Annals of Mathematics and Philosophy

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La philosophie mathématique

Mathematical and philosophical inspirations
from Brunschvicg to Granger. Part 1

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Special issue 1/2

Editors: Gabriella Crocco & Frédéric Jaëck

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Introduction

This special issue[1] in two volumes is devoted to a certain French tradition in the philosophy of mathematics, a tradition characterized by the tutelary presence of two major figures, Jean Cavaillès (1903-1944) and Albert Lautman (1908-1944), both of whom were shot by the Nazi occupiers for their involvement in the French resistance, and both of whom produced original, albeit unfinished, works in the course of their short lives. The close dialogue between these two authors throughout their short lives delineated a theoretical space where points of agreement also gave way to fundamental disagreement. Among the main points of agreement was the idea of the need to focus on an effective understanding of the mathematical practice of their time[2] (experience, as they both call it), in particular by entering into a concrete dialogue with leading mathematicians. Secondly, the thesis that the true nature of mathematics is to be a "becoming". This implies that there can be no real understanding of mathematics if we neglect its history. Finally, the idea that the central philosophical question that guides philosophers in this task should be the problem of the meaning of this becoming, of what it tells us about reality and about our reason. So, it is not simply a question of a philosophy of mathematics, in the same way as there is a philosophy of physics or biology, each of which would be regional epistemologies, but rather of a Mathematical Philosophy (*Philosophie mathématique*), that is to say a reflection on the lessons that philosophy of knowledge must draw from mathematical experience. The disagreements, on which we will focus below, concern the philosophical meaning that should be attributed to this development : philosophical meaning that touches directly on the problem of

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[1] The authors would like to warmly thank Jean-Pierre Marquis for his suggestions, which helped to clarify this introduction.

[2] The characterization of the effectiveness of this knowledge that Cavaillès gave in his work constituted a kind of standard of requirement for the French school in the philosophy of mathematics, and more generally in the philosophy of science; the philosopher must not be satisfied with abbreviated or simplified accounts of mathematical work: he must be capable of "catching the gesture" (Cavaillès 1981, p. 178), that is to say of following the genesis, the explanation and the justification that mathematics gives in its own singular and concrete becoming.

the relationship between mathematics, history and philosophy. This theoretical area of debate, what inspired it and what it inspired in the next generation, is the subject of this double issue.

This first issue opens with a full English translation of the meeting of the *Société française de philosophie* on 4 February 1939, devoted to the presentation of the theses of Cavaillès[3] and Lautman[4] respectively, followed by a debate attended by several mathematicians, including Elie Cartan, Charles Ehresmann, Paul Lévy, Maurice Fréchet and others. Then, in the two volumes, we present a series of articles which, without any claim to exhaustiveness, discuss certain aspects of the philosophical work of Cavaillès and Lautman, as well as the conceptions of several authors around them. Léon Brunschvicg (1869-1944), Cavaillès and Lautman's thesis supervisor. Jules Vuillemin (1920-2001), Gilles Gaston Granger (1920-2016) and Jean Toussaint Desanti (1914-2002), who attended classes with Cavaillès at the École normale supérieure and listened to Lautman there. As a counterpoint, we add an article on Maximilien Winter (1871-1935), a contributor like Brunschvicg to the *Revue de métaphysique et de morale*, but whose interest in the history and practice of mathematics is tinged with a greater presence of positivist themes.

The term "philosophy of the concept" has long been used in France to designate, following Cavaillès, part of the tradition of French philosophy of mathematics which proposes a third way between analytic philosophy and Husserlian phenomenology. The origin of this term can be found in the very last lines of the posthumous *Sur la logique et la théorie de la science* (edited by G. Canguilhem and Ch. Ehresmann in 1946), in which Cavaillès states that "It is not a philosophy of consciousness but a philosophy of the concept that can give a doctrine of science. The generating necessity is not that of an activity but of a dialectic". In 1946, Canguilhem emphasized the enigmatic nature of these last words of Cavaillès's intellectual testament, but in 1976, at a time when structuralist currents were in full swing in France, he added: "We can understand today that the enigma was tantamount to a foreshadowing; Cavaillès assigned,

[3] Cavaillès defended his theses at the Sorbonne (Méthode axiomatique et formalisme (main thesis) and Remarques sur la formation de la théorie abstraite des ensembles) in January 1938, under the supervision of Léon Brunschvicg.

[4] Lautman defended his theses in December 1937 at the Sorbonne, also under the direction of Léon Brunschvicg. These were Essai sur les notions de structure et d'existence en mathématiques (main thesis) and Essai sur l'unité des sciences mathématiques dans leur développement (essay on the unity of the mathematical sciences in their development).

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twenty years in advance, the task that philosophy is in the process of recognizing for itself today: to replace the primacy of lived or reflected consciousness with the primacy of the concept, the system or the structure" [Canguilhem 1976, p. 32].[(5)]

However, the interpretation of this passage is far from unanimous and many works devoted to Cavaillès have discussed, and are still discussing, these last lines. In fact, this interpretation calls for a diagnosis of the posterity of Cavaillès's work and therefore (a rather thorny question) of the perimeter of those who can be considered to have prolonged his unfinished work.[(6)] It is significant that recently some (see, for example, Alain Badiou, *L'aventure de la philosophie française*) have gone so far as to speak of a Brunschvicg school (which would include, according to Badiou, Lévi-Strauss and Althusser) that would confront the Bergsonian school in such a way that their opposition would have marked French philosophy as a whole from the beginning of the twentieth century, precisely as an opposition between the philosophy of consciousness and the philosophy of the concept.[(7)]

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[(5)] This same term was later used by Michel Foucault in his introduction to the American edition of Canguilhem's book *Le normal et le pathologique*, to contrast the French philosophers of science (the philosophers of the concept), almost all of whom were involved in the Resistance, with the philosophers of consciousness who, like Sartre and his existentialist friends, were more concerned with their own work during the Occupation.

[(6)] See the analyses by Houria Benis Sinaceur (in: *Jean Cavaillès : philosophie mathématique*, Presses universitaires de France, 1994 and *Cavaillès*, Les belles lettres, 2013) and Elisabeth Schwartz (in particular "Jean Cavaillès et la philosophie du concept", *Philosophia Scientiæ*, no. 3, 1998, pp. 79-97 and the two issues she edited, "Jean Cavaillès 1", *Revue de métaphysique et de morale*, no. 105, 2020, and "Jean Cavaillès 2", *Revue de métaphysique et de morale*, no. 106, 2020).

[(7)] "In 1911, Bergson gave two very famous lectures in Oxford, later published in the collection La Pensée et le mouvant. In 1912, Brunschvicg's book Les Étapes de la philosophie mathématique was published. These two interventions (just before the 14-18 war, which is not insignificant) set thought in directions that were, on the surface at least, completely opposite. Bergson proposed a philosophy of vital interiority, subsumed by the ontological thesis of the identity of being and change based on modern biology. This orientation was to be followed throughout the century, up to and including Deleuze. Brunschvicg proposed a philosophy of the concept, or more precisely of conceptual intuition (a fruitful oxymoron since Descartes), based on mathematics, and describing the historical constitution of symbolisms in which fundamental conceptual intuitions are in some way collected. This orientation too, which ties subjective intuition to symbolic formalisms, continued throughout the twentieth century." A. Badiou, *L'aventure de la philosophie française*, La Fabrique editions p. 9. In the wake of Badiou, see also Tryggvi Örn Úlfsson, *D'une épistémologie mathématique vers une ontologie phénoménologique : Séquences de la philosophie du concept du 20e siècle* ; Thèse de doctorat de l'Université

We will not enter into this debate, wishing to remain focused in this presentation on this current of French epistemology which has made specific reflection on mathematics the central point of its own philosophical approach. We do not claim to trace the contours of a school, but, as we have said, to indicate the perimeter of a debate, even a contradictory one, which seems to us to be fruitful, which was structured by Cavaillès and Lautman and which has been extended, actually or virtually, by those who have been inspired by their philosophy (Granger, Vuillemin and Desanti in particular).[8]

Hence the dual purpose of these two volumes.

