descartes. It seems to me that, in this case, we can perceive both a necessary development, and then the appearance of an entirely new method in the problem, an unforeseeable creation, even if we only perceive this after the fact.

This problem of the evolution of the theory of equations from Viète to Galois inspires another remark, which one might express vulgarly by saying that we do not know how to undo what we know how to do, or that intellectual activity surpasses itself in what it engenders. Given equations seem enigmatic mathematical beings in a certain way. We know how to construct them, with the products of binomials, as Harriot did; we can thus manage to construct equations of any degree whatsoever; but we are incapable, subsequently—the problem of division after multiplication—of undoing any given equation.

It was necessary, to attempt this analysis in general, to introduce new notions which, moreover, could be understood in a certain way, such as for example the imaginaries as foreseen by Descartes: Descartes, in 1637, said explicitly that there were *n* roots of the equation of n^e positive degrees, negative or imaginary; which was a forecast of something that would appear much later.

I think, to sum up, that I agree more with M. Cavaillès, who sees in mathematics an essential autonomous life; we might also consider that the necessity of the development of mathematics and historical contingency must be reconciled in this 'life of mathematics'.

As to M. Lautman's thesis, one may well fear, in adopting it, that mathematical notions would evaporate, in a certain way, into pure theoretical problems that surpass them: such as form and matter, the local and the global. The very originality of this 'mathematics' would be at risk of disappearance.

I did not understand very well, in M. Lautman's thesis, whether the mathematician ends up finding these problems once more, or whether, on the contrary—and this would be the problematic—an ideal exigency of these problems which, given from the outset, came subsequently to be incarnated in mathematics.

There is an ambiguity there; but perhaps I did not fully understand M. Lautman's thesis.

M. SCHRECKER: After so many mathematical concerns, perhaps it will be permitted to a philosopher to present some reflections that do not respect absolutely the autonomy within which mathematicians necessarily sequester themselves. It concerns the impossibility, affirmed by M. Cavaillès, of defining mathematics. According to him, every definition of mathematics ends up in absurdity, since it would be impossible to define mathematics by anything that they are not. But it seems to me that this same difficulty is found in all the sciences: no science can be defined through its own means and methods, one must always place oneself outside of a science in order to be able to arrive at a definition of its domain.

But this is not to say that one would necessarily define mathematics through something that they are not. Mathematics is a science: here is the first element of a definition, and one that is certainly not heteronomous. It is a hypothetico-deductive science: here is a second element. But it is true that one cannot define it while remaining entirely within mathematical formalism and respecting, in the definition, the autonomy of the mathematical domain. Formalism and autonomy are applicable for all mathematical problems: however, the definition of mathematics is not itself a mathematical problem; it is a problem that poses itself to the theory of science, which is not at all obliged to insert itself into the coherency of mathematical formalism itself.

Thus the refutation of the hypothetico-deductive character of mathematics seems to be to be circular, since this refutation itself uses the hypothetico-deductive method. The attempt was made to carry through this refutation by way of an argument that, being deductive, is necessarily also hypothetical, because it supposes the efficacy of the formalism through which it operates. In denying in this way the hypothetico-deductive character of mathematics, one turns in a closed circle or in a closed system that has neither entrance nor exit....

M. CAVAILLÈS: I never denied the hypothetico-deductive character of mathematics, I only said that one can only define it in this way, since one must employ mathematical theories.

M. SCHRECKER: But it is obvious that, if one tries to define mathematics by making use of mathematical theories, one will never succeed. If, on the contrary, one decides to define it by other means, emancipating oneself from formalism and using historical or philosophical methods, it seems possible to succeed.

And this all the more given that, no doubt, we know how to distinguish mathematics from other sciences, when we undertake its history or when we consider it as an object of philosophy.

Certain great mathematicians have proposed a definition that, if it is not absolutely satisfactory, nevertheless seems to me to be on the right road. Thus, Bolzano defined mathematics as the science of the general laws that all possible things necessarily follow. And Hermann Weyl proposed a definition that is essentially the same. It therefore does not seem that the philosopher should be obliged, faced with the problem of the definition of mathematics, to the resignation that M. Cavaillès counsels.