The first aim, is to raise awareness of this movement which, despite the recent revival of interest in French epistemology and philosophy, is still little known outside a restricted field of specialists. It is true that a series of works in English have recently appeared, filling a glaring gap[9], but it has to be said that the American analytic tradition continues largely to ignore the names of Cavaillès and Lautman and the importance that their teaching and of their work had on the whole generation of French philosophers of mathematics in the second half of the twentieth century and until now. In this respect, the *Stanford Encyclopedia of philosophy* is symptomatic. There are entries on Bergson, Poincaré, Duhem, Althusser and Foucault, but not on Cavaillès or Lautman, and no mention of Vuillemin, or Desanti,[10] nor any specific entry

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Paris VIII, Mars 2024. Úlfsson does not mention Granger and Vuillemin in this tradition, but does include Bachelard and Badiou.

[8] In fact, all the implications of their debate on the relationship between history, philosophy and mathematics (for example, the question of objectivity, necessity, applicability and the unity of mathematics in its historical development) were explored by these students...

[9] We are thinking here of A. Brenner, J. Gayon *French studies in philosophy of science*, (2009) Contemporary Research in France, Springer, Vienna/New York, A. Brenner, Epistemology Historicized: The French New Directions in the Philosophy of Science, (2014). In: Galavotti, M., Dieks, D., Gonzalez, W., Hartmann, S., Uebel, T., Weber, M. (eds) The Philosophy of Science in a European Perspective, vol 5. Springer, as well as monographs such as Pietro Terzi *Rediscovering Léon Brunschvicg' critical idealism* (2022), Tiles, Mary, *Bachelard: Science and Objectivity*, Cambridge University Press, (1984) as well as reprints and translations of Lautman's texts (*Mathematics, Ideas and the Physical Real*, 2011 - Bloomsbury Publishing, the new English translation of Cavaillès's *Sur La Logique et la théorie de la science* with preface by Knox Peden, in 2021. (Cavailles, Jean. On logic and the theory of science, (2021). New York, NY: Sequence Press. Edited by Knox Peden & Robin Mackay)

[10] There is, however, an occasional mention of Granger in the SEP entry on *Style*.

on French historical epistemology (although there are some on China, India and Latin America). This is despite the fact that Paul Bernays published a review of Lautman's thesis *Essai sur Les Notions de Structure et d'Existence en Mathématique* in the *Journal of Symbolic Logic* in 1940[(11)] and that between 1938 and 1940 there were seven reviews of Cavaillès's work in the same journal by A. Church, E. Beth, L. Henkin, E. Nagel and M. Black.

Perhaps a first reason for such a blind spot in the American tradition of philosophy of mathematics lies in the disconnection between it and French epistemology between the 50s and the 70s. At that time, the fundamental tool in the American analytic tradition was formal logic, a tool that was fairly secondary in France during the same period in the field of epistemology. It is a fact that after the Second World War, the focus in America shifted to Gödel's incompleteness theorems, the development of Tarski and Robinson's model theory and its impact on philosophy (see for example Putnam), the development of intuitionistic logic and multivalent logics (very popular in the 1950s and 1960s), the rediscovering of Gentzen's work on natural deduction (via Beth's and Hintikka's tables in the 50s and 60s), the semantics of modal logics, Cohen's forcing in set theory, the introduction of non-standard analysis by Robinson and, finally, higher-order logics. The works of Stephen Kleene, Alonzo Church and Georg Kreisel, with all their profound philosophical implications, have been widely discussed and recognized. In short, analytical philosophy required formal logic, which was quite absent in France at the time, nevertheless with some remarkable exceptions, related to Cavaillès and Lautman's legacy.[(12)] It's a fact that since the 1990s, formal logic has had less impact on philosophy in general, and philosophy of mathematics is opening to other methodological tools.

A second reason for this blind spot in American philosophy of mathematics of analytic orientation lies in the difficulty for it of perceiving the specificity and originality of the French tradition in philosophy of mathematics. The progress made in the historical reconstruction of the debate on the philosophy of science in

[(11)] *The Journal of Symbolic Logic*, (1940), Vol. 5, No. 1, pp. 20-22. Paul Bernays also mentioned Lautman's work to Kurt Gödel in a letter dated 17 September 1965 (S. Feferman et alii *Kurt Gödel Collected Works vol. IV*, p. 242-243.

[(12)] The works of Vuillemin, Granger and their scholars as well as of some of the members of the *Institut d'histoire et de philosophie des sciences et des techniques* in Paris (most of them scholars of Roger Martin) bear witness on that.

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the twentieth century, in this great, lively and plural cultural area that was Europe before the Second World War, is certainly undeniable. However, it should be kept in mind that what characterizes this debate is not so much the plurality of languages in which it is expressed, but rather the subtle interplay of borrowings and refusals to borrow (*emprunts et refus d'emprunts*),[13] constitutive of each of the currents that have been expressed. This interplay of borrowings cannot be reduced to national characteristics, and even less to uniform philosophical choices, but is the result of a conscious and argued search for a solution to the problems posed by science. The tradition we are interested in has distanced itself in many ways from its contemporaries. Distancing itself from Poincaré[14] , from the ideas of Abel Rey, Meyerson and the philosophers who founded the Institut d'Histoire des Sciences et des Techniques in Paris, from Bergson, and also from the Husserlian phenomenology, German neo-Kantianism and the Vienna Circle, in particular Carnap, Schlick and Reichenbach. Moreover, this interplay of borrowings and refusals to borrow is nourished by dialogue with the history of philosophy, and in particular the history of French philosophy, characterized by the Cartesian heritage, Comtian positivism, the philosophy of Kant which had penetrated France after post-Kantianism, and contacts with American pragmatism (James and Dewey in particular). We are a long way from having carried out a dispassionate historical analysis of what gave rise to the debate on the philosophy of science and, in particular, contemporary mathematics. Knowledge of this underestimated tradition seems to us to be an important element in developing this history.

The second aim of this double issue is theoretical and methodological, and concerns the fact that knowledge of this tradition has, in our view, an intrinsic value for the current debate in the philosophy of mathematics, marked by the advent of the philosophy of

[13] For the notion of "refusal to borrow" as a factor in the formation of identities, see Marcel Mauss (1920), "*La nation*" : « les sociétés sont en quelque sorte plongées dans un bain de civilisation ; elles vivent d'emprunts ; elles se définissent plutôt par le refus d'emprunt, que par la possibilité d'emprunts. » Extract from Année sociologique, Troisième série, 1953-1954, p37. Text reproduced in Marcel Mauss, *Œuvres. 3. Cohésion sociale et division de la sociologie* (pp. 573-625). Paris: Les Éditions de Minuit, 1969.

[14] Of course, Poincaré's work remains at the heart of the mathematical and epistemological thinking of the Cavaillès-Lautman tradition. However, the absence of any reference to intuition, to a doctrine of faculties or to non-historical a priori forms marks a clear philosophical distance.

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mathematical practice movement. It is undeniable that this current is part of a critical study of the "orthodox" epistemological approach typical of the analytic tradition, in which logic, the notion of first-order formal theory, the foundationalist point of view and ontological questions play a prominent role. This new strand of contemporary analytic philosophy thus claims a wider field of reflection than number theory and/or set theory, an attention to the "non-logical" methods intrinsic to actual mathematical practice and to the processes of explanation and justification present in various mathematical fields throughout history, and the need to move away significantly from properly ontological issues and the "quarrels" linked to logicist, intuitionist and formalist positions.

When we speak of an "orthodox" tradition in the philosophy of mathematics of analytic obedience, we mean a tradition that has essentially developed from Quine's philosophy, from his own way of inventing the analytic tradition with clearly identified founding ancestors (essentially Frege, Russell and Carnap), and from the "system"[15] that Quine constructed to "renew" Carnapian logical empiricism in a "non-dogmatic" sense. This system clearly includes two principles. The first is the reduction, in the analysis of scientific discourse, of all ontological considerations to the objects of formalized theories. This is the famous principle according to which "to be is to be the value of a variable", which eliminates any entity of an intensional or intentional nature from the universe of discourse of philosophy of mathematics, and has as its consequence extensionalism, i.e., the elimination (in the analysis of proofs based on closed and given a priori sets of axioms and rules) of any consideration other than the establishment of truth. Secondly, the principle of indispensability, which states that the objects that we must recognize as belonging to our ontology of reality are the objects that are indispensable to our best scientific theories, the objects belonging to the domains on which their corresponding formalized theories quantify. To these two principles Quine first adds the doctrine of holism (the idea that the network of our knowledge faces experience as a whole), which deprives the assertion of the reality of the objects on which our theories quantify of any metaphysical value. Holism also implies the idea that radical epistemological ruptures or changes, since they do not affect the

[15] We are talking here about a philosophical system in the classical sense of the term: a set of principles and methods from which coherent doctrines are derived.

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empirical base which only increases, are merely ontological revolutions and therefore a kind of palace revolution. Knowledge as a whole develops continuously and progressively.[16] Secondly, its naturalism, which has two distinct components: the rejection of all metaphysics ("first philosophy") and the denial of any radical difference between common sense knowledge and science on the one hand, and between philosophy and science on the other.[17] These two aspects were transformed in the Quinian vulgate into the doctrine according to which naturalism asserts that philosophy must be constructed in dialogue with science, which implies that not being a naturalist means ignoring or despising science.[18] This transformation of the principle of Quinian naturalism is obviously false, as shown by twenty-five centuries of philosophy and by the doctrines of many philosophers who have nevertheless attracted the interest of analytic philosophy. Among them, Wittgenstein, for example, who describes what an anti-naturalist position is in the simple aphorism 4.111 of the *Tractatus*:

"Philosophy is not one of the natural sciences. (The word "philosophy" must mean something whose place is above or below the natural sciences, not beside them)."

This expresses the dual place that philosophy has in the *Tractatus*: philosophical work must unfold both as a critique of science (in the broad sense of the term) and therefore below it, and as a radical interpretation (or vision) of the meaning of this human practice, above it.

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[16] It is remarkable that the realism/anti-realism debate should have been built around the so-called Quinian Platonism, because Quine, as he himself says, attributes only a theoretical and not a metaphysical value to his criterion of existence: "Reference and ontology thus recede to the status of mere auxiliaries. True sentences, observational and theoretical, are the alpha and the omega of scientific enterprise. They are related by structure, and objects figure as mere nodes of the structure. What particular objects there may be is indifferent to the truth of observation sentences, indifferent to the support they lend to theoretical sentences, indifferent to the success of the theory in its predictions" W. V. Quine, Porsuite of truth, Harvard University Press 1990, p. 31.

[17] "Philosophically I am bound to Dewey by the naturalism that dominated his last three decades. With Dewey I hold that knowledge, mind, and meaning are part of the same world that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy." W.O. Quine, Ontological relativity, The journal of philosophy, vol LXV, no 7, April 1968, 185.

[18] See David Papinau *Naturalism*'s SEP entry on this subject.

Of course, this orthodoxy began to be challenged in the second half of the 1960s, with the work of Benacerraf and Putnam, but it is symptomatic that each of these challenges, and the currents that developed in their wake, concerned specific aspects of the Quinian system, without ever raising the more general question of a reconsideration of its thinking as a whole and an overall critical reflection on its project.

First of all, structuralism, which developed within the analytic tradition from the famous "What numbers could not be", calls into question all the constraints that Quine had placed on the objects of analysis in the philosophy of mathematics with his foundationalist reductionism to the objects of formalized theories (Quine's first principle mentioned above). However, structuralism (whether eliminative or non-eliminative) does not seem to break with naturalism at all, since the properly historical and philosophical questioning of the emergence, nature and meaning of these structures seems secondary in this tradition.

Secondly, Penelope Maddy's work on the choice of axioms in set theory has called into question one of the consequences of Quine's holism (the non-autonomy of mathematics with respect to physics) and certain aspects of the indispensability argument. She thus opens the way for consideration of the arguments which, within the mathematical community, lead to the rejection or acceptance of certain axioms and certain entities rather than others. However, Maddy argues, in full accordance with Quinian naturalism, that mathematics does not allow for deeper forms of justification than are operational in current mathematics. This way of posing the problem certainly opens up far wider investigations into mathematical practice and methodology than the Quinian horizon, but they remain circumscribed, in Maddy's work, to the realm of set theory and Quinian naturalism.

A final challenge to Quinian dogmas comes from the opening up of epistemology to the history of mathematics, a consequence of the reception of Lakatos' doctrine by analytic philosophy. The notion of "mathematical practice" can receive a clear definition in this tradition, being linked Lakatos's idea of the development of competing research programmes. Consequently, attention to the historical development of knowledge opens the door to considerations related to heuristics, to non-formal or non-logical processes of explanation and argumentation outside set theory, although the empiricist bias of the Lakatos tradition tends to restrict mathematical practice to a *problem-solving* approach.

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All these elements have certainly opened up new avenues of analysis that the new wave of the philosophy of mathematical practice is currently developing.

However, as Paolo Mancosu makes clear in his book on the subject, this new wave does not wish to call into question the programme of analytic philosophy, but only to broaden (as far as possible)[19] the field invested by the philosophical analysis of mathematics, without undertaking a clarification of what is meant by mathematical practice.[20] Actually, in full observance of Quinien naturalism, which denies any specificity to philosophical enquiry, this is done by opening up within it to valuable considerations which belong rather to the fields of psychology, sociology or the history of academic institutions. The problem is that without clarifying from the outset the question of what is meant by mathematical practice, and how it fits into the dogma of naturalism, we risk to leave out whole areas of mathematical experience, which concern creation, heuristics, the meaning of the problems posed and the solutions envisaged, leaving it to the cognitive sciences alone to inform us about what these processes involve.

Perhaps this radical questioning and opening up to other perspectives of analysis, which analytic philosophy seems to want to accomplish, could be set in motion by a more attentive analysis of this French tradition in the philosophy of mathematics.

The latter is profoundly anti-naturalist, believing in epistemological ruptures, and at the same time it is fundamentally attentive to present and past mathematical experience. It is profoundly philosophical and even metaphysical, and at the same time aware that nothing can be said about mathematics without a careful analysis of its present, its history, the way in which the singular experience of the constitution of mathematical objects and theories develops over time.

[19] See Brendan Larvor *Review of Paolo Mancosu' the philosophy of mathematical practice*, in *Philosophia Mathematica,* Volume 18, Issue 3, October 2010, Pages 350-360.

[20] This empiricist bias is certainly the opposite of the neo-Kantian conceptions of the tradition we are presenting here. Already in the *Critique of Pure Reason*, Kant expressed himself clearly against the empiricist myth that claims to construct theories by gleaning disparate observations without any prior plan: "They [those who founded modern science] comprehended that reason has insight only into what it itself produces according to its own design; that it must take the lead with principles for its judgments according to constant laws and compel nature to answer its questions, rather than letting nature guide its movements by keeping reason, as it were, in leading-strings; for otherwise accidental observations, made according to no previously designed plan, can never connect up into a necessary law, which is yet what reason seeks and requires." B XIII, translated and edited by P. Guyer, A. Wood, Cambridge University Press.