Chabauty comes back to M. Cartan's remark that the dialectical themes envisaged by Mr Lautman are only encountered in certain parts of modern mathematics. One finds a few examples of them in the work of 'set theorists'. When one has recognized one of these themes in certain approaches of mathematics, it would perhaps be interesting to see what initial conditions, what axioms imposed upon the sets in question, have permitted the commonalities in the theories in question.

M. DUBREIL: I was particularly interested by what M. Cavaillès said about the efforts mathematicians have made to reflect upon their own science, and about one of the difficulties that they have met with in doing so: to study the non-contradiction of a system of axioms, one must bring to bear upon it mathematical theories of a higher level. For example, to establish the non-contradiction of arithmetic, we use transfinite induction.

I wonder whether this difficulty is not more apparent than real, and whether the power of the necessary means to establish the non-contradiction of a system of axioms does not rather bring to light the profound nature and the true import of these axioms. Take again the example of whole numbers: it is perhaps not excessive to say that, if one wishes to exhaust the *mathematical* content of this notion, one is led to attach it to that of a well-ordered set.

For let us direct our attention not to natural whole numbers considered individually, but to the set of these numbers. This set is ordered, and even well-ordered; and also, each element in the set has an antecedent. Since the notions of set, of order, of well-orderedness, and of antecedence are logically independent of that of a whole natural number, let us consider a priori well ordered sets in which each element admits of an antecedent: two possibilities present themselves, depending upon whether the set has a last element or not; we call it finite in the first case, denumerable in the second. Starting from these definitions, we see easily that any two denumerable sets whatsoever have the same power and that every finite set has the same power as a certain segment of a denumerable set. The set of natural whole numbers thus appears as a denumerable set chosen once and for all from, but indistinct from, the segments with which one compares finite sets. The notions of union and the product of sets flow immediately from this, with their properties, the operations on whole natural numbers.

We see that a small number of remarkable properties characterize finite sets and denumerable sets, in particular the set of whole natural numbers, in the more general class of well-ordered sets. We have also brought to light a fact which, if one reflects upon it, seems quite natural: like so many other sets considered in algebra, the set of whole numbers is only defined up to isomorphism.

M. CAVAILLÈS: I will respond, if you agree, in the inverse order of the interventions.

To Dubreil, I would respond very simply: Dubreil is not the only one to say that what Gödel discovered was predestined to be discovered. Yes: but when Gödel presented his paper, no one imagined that such a thing was possible. There was work going on, around Hilbert, von Neumann, who I have cited, there was work going on for years trying to demonstrate with finite means the non-contradiction of arithmetic, without appealing to transfinite induction. Von Neumann himself was very surprised by Gödel's result.

As to the priority between notions of whole numbers and well-ordered or denumerable sets, it is a mathematician's question, I cannot claim to resolve it myself; my humble opinion is that the notion of whole number is primary, and this, it seems to me, is confirmed equally by the work, for example, of

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—that is to say, an extension of the notion of whole number, through the placing into correspondence each time of an object with a system of already-numbered objects; in thus extending it, one arrives at the notion of transfinite numbering.

The extension of these metamathematical procedures is what makes possible—if we allow radically new procedures—yet vaster theories

This has only a vague relation to Gödel's result. The latter was a matter of demonstrating that it was possible, using finite arithmetic, the ordinary axiom of complete induction (and not general complete induction), to bring to light a certain property in the symbols: arithmetical non-contradiction. Gödel succeeded in demonstrating that it was impossible. It was a considerable result. About a month later, Gödel introduced a new considerable result: the possibility of demonstrating, using set-theoretical axioms—without the axiom of choice—the non-contradiction with these axioms of the axiom of choice and even the continuum hypothesis. If I cite a new example, it is just in order to show that the extension of these metamathematical procedures is what makes possible—if we allow radically new procedures—yet vaster theories.

As for M. Schrecker, I do not know if he is satisfied by his definition of mathematics, one must ask mathematicians what they think of it. If someone had never done mathematics, and we said to them, 'It is a deductive science', I don't think this would give them the idea of mathematics.

What I mean to say is as follows: What do we actually think when we speak of science, and of deductive science? There is only one way to think something deductively, and that is to do mathematics. Here, I am touching a little on the problem that I wanted to distance myself from, and you will tell me that the definition of a deductive science is a logical question. I do not want to enter into this debate, but if we wish to know what a deduction is, there is only one way to find out: to do mathematics; and the logical processes that are called deductive are a very elementary mathematical combinatory.

I would add that this is very important: I can invoke the testimony of Carnap, who was a partisan of the reduction of every mathematical notion to a logical notion; he nevertheless had to specify, in his *Logische Syntax der Sprache*, that he was now saying: the sense of a sign is its use. It is impossible to give a complete sense to the notion of deduction independently of mathematical argument. What is more, if you limit yourself, by deduction, to the calculus of propositions or predicates, you will not have the axiom of complete deduction, and it will mean nothing to say: 'Mathematics is a deductive science', since the axiom of complete induction, as Poincaré said, and as Hilbert said once again, is the very essence of mathematical life.

I'm afraid I completely disagree with what M. Fréchet said.

I do not seek to define mathematics, but, by way of mathematics, to know what it means to know, to think; this is basically, very modestly reprised, the question that Kant posed. Mathematical knowledge is central for understanding what knowledge is.

Fréchet says: 'There are notions that are taken from the real world, and others that are added by the mathematician'. I respond that I do not understand what he means, since what it is to know the real world, if not to do mathematics on the real world?

I am not an idealist, I believe in what is lived. To think a plane, do you live it? What do I think, when I say that I think this room?

What do you call 'real world'? I am not an idealist, I believe in what is lived. To think a plane, do you live it? What do I think, when I say that I think this room? Either I speak of lived impressions, rigorously untranslatable, rigorously unusable by way of a rule, or else I do the geometry of this room, and I do mathematics. What do you think when you think a plane? The geometric properties of that plane, the symmetry?

Our disagreement comes from the fact that I have not sufficiently expressed my thought, I am quite aware of to the shortcomings there.

I spoke of a solidarity on the basis of sensible gestures. There is not, on one hand, a sensible world that is given, and, on the other, the world of the mathematician, beyond it. The symmetry of a plane, for example, coincides with that permutational character that is one of the properties of experience in the sensible world.

M. FRÉCHET: This character is revealed to me by the sensible world.

M. CAVAILLÈS: Hilbert said that there is never mathematical thought without the use of signs, without sensible work with signs. I apologise for saying this, I suppose that mathematicians agree with me in saying that they experiment with the signs they have: there is, in a formula, a sort of appeal. 'Who can pass from the circle with its centre, from the cross of the coordinate axes? Arithmetical signs are written figures, geometrical figures of drawn formulae and it would be as impossible for a mathematician to do without them as it would be to ignore the parentheses in writing.'

I cite from memory the very beautiful article by Hilbert—before the war, the 'first' Hilbert. This article studies the unconscious experiments on possible relations, the possible usage of certain signs: I know the usage that I can make of them, there is a possibility of experimentation; we cannot define exhaustively the mathematical object independently of the implementation of the object in the sensible world.

I believe that we never leave this starting point, in the sense that there is a internal solidarity and that, each time we substitute for a less well-thought mathematical object some more thought-out objects, that is to say each time we separate what was simply accidentally united, by the process I have indicated, to that extent, all the same, we do not leave the sensible world.

But there is autonomy. Because (1) the questions posed by direct practice in its unified form (theoretical physics) only take on meaning and form in being transformed into mathematical questions—that is to say, in being inserted into the becoming of pure mathematics. (2) This insertion does not provoke a rupture: physics acts only as an kind of occasional developing fluid: in reality the problem was latent—internal difficulties, the need to surpass a too-summary system of notions—in the fabric of mathematical substance. Here again, I could invoke history: a detailed enough study would always show, for all the examples of the services rendered by physics to mathematics, that there is an internal necessity; that physics, in these matters, was but an occasion. I believe that it is essential, if one wishes to understand—and above all, it seems that there is complete disagreement here, but this has at least one advantage, which is that one can decide: of course, we shall not do so here—I believe that it is essential to see, in the notions used by the mathematician to resolve problems, the result of an exigency that is always there to be found in the foregoing system.