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The distinctive feature of the French tradition in the philosophy of mathematics that we are presenting in this issue is that it places the notion of structure at the heart of its thinking, while at the same time addressing the question of the constitution of mathematical objects, and taking an interest in the concrete practice of mathematics (mathematical experience), without however rejecting out of hand the question of foundations, because as Jean Cavaillès so aptly put it: "I am not trying to define mathematics, but by means of mathematics to understand what it means to know and to think".

It is also profoundly philosophical in the sense that it does not shy away from engaging mathematics with philosophy and its history, without a priori judgements or preconceived ideological exclusions, because what is at stake, as Lautman says, is the questioning of the meaning and scope of mathematical experience. And despite all this, it has not given up on dialogue with mathematicians. This obviously complex dialogue between these philosophers and the mathematicians of their time, this dialogue that they construct over and above all the radical differences in language, methods and even interests, is felt to be a vital necessity for philosophy and is at the foundation of what has often been called the mathematism specific to this tradition.

This shows that attention to mathematical practice, far from excluding the question of foundations, actually enriches it.

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MATHEMATICAL THINKING

Jean Cavaillès & Albert Lautman

Introduction and translation[1]
by
Gabriella Crocco and Frédéric Jaëck

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

For Cavaillès, irreducibility appears on several levels:

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

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

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

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

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

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as example of incarnations, domains in which the ideal expectation of possible relations is actualized".

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

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

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

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

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

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

The second point of disagreement concerns the question of existence.

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

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

The text that follows is divided into three parts.

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

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

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

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

This text undoubtedly reveals a philosophical debate that marks a new direction compared to the dominant approaches that have spread worldwide. It is time to fully grasp this shift. Contemporary philosophy ought now to take advantage of this promising path and engage with it vigorously.

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

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MATHEMATICAL THINKING

February 4, 1939

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

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

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

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

Mr. Cavaillès is then led to the following statements:

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

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

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

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

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

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

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

The Ideas of such a Dialectics must be conceived as Ideas of possible relations between abstract notions, and their knowledge

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

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

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MEETING MINUTES

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

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

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

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

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

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

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

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

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

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

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work. For those who knew him, both philosophers and mathematicians, his absence is still sorely felt today.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Mathematics is a becoming. All we can do is try to understand its history, that is, to situate Mathematics among other intellectual

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activities, to find certain characteristics of this becoming. I would like to mention two of them:

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

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

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

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

By experience, I mean a system of gestures, governed by a rule and subject to conditions independent of these gestures. I recognize the vagueness of such a definition, but I don't think it's possible to completely overcome it without taking actual examples. By this, I mean that each mathematical procedure is defined in relation to a previous mathematical situation, on which it partly depends, and

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in relation to which it also maintains such independence that the result of such a gesture must be observed in its accomplishment. This, I believe, is how one can define mathematical experience.

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

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

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

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

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

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

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

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

I don't want to elaborate on this paradox, especially as it would require a formalization to explain it precisely. Roughly speaking,

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it means this: if we have a model, which we assume satisfies a system of axioms, it is always possible to construct a countable model satisfying this same system of axioms. In particular, we can satisfy the axiom system of Set Theory with a countable model.

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

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

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

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

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

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independently of a necessary sequence from the very beginning of human activity.

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

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

COMMUNICATION OF MR. LAUTMAN

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

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

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

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

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

If we reflect on the internal mechanism of the theory we've just alluded to, we realize that it establishes a link between the degree of completion of the internal structure of a certain mathematical being (a surface) and the existence of another mathematical being (a function), i.e., in short, between the *essence* of one being and the *existence* of another being. These notions of *essence* and *existence*, like those of *form* and *matter*, *whole* and *part*, *container* and *contents*, etc., are not mathematical notions, yet it is to them that the consideration of actual mathematical theories leads. I call them dialectical *Notions*, and propose to call the problem of the possible connection

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between dialectical notions thus defined dialectical *Ideas*. The reason for the relationship between Dialectics and Mathematics lies in the fact that problems of Dialectics can be conceived and formulated independently of Mathematics, but that any tentative solution to these problems must necessarily be based on some mathematical example that concretely supports the dialectical connection under study.

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

I mentioned earlier that the distinction between an ideal Dialectics and an effective Mathematics must be interpreted above all from the point of view of the genesis of Mathematics from Dialectics. Here's what I mean by this: Dialectics, in itself, is pure problematization [*problématique pure*], a fundamental opposition [*antithétique fondamentale*] concerning pairs of notions that appear, at first glance, to be opposed, and in relation to which the problem of a possible synthesis or conciliation nevertheless arises. In my thesis, for example, I considered the problem of the relationship between the local and the global, the extrinsic and the intrinsic, the continuous and the discontinuous, and so on. As in Plato's *Sophist*, it turns out that opposites are not opposites, but that they can be combined to form the mixtures [*mixtes*] that are Mathematics. Hence the need for these intricate subtleties, this unpredictable peculiarity [*ce pittoresque imprévisible*], these

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obstacles that are sometimes overcome and sometimes circumvented, this whole historical and contingent becoming that constitutes the life of Mathematics, and which nevertheless presents itself to the metaphysician as the necessary extension of an initial Dialectics. We pass imperceptibly from the understanding of a dialectical problem to the genesis of a universe of mathematical notions. It is to the recognition of this moment, when the Idea gives birth to the real, that Mathematical Philosophy must, in my opinion, aim. In a booklet published by *Librairie Hermann* since my thesis, I have tried to show the analogy between these conceptions and those of Heidegger. The extension of Dialectics into Mathematics corresponds, it seems to me, to what Heidegger calls the genesis of ontic reality from the ontological analysis of the idea. This introduces, at the level of the ideas, an order of before and after that is not time, but rather an eternal model of time, the schema of a genesis constantly in the making, the necessary order of creation.

It seems to me that the problem of the relationship between the theory of Ideas and Physics could be studied in the same way. Consider, for example, the problem of the *coexistence* of two or more bodies. This is a purely philosophical problem, which we'd say Kant posed rather than solved in the third category of relation. Nevertheless, as soon as the mind tries to think what the coexistence of several bodies in space might be, it necessarily gets involved in the as yet unsolved difficulties of the n-body problem. Consider again the problem of the relationship between *motion* and *rest*. We can abstractly pose the problem of whether the notion of motion only makes sense in relation to absolute rest, or whether, on the contrary, there is rest only in relation to certain changes; but any effort to resolve such difficulties gives rise to the subtleties of the Theory of Special Relativity. The question also arises as to which of the two notions of motion and rest should be given a physical meaning, and this is a point on which classical Mechanics and wave Mechanics clash. The former considers the wave as a physical movement. For the latter, on the contrary, the wave equation appears to be no more than an artifice designed to highlight the physical invariance of certain expressions with respect to certain transformations. It thus appears that the theories of Hamilton, Einstein and Louis de Broglie take on their full meaning with reference to the notions of motion and rest, of which they seem to represent the

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true dialectics[11]. It may even be that what physicists call a crisis in contemporary Physics, grappling with the difficulties of the relationship between the continuous and the discontinuous, is a crisis only in relation to a certain rather sterile conception of the life of the mind, where the rational is identified with unity. On the contrary, it seems more fruitful to ask whether the purpose of reason in the sciences is not rather to see in the complexity of the real, in Mathematics as in Physics, a mixture whose nature can only be explained by going back to the Ideas of which this real partakes.

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

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

DISCUSSION

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

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

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

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

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

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

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is what happens. If it can be extended to form a complete space, it can be extended in only one way, with certain restrictions.

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

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

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

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

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

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

Certainly, the development of Mathematics is in itself somewhat unpredictable, and when you reach a certain age, you realize that certain theories, after twenty, thirty or forty years, take on a completely unexpected development, and that the point of view from

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

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

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

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

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

This leads me to believe that, once a certain mathematical theory has begun, a superior mind can foresee in what direction it will develop. Let me take, as a concrete example, one of the mathematical theories whose philosophical aspect has attracted the most attention: the Theory of the Integral, as constructed by modern Set Theory. It was Mr. Lebesgue who gave the notion of the integral its definitive form, and today you all know that the integral is an essential tool in Mathematics. It is so essential that, without a

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doubt, if Mr. Lebesgue hadn't existed, his integral would still have been discovered today, long ago. I don't think I'm diminishing Mr. Lebesgue's merit; on the contrary, I think I'm only enhancing it, by saying that he brought to light a notion that was necessary for the further progress of science. Would Émile Borel, who was already working along these lines, have developed this theory? Would another of his students have been given the chance? I don't know. But, after the work of Jordan and Mr. Borel, given the current level reached by humanity as a whole and the number of researchers specializing in the field of Mathematics, I believe it was necessary and inevitable that, within ten or fifteen years, the theory of the Lebesgue Integral should be put on the bridge. And, in this vein, I believe, to a certain extent, that the development of Mathematics is predictable.

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

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

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

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

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

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

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

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

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

First of all, there are two related questions, at least in my mind, to which I might be able to provide an answer: Mr. Cavaillès indicated that, in his opinion, Mathematics is an autonomous science. Personally, I don't think so. It all depends, of course, on what we call "Mathematics". Many people call "Mathematics" the set of deductive theories that enable us to go from a set of properties and axioms to certain theorems. This is undoubtedly the most

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specific part of Mathematics, but it seems that, if we were to stop there, not only would Mathematics be reduced to a machine for transformations, and in that case its role would still be very useful, but it would be limited to transforming, so to speak, emptiness into emptiness. I believe that, in order to justify the existence of Mathematics, it is essential to show that it is an instrument invented to help mankind understand nature and predict the course of phenomena. The notions that seem to me to be the most fundamental in Mathematics are all notions that do not, in my opinion, originate from our intelligence, from our mind, but are imposed on us by the external world.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

And in a third sense — and this is where the ambiguity seems to me to be strongest — Mr. Lautman uses the word "dialectics" in the sense in which philosophers have most often taken it. It's a dialectics of "form and matter, local and global", and so on. It seems to me, however, that if we were to use the word "dialectics"

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at all costs in the philosophy of Mathematics, we would have to use it only in the first sense, i.e. in the sense of a life of mathematical experience in the course of its history.

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

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

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

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

As for Mr. Lautman's thesis, we might fear that, if we adopted it, mathematical notions would somehow evaporate into purely theoretical problems that go beyond them: such as form and matter, the

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local and the global. The very originality of "Mathematics" would be in danger of disappearing.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

I'm not trying to define Mathematics, but rather, by means of Mathematics, to find out what it means to know, to think; this is

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basically, very modestly, the problem posed by Kant. Mathematical knowledge is central for knowing what knowledge is.

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

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

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

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

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

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

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

I believe that this point of departure is never left, in the sense that there is an internal interdependence, and that every time we substitute a less well-thought-out mathematical object with more

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well-thought-out objects, i.e. we separate what was united merely accidentally, by the process I've indicated, to that extent all the same, we don't leave the sensible world.

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

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

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

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

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

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

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

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

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

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

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

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

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

I'd be much less inclined to agree with Mr. Fréchet. I spoke of the genesis of the real from the idea. Mr. Fréchet claims to understand only the opposite, i.e. the genesis of the idea from the real,

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

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

Mr. Chabauty points out that I've attached great importance to theorems that establish the existence of certain functions on certain surfaces or sets, but that this result may seem less surprising if you realize that the sets in question have been "tricked" in such a way that it's not hard to find the functions you're looking for on them. It would seem, then, that you can only find on a set what you have put there beforehand. Such a way of putting things doesn't seem

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to me to highlight sufficiently the fact that there can be two kinds of "tricking", in Mr. Chabauty's sense: those that are fruitful and those that are not. In terms of properties, a set never possesses more than those given to it *a priori* by axioms, but it may happen that some of these artificial definitions have the consequence of bringing a set or surface to such a state of completion or perfection that this internal perfection blossoms into assertions of the existence of new functions defined on this set. This fruitfulness of certain structural properties, which extends into the genesis of new mathematical beings, seems to me to distinguish, within the possibilities of axiomatic definition, creative conceptions from those that lead to nothing truly new.

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

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

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

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Discussion of an Identity Phylum

Jean-Michel Salanskis

Abstract. Are we right to attribute an a priori value to what we call *French epistemology* or the French tradition of epistemology? For a person educated in this country, it is difficult to answer in the negative. It's a bit like discrediting French wine, French cheese, or the French school of mathematics with its abnormal number of Fields Medals. Yet the various dignities just mentioned are obviously incomparable.

On a more serious note, for me the notion of French epistemology overlaps three things and gives rise to at least two guilty feelings.

Keywords. French epistemology, historical epistemology, neo-Kantianism, mathematism.

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§ 1. — Three faces and two guilts.

1) For many, the French school of epistemology coincides with the school of historical epistemology. The originality of the epistemological work carried out in France would simply consist in the adoption of a historical perspective. But this raises the question: are we really that much of a historian? From what I have observed, Italian academics, on almost all occasions, are more deeply attached to the historical point of view than the French. Either historical sensibility is not sufficient to recognize French epistemology, or the claim to national excellence is difficult to defend.

2) French epistemology is also seen by many as the antidote to the epistemology of the Vienna Circle, or more broadly to analytic

epistemology. Admittedly, the school's leading authors were also those who drew the attention of French philosophers to the new epistemological and philosophical style (particularly the two most recent, Granger and Vuillemin). The use of French epistemology *against* the logicist style of analytic epistemology nonetheless exists, and may still constitute a major reason for adherence. The difficulty here is that it is not immediately clear what competing principles so-called French epistemology opposes to the doctrine of Carnap, for example. If there is an antidote, it is not linked to an explicit critique or a similar counter-proposal. Rather, one gets the impression that French epistemology and the epistemology of the Vienna Circle, or the contemporary analytic epistemology that emerged after it, are not in the same league.

3) In a third way, the distinctive feature of French epistemology can be seen as its relationship to a Kantian philosophical background. From Brunschvicg to Granger and Vuillemin, French epistemologists seem to brood over Kant, seeming to search tirelessly for the amendment to the *Critique of Pure Reason* that will bring critical philosophy into line with the development of science. Indirect proof is provided by the following quotation from Thomas Kuhn:

"There have been philosophers of science, generally those with a vague neo-Kantian tinge, from whom historians can still learn a great deal today. I recommend that my students read Émile Meyerson, and sometimes Léon Brunschvicg. But I recommend these authors for what they have seen in the historical material, not for their philosophy, which I reject, in agreement with the majority of contemporary specialists"[(1)].

Thomas Kuhn does not name France, but we do read that he refers to Koyré as his main master in terms of the historical motive[(2)]. In our passage, he reliably mentions Brunschvicg and Meyerson, in whom Frédéric Fruteau de Laclos recognises the

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[(1)] *Cf.* Thomas Kuhn, *La tension essentielle*, trans. franç. M. Biesunski, P. Jacob, A. Lyotard-May and G. Voyat. Voyat, Paris, Gallimard, 1990, p. 44 [all quotations in the article are translated from French].

[(2)] He writes: "The men who did most to establish the tradition that flourishes today in the history of science — I am thinking in particular of A.O. Lovejoy and, above all, Alexandre Koyré — were philosophers before they turned to the history of scientific ideas. It was from them that my colleagues and I learned to recognise the structure and coherence of systems of ideas that were not our own" [*La tension essentielle*, p. 43].

founding fathers of the French epistemological spirit[3] : the former has such a status officially and notoriously, the latter holds it more clandestinely and in relation to a slightly different posterity[4]. In any case, Kuhn has no hesitation in identifying what our philosophers of science have in common, which is precisely the Kantian element.

Such would be the three figures, or such would be the three faces that the notion of French epistemology would superimpose, giving rise to a philosophical problem by its very identity. Earlier, I also spoke of two types of guilt. So what did I have in mind?

The first, and most obvious, is to ask for special attention, perhaps even exceptional esteem and favour, for a body of work that has no constitutive character other than its belonging to a national context. Is it enough for an epistemology to be recognised as French for it to merit a study aimed at recognising its value?

The guilt envisaged and confessed here is not far removed from chauvinism, which seems clearly a "sin against the spirit", to use the Christian nomenclature furtively.

The problem is made all the more intense by the polemical function attributed to "French epistemology": it is said to represent the possibility of a rebellion against the international model of epistemology, which was constructed in the analytical academic space in the wake of the work of the Vienna Circle, often taking advantage of the emigration of its first masters (we know that Carnap was Quine's master, as well as his colleague and interlocutor).

Let's face it: one may have been hoping, for a long time, that any conference on any epistemological subject would also give the floor — or even half the floor — to French epistemology. But there inevitably comes a time, it seems to me, when such person rebels against the favour thus accorded: shouldn't we rather listen to all the strong and interesting contributions, without worrying about their national tone? For my part in any case, it has become clear to me over the years that it is impossible to presuppose that the "French" contribution is as such essential. As an intellectual immersed in an international society of research and study, I can

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[3] See Frédéric Fruteau de Laclos, *La connaissance des autres*, Paris, Cerf, 2021, especially p. 335- 344.

[4] Typically, according to Fruteau de Laclos again, that of historical psychology (*cf.* Frédéric Fruteau de Laclos, *La psychologie des philosophes — De Bergson à Vernant*, Paris, PUF, 2012).

only consider a priori all sources as legitimate and likely to promise the best.

What I observe with some surprise is that, nevertheless, French sedimentation continues to interest me as such. I persist in wanting to understand how trends, approaches and paradigms have succeeded one another and blended, according to complex constellations and genealogies, within the small national space I inhabit. And it is not just for the obvious fact that this history determines me, to the point of governing more than I can imagine the work I am likely to accomplish. The French affair continues to arouse a specific curiosity in me, after I have ceased to attribute any a priori superiority to it. And in particular, after I have ceased to minimise in any way the developments proposed in the vast arena of contemporary analytic philosophy.

Let us try, then, to take the measure of this corpus of French epistemology, or "à la française", as it is sometimes called. After more than a century, this corpus is more extensive than one might suppose.

§ 2. — From historicism to mathematism.

Which authors do we have in mind, to begin with? The founding fathers, as we said earlier, referring to Fruteau de Laclos, would be Brunschvicg and Meyerson. Although Fruteau de Laclos rightly persuades us, with good reason, to emphasise the "complement" provided by Meyerson, thereby succeeding in describing a quasi-externalistic modulation of French epistemology that is most often ignored or minimised, we have to concede that the filiation in question is most often seen in terms of Brunschvicg[(5)]. After him, Bachelard is generally counted as an exemplary and/or significant expression of the current. Beyond these two emblems, names linked to the epistemology of mathematics (Cavaillès, Lautman, Desanti, Granger, Vuillemin) or, differently, to the epistemology of biology or the human sciences (Canguilhem, Foucault) come to mind. To these names could be added that of Althusser. In these series, I have forgotten Michel Serres, who is no stranger to what we

[(5)] A reviewer points out the equally fundamental role played by Henri Poincaré and the Boutroux circle. Henri Poincaré's personality seems to me to be fundamental: he embodies — by playing both roles at once — a pride typical of our movement, that of eliciting the agreement of the scientists themselves.

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are talking about. I have also neglected the epistemology of physics, whose importance cannot be denied (from Duhem to Bitbol and Soler, via D'Espagnat).

The methodological element common to these many proper names is undoubtedly the historical perspective. French-style epistemology is very often characterised as that which substitutes history for logic in order to "frame" the study of science.

We remember, however, that Kuhn saw, behind the consideration of history, the neo-Kantianism congenital to French researchers. Whether or not we agree with him on this point, perhaps we should say that it is not necessarily attention to history that characterises our national school, but rather the desire to understand the status of scientific knowledge on the basis of its style of historical variation. It is as if French epistemology instinctively reconstructed the essence of science on the basis of its historically observable variation. We would have a kind of differential equation $\mathrm{d}s = f(t)\,\mathrm{d}t$, where s is science,ds its infinitesimal variation, t is time and f is a function of time that allows us to calculate ds at time t. In this way, if F is a primitive of f, $F(t)$ would express science at time t, to within a constant. Except that, in this daring symbolisation, we replace finite variation by infinitesimal variation.

In the French context, such a historicising vision undoubtedly comes into conflict or competition with a determination of the essence of science that is otherwise known and learnt, the one provided by the Kantian system. And this is where we come across the Kuhnian evaluation. It seems to me impossible to say that Kantian determination takes science or scientificity as a function of time. The association or conjugation identified by Kuhn therefore seems to involve an impossibility or a paradox.

At the same time, as we know, the Kantian system was historically motivated by Newton's scientific breakthrough. In his *Prolegomena to All Future Metaphysics*, Kant explains that he can give two accounts of his transcendental system of knowledge. The first exposition constructs science as a priori knowledge, and elaborates its conditions and moments on the basis of its concept: it is an *architectonic* exposition in the sense of the section "Architectonics of Pure Reason" in the second half of the *Critique of Pure Reason*[(6)]. The second exposition starts from Newtonian science, and simply

[(6)] In this section, Kant contrasts a technical organisation and an architectonic organisation of a statement: whereas the former allows its branching structure to be dictated by the data encountered, the latter unfolds a form that is that of the

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attempts to reconstruct the normative structure of such knowledge in a regressive manner. If, then, the Kantian system is susceptible to such an exposition, this means that it is not radically alien to the historical perspective on science, or, at least, that it is in its own way sensitive to the events and refoundations of science.

Starting from the premise that French epistemology is as much a debate with the Kantian heritage as the adoption of the historical perspective, we can immediately add a second parameter that is less often emphasised than the historical parameter, but which could just as well characterise French approaches: that of "mathematism", if we allow ourselves such a neologism.

Beginning with Brunschvicg, and continuing uninterrupted through to Granger and Vuillemin, and including significant cases such as that of Albert Lautman, French epistemology stands out as an epistemology that accords a certain privilege to the function of mathematics in the natural sciences. The analyses of French philosophers of science emphasise the idea that science, in its sometimes revolutionary progress, manifests itself as a break with common sense, and that it can only do so by relying on a mathematical imagination of the world. Instead of conceiving of science as a common sense whose logical structure is closely monitored — as may still be the case with logical empiricism — French epistemology sees science as a mathematical construction with an empirical spin-off. The author who emblematises this option is undoubtedly Bachelard[7].

This epistemology's attention to historicity is even deduced from mathematism: science emancipates itself from its own theoretical frameworks to propose new ones, typically by reworking the mathematical structure chosen for the construction of the world. So what we need is a historical investigation that identifies the discontinuities in the mathematical fabrication of reality.

As much as the historical perspective, our filiation basically attempts to add to the Kantian system the vision of a permanent mathematical reconstruction of the world (a vision that is clearly in harmony with the Kantian understanding of scientificity). The above concerns physics first and foremost, but French epistemologists are also concerned, in the same way, with understanding the specific historical nature of mathematics.

idea of the whole of anticipated knowledge, and which is therefore a rationally necessary "schema". *Cf.* A 832–851, B 860–879, Ak III 538–549.

[7] Gaston Bachelard, *Le rationalisme appliqué*, Paris PUF, 1949, 2004, p. 133.

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In what follows, I would like to extend what I have just said by giving a more precise presentation of neo-Kantianism, which seems to me to be the main thrust of French epistemology.

§ 3. — French nenokantism: a reconstruction.

In my experience, and especially in the light of a recent reading of Jules Vuillemin's *Philosophie de l'algèbre* and *Physique et métaphysique kantiennes*[8], it seems to me that French neo-Kantianism can be characterised by five convictions or options, five philosophical operations perhaps.

Of course, this would require a study of the various authors of the filiation, highlighting the accuracy of such a reconstruction on the basis of the texts. I dispense with this, hoping that at least some readers familiar with the philosophers of science mentioned above will recognise in my reconstruction the atmosphere that is common to them. So here is my list.

1) *The historicist option*. Our authors in France are extremely sensitive to the profound changes that occurred at the end of the 19th century and the beginning of the 20th century (pluralisation of geometries, inauguration of set theory and formalism, theories of relativity, quantum theories). They deduce from this constellation of crisis events the idea that science must essentially be understood as self-revolutionary, which would force us to reckon with an intrinsic historical ingredient. The mutability of the scientific fact requires of us a "historicist" approach: not just the addition of historical examination to the study of theoretical constructs, but also the vision of scientific theorising as including in itself its own historicity. As if the scientific mind were an exemplary case of becoming. Thus, commenting on Lagrange's dissertation on algebraic equations, Vuillemin presents his contribution as going beyond the first Italian works (del Ferro, Cardan, Tartaglia), and as anticipating the Galoisian construction[9].

2) *The dynamic rationalist option*. This second point is close to the first, and largely dependent on it. Insofar as the historical declension is conceived here as not affecting rational deployment from the outside, but rather as a major trait of the rational essence, we can think,

[8] See Jules Vuillemin, *Physique et métaphysique kantiennes*, Paris, PUF, 1955; *Philosophie de l'algèbre*, Paris, PUF, 1962.

[9] *Cf.* Jules Vuillemin, *Philosophie de l'algèbre*, *op. cit.* p. 37–47.

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and neo-Kantian philosophers of science do, that the critical movement revealed by the historical perspective clarifies for us the radical nature of scientific rationalism. Studies of science that are attentive to this movement are therefore precisely the magnifying glass that reveals rationality at its most distinctive. Once again, Vuillemin understands the overcoming of Cardan et alii and the announcement of Galois as the very essence of Lagrangian rationalism (motivating, moreover, in this respect, an analogy[10] with Fichte).

3) *The "anti-intuitivist" option*. According to the analyses of our French philosophers, the correct understanding of the self-overcoming gesture of science requires the rejection or disqualification of intuition. The "fault" of intuition is that it is supposed to deliver to us, as it were, the object of which it is the intuition. The notion of intuition thus lends credence to the idea of an evident given (on the object side: a given; on the subject side: an evidence of the gift), and it is for this reason that it is judged to be bad and unsuited to the reading of the sciences. Indeed, the sciences do nothing more than go beyond any moment of obvious donation of this kind. They are constantly resorbing intuitive content, in order to take it up again in a systematic articulation that is constantly being reworked and rearranged. This option is linked to the vision of the "architectonic" function of reason already mentioned (*cf.* note 6). The attitude we are describing is indeed a "neo-Kantianism", which claims to be Kantian, at the very moment when analyses are being promoted that seem to prove him wrong or depart from him. In Vuillemin's case, the criticism under point 3) takes the form of a challenge to what he calls *extrinsic intuitionism*[11].

4) *The "mimetic" option*. Another aspect of our neo-Kantian philosophy of science is that we ascribe two levels of truth to the discourse of science. At the first level, it is "true" in the sense of the classical notion of truth: it proposes a theoretical organisation of the world that reflects its actual organisation, at least as long as the court of experience does not denounce this proposition. But at a second level, we have a kind of Heraclitean meta-truth: science surpasses itself, and thus appears as an organism in the process of becoming. In this way, it mimetically embraces an "absolute" becoming, which is the metaphysical core conquered as the essence of being by Heracliteanism. Our philosophers even have the impression that by recording the scientific movement in its language, the philosophy of science makes this movement its

[10] *Cf.* Jules Vuillemin, *Philosophie de l'algèbre*, *op. cit.* p. 48–55.
[11] *Cf.* Jules Vuillemin, *Philosophie de l'algèbre*, *op. cit.* p. 77–88.

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own, and participates in the scientific meta-truth[12]. The vision of such a meta-truth can already be seen in Brunschvicg[13], and in Vuillemin[14]. It is not only that reason recognises itself as essentially dynamic and that its own movement is apprehended at the level of science (as stated in point 2), it is also that becoming is recognised as the fundamental ontological evidence, and that science and the philosophy of science are assigned the task of corresponding to such a mobile essence, of apeing it, as it were.

5) *The physical-mathematical coupling*. That science is, par excellence, physical-mathematical is, in many respects, exactly the Kantian message, explicitly underlined in *Metaphysical Foundations of Natural Science*[15]. In the reading of our philosophers, the mainspring of the critical movement of science, of its perpetual self-depassment, lies in the resorption of the given and the obvious in favour of architectonic elaboration. But they see this gesture as identical between physics and mathematics. Mathematics abrogates the intuitive primacy of Euclidean geometry and reconstructs geometric contents as plural systematic developments. Physics goes beyond the given of the orthogonality of time to space, which corresponded to our intuitive apprehension of the dimensions of presentation, to entrust the thought of space-time to the theoretical system of Minkowskian geometry. In the end, mathematics and physics are the same thing, or mathematics is the language of going beyond the evidence of the given, which is at the same time the language of physics, and whose operation is verified in both disciplines. It is not only that our neo-Kantianism sees a continuity between physics and mathematics; it is also that it identifies the

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[12] Can a philosophy that claims this kind of mimicry at the same time inherit a method like Gueroult's structural method? I leave the answer to more learned people.

[13] In "La philosophie nouvelle et l'intellectualisme" [*L'idéalisme contemporain*, Paris, F. Alcan, 1921, pp. 98–185], Brunschvicg argues that the criterion of truth emanates from the development of science, and that this immanence is precisely what Kant would have given us. Brunschvicg also describes scientific explanation, constantly renewed by criticism, as a way of making reality continuous.

[14] Here, for example, is a significant quotation: "In the critical system, the thought of movement commands the movement of thought" [Jules Vuillemin, *Physique et métaphysique kantiennes*, Paris PUF, 1955. P. 41]. We can see that the conception leading to the conception of such a meta-truth is imputed to Kant himself.

[15] *Cf.* Immanuel Kant, *Premiers principes métaphysiques de la nature*, trans. J. Gibelin, Paris, Vrin, 1982, p. 11 (Ak, IV 470).

two disciplines by identifying their movement. Once again, this tendency can already be seen in Brunschvicg[16].

§ 4. — The very French anti-Kantism of neo-Kantism.

There is a coherence and strength to the system expressed by the five options. And the advantage of such a reconstruction is that, if you have been educated in France and have grown up in the hexagonal atmosphere, you will soon discover that you adhere or have adhered to some of these options. The five points do not give rise to a series of propositions to each of which you are likely to give your approval in a contingent and independent manner: rather, they determine a fragment of the national philosophical spirit, by which you find yourself influenced, even if unwittingly. The adage that in politics everyone has been, is or will be a Gaullist can be transposed to the field of the philosophy of science: everyone has been, is or will be a Neo-Kantian in the sense of the proposed reconstruction.

I am also convinced of the depth of this shared destiny by Alberto Gualandi's PhD thesis, published as a book by Hermann in 1998[17]. The author describes French philosophy in the xx^{e} century in terms of the epistemological orientation determined by Brunschvicg. He sees this as an extension of Kantian criticism which offers an alternative to the one favoured by the Vienna Circle and, by extension, by analytic philosophy as a whole. He describes how authors such as Brunschvicg and Bachelard internalised the

[16] Here is a passage, for example, in which Brunschvicg attributes the superposition of mathematics and physics to Kant himself: "We should go even further: the 'fragmentary' formation of critical philosophy provides a means of seeing how, under the influence of Newtonian physics, whose rational value it was later to serve to justify, the idea of mathematics underwent in Kant a kind of unconscious shift, the result of which was to make the demonstrations of arithmetic or geometry relate directly to things that are numbered or figures that are drawn. Later, no doubt, when he composed the *Critique* or the *Prolegomena,* Kant would believe that he was moving from 'pure mathematics' to physics ; but the question is whether he did not begin by substituting for the notion of pure mathematics a conception of applied arithmetic and applied geometry, so that the passage from arithmetic or geometry to physics will in fact be no more than the passage from a simple form to a more complex form of applied mathematics" [*cf.* Léon Brunschvicg, *Les étapes de la philosophie mathématique*, Paris, F. Alcan, 1922, pp. 257–258].

[17] *Cf.* Alberto Gualandi, *Le problème de la vérité scientifique dans la philosophie française contemporaine. La rupture et l'événement*, Paris, L'Harmattan, 1998.

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need for philosophy to follow the movement of science in order to reproduce or reflect it in itself.

And he makes this desire to embrace the external becoming indicated by the revolutions of science the major trait of French philosophy: this trait, in the end, passes from the philosophy of science to philosophy in general. So much so that Alberto Gualandi can describe the interventions of Deleuze and Lyotard as echoes of epistemological historicism: their discourse obsessed with the event and the metamorphic rupture transposes the national epistemological orientation to a philosophy of subversion. Gualandi goes so far as to name the work of Michel Serres as the intermediary through which this "generalisation" is accomplished.

In order to appreciate the regulatory nature of the system presented in relation to national intelligence, it is worth considering it in the wider context of the reception of Kantianism in France. To this end, we have at our disposal two extraordinary summaries recently written by Laurent Fedi and Pietro Terzi. These works of over 600 pages describe the adventure of Kantian reception, from 1854 to 1986 in the case of the latter, and from 1795 to 1940 in the case of the former[(18)]. Both show the paradox that constitutes the "Kantian favour" in France: although reference to Kant and the study of Kant very quickly became quasi-institutional parameters for philosophy in France, this does not mean that the readings and positions taken confirm or validate the Kantian message. On the contrary, the atmosphere of this reception was immediately polemical. The most official continuators of Kantianism, such as Renouvier and Brunschvicg, were opposed to it on essential points. In particular, none of the concertists of the French post-Kantian symphony validated exactly his conception of the transcendental.

For those who look at the five options listed just now, this opposition is, I think, quite clear. Very officially, the reference to history and the adoption of a certain historicism are presented by French neo-Kantian philosophers of science as additives to Kant, or as corrections to Kantianism. Vuillemin puts it this way in the following passage:

"(...) how is the transcendental constitution to be conceived, if the correlate of the Cogito is no longer the universe of rational

[(18)] *Cf.* Laurent Fedi, *Kant, une passion française 1795–1940*, Olms, Georg AG, 2018; Pietro Terzi, *Images de Kant et formes du criticisme dans la philosophie française contemporaine, 1854–1986*, doctoral thesis from the University of Paris Nanterre and the Fondazione San Carlo in Modena, January 2020.

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Mechanics, but the world of historical experience? How can the unity of knowledge be preserved without doing violence to the dissociation of vulgar and scientific experience? How can we finally reconcile the true with the relative, and, in the incessant variation of the methods and principles of the most exact sciences, ensure the certain foundations of philosophy?"[19].

A formulation that is both measured and perplexing.

The disapproval of intuition is also presented as the liberating element that saves science from the prisons in which Kantianism wanted to keep it. Vuillemin expressly points out the Kantian doctrine of intuition as a case of bad *extrinsic intuitionism*[20], Bachelard, according to the Bachelardians, only wants to consider worked intuition as a component of scientific value, and therefore cannot validate Kantian pure intuition, understood as immediate.

On the other hand, the vision of science as an architectonic whole striving ever more to unfold reason from itself is clearly what our authors retain from Kant, what they take from him and in the name of which, moreover, they feel they must shake the epistemological edifice of the first critique.

On the other hand, the alignment of mathematics with physics, a consequence of the reference to the architectonics of pure reason and, strangely enough, of the "mathematism" of the French school itself, once again constitutes a head of opposition to Kant. Mathematism' tends to reduce physics to the mathematics that plays within it, but the result is that the separation and autonomy of mathematics, affirmed with rare clarity in Kant, is abandoned or forgotten. We quoted Brunschvicg a little earlier on this subject (note 13): in the corresponding passage, the author's discomfort (with respect to the Kantian precedent) seems quite noticeable.

To conclude, this was one of the points we wanted to make: the school of French epistemology is an emblematic example of how French philosophy has managed to perpetuate its paradoxical game with Kant. On the one hand, this school participates in the institutionalisation of criticism and transcendental thought — to the

[19] See Jules Vuillemin, *Physique et métaphysique kantiennes*, Paris, PUF, 1955, p. 360.

[20] We read, for example, the following, which illustrates the notion of extrinsic intuitionism in context: "This is especially the case with Kantian philosophy, when it demands that the concepts of the understanding relate to intuitions that are necessarily sensible, and whose pure form therefore relates to the exteriority of space and time" Jules Vuillemin, *Philosophie de l'algèbre*, *op. cit.*, p. 78.

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extent that Desanti can still, quite recently, complain about it[21] — while, on the other hand, it displays a resolute opposition to Kant. But as we said, this is the dual aspect that has characterised the French reception of Kant from the outset, across all branches of philosophy.

§ 5. — French neo-Kantianism and analytic epistemology.

Finally, it may be appropriate to take into consideration once again what was mentioned at the beginning of this article, namely the confrontation with the logical empiricism of the Vienna Circle (or even with analytic epistemology in general).

Among us, but also to some extent outside us, according to the vision of members of other nations, European or non-European, French epistemology functions — at least sometimes — as an antidote to an international epistemology whose first crystallisation was the empiricism of the Vienna Circle, and which has since given rise to reformulations or often critical clarifications (starting, spectacularly, with an author like Quine)[22].

Is this the right way to understand the lineage of authorship discussed from the beginning of this article?

In a sense, the answer must be in the affirmative, because our French authors perceived what was going on in logical empiricism as far as epistemological matters were concerned, and more often than not expressed a distance or reticence. Brunschvicg had already criticised Russellian logistics, in terms that seemed to be aimed at all mathematical formalism[23]. Meyerson wants to understand science in terms of a kind of psychological device of which it is a symptom, and takes the liberty of confronting his views with the Hegelian model[24], adopting an approach that is totally incompatible with the new Carnapian norm. Lautman and Cavaillès

[21] Here's a quote: "This is the third form of 'internalization' of science into philosophy. Inaugurated by Kant in a well-defined epistemological field, it has long outlived him; and even today it continues to nourish, in France at least, certain