Physics acts only as an kind of occasional developing fluid: in reality the problem was latent in the fabric of mathematical substance

It is possible, if the mathematician is lazy, or for external reasons, that he might not resolve certain problems, that he might just live with the difficulties, but I do not believe that one can, for all that, deny the role of internal necessity.

It seems to me that M. Paul Lévy's objection to me was more or less the same.

M. PAUL LÉVY: I wanted to express the idea that there is something a priori existent, independently of the way in which it is discovered.

M. CAVAILLÈS: Here again, I expressed myself insufficiently: I do not at all say that these notions are independent of an historical order; I believe that they are necessitated by the problems.

When we have used whole numbers, it is obvious that we will posit the product as commutative; in other cases, we will employ noncommutative products.

The historical, contingent mathematician may stop, may be tired, but the exigency of the problem imposes the gesture that will resolve it

Consequently, when you say: 'Given a problem, there is a solution'—'Seek and you shall find', as Hilbert said; this is what I indicated as the projection of the system of mathematical gestures. The historical, contingent mathematician may stop, may be tired, but the exigency of the problem imposes the gesture that will resolve it.

This, if you like, is what I pointed to, calling it the reality of knowledge: that which, from the very point of view of an anthropology or a philosophy of the human constitution, is the extraordinary miracle of human destiny; independently of life in the lived world, it presents problems that necessitate solutions and lead outside of that which is, by way of a necessary sequence.

Here, I am not too far from Lautman, except for the word 'real' which I don't like; it would be a matter of distinguishing whether or not it is the sensible real, and here I do not agree with him, and perhaps also with M. Paul Lévy. That is to say that obviously this solution turns out to be necessitated by the problem that is posed: you say that it is somewhere, it is a question of taste.

M. PAUL LÉVY: The word 'somewhere' indicates that it is not localized.

M. FRÉCHET: Personally, I am entirely in agreement with M. Paul Lévy, I see this proposition as existing outside of us.

M. CAVAILLÈS: Lautman separates his position from mine; what I find very interesting, in what he does, are the links that he makes appear, very precisely, between certain theories. The future will prove him right here: personally, I am very much against positing some other thing that would govern the effective thought of the mathematician, I see an exigency in the problems themselves. It is perhaps this that he calls the dialectic that 'governs'. Otherwise I believe that with this dialectic, one only arrives at very general relations, or indeed at relations like those indicated by M. Cartan. There is doubtless some interest in inquiring into this; but to transform this into a philosophical position—this does not seem possible to me.

M. LAUTMAN: I would first of all like to thank M. Cartan for the goodwill with which he justified my logical interpretation of certain contemporary mathematical theories, among which some of the most beautiful were those he himself produced. I am equally appreciative of his admitting that notions such as those of global and local, of matter and form, are tied to no determinate theory, but can be found in very different theories, like analysis or geometry. In brief, if M. Cartan does not feel for himself the need of a reference to a dialectic, he recognizes that philosophers have the right to do so, and no encouragement could be more precious to them.

Mathematical theories seem to me to gain their full meaning when one interprets them as responses to a dialectical problem or question

I am far less in agreement with M. Fréchet. I spoke of a genesis of the real from the idea. M. Fréchet declares that he only understands the inverse: that is to say, the genesis of the idea from the real—by abstraction, obviously. It seems to me, in this regard, that one must distinguish between the historical order of human reflection and the logical or ontological order of the dependence of notions. Mathematical theories seem to me to gain their full meaning when one interprets them as responses to a dialectical problem or question. It is clear that it is only via an effort of regressive analysis that one gets back from the theory to the idea that it incarnates, but it is no less true that it is in the nature of a response to be a response to a logically anterior question, even if the consciousness of the question is posterior to the understanding of the response. The genesis of which I spoke is thus transcendental and not empirical, to take up Kant's vocabulary.

As far as M. Ehresmann's objections are concerned, I really feel I am in agreement with him, even if he does not want to recognize it. M. Ehresmann says to me that the problems I call dialectical remain vague in so far as I do state them in detailed form, and that as soon as I did so they would become purely mathematical problems. I have myself written that dialectic, not being affirmative of any effective situation and being purely problematic, is necessarily extended into effective mathematical theories. It is all about knowing whether it is possible to conceive of a logical or metaphysical problem being stated independently of any concrete mathematical solution. The response to this question is found in the history of philosophy. I will take just two examples. One is that of the Leibnizian monad. Is it possible to conceive of all of the relations a being entertains with the whole universe as inscribed within the internal properties of that being? This conception of the monad is purely metaphysical, and I have shown, I believe, in my thesis, the links that unite it to current theories of *analysis situs*—which are also, moreover, of a Leibnizian inspiration. As a second example, I take the case I cited earlier: the problem of the reciprocity of action between two or more bodies, a problem assuredly distinct from the Newtonian theory, and to which Kant believed, nevertheless, that he found the definitive solution in the famous law of universal attraction. The history of philosophy thus demonstrates the autonomy of the conception of problems of structure in relation to the contingent elaboration of particular mathematical solutions.

Chabauty leads me to remark that I have attached a great importance to theorems that establish the existence of certain functions on certain surfaces or certain sets, but that this result may appear less surprising if one takes account of the fact that the sets in question have been 'rigged' in such a way that it should be pretty much impossible to find the functions we seek on them. It thus would seem that one only finds in a set what one has already put there. Presenting things in this way, it seems to me, does not place sufficient emphasis on the fact that there can exist two sorts of 'rigging', in M. Chabauty's sense, those which are fruitful and those which are not. A set only ever possesses, as far as properties are concerned, those one gives it a priori with axioms; but it turns out that certain of these artificial definitions have as a consequence to bring a set or a surface to such a state of completion or perfection that this internal perfection blossoms into the affirmation of existence of new functions defined upon this set. This fruitfulness of certain structural properties, which is extended into the genesis of new mathematical beings, seems precisely to me to distinguish, within the possibilities of axiomatic definition, creative conceptions from those which lead to nothing really new.

Hyppolite reproaches me for using the term 'dialectic' in three different senses at least. There is one that I do not accept. It is the definition according to which there could exist a dialectic of local and global that would be self-sufficient, independently of mathematics; on the contrary, the two others seem to me to complement each other, not destroy each other. Mathematics constitutes a true dialectic of local and global, of rest and movement, in the sense that dialectics studies the manner in which the abstract notions in question can be composited with each other; this does not prevent us from conceiving of a dialectic anterior to mathematics, conceived as problematic. M. Hyppolite says that posing a problem is not conceiving anything; I respond, after Heidegger, that it is to already delimit the field of the existent.

Schrecker mostly addressed his comments to M. Cavaillès, but I believe that we agree in admitting the legitimacy of a theory of abstract structures, independent of the objects that are linked with each other by these structures.

Mathematics do indeed belong to the domain of action, but the dialectic is above all a universe to be contemplated

It only remains for me to respond to M. Cavaillès. The precise point of our disagreement bears not on the nature of mathematical experience, but on its meaning and its import. That this experience should be the condition sine qua non of mathematical thought, this is certain; but I think we must find in experience something else and something more than experience; we must grasp, beyond the temporal circumstances of discovery, the ideal reality that alone is capable of giving its sense and its status to mathematical experience. I conceive this ideal reality as independent of the activity of the mind, which only intervenes, in my view, when it is a matter of creating effective mathematics; mathematics do indeed belong to the domain of action, but the dialectic is above all a universe to be contemplated, the admirable spectacle of which justifies and recompenses the lengthy efforts of the mind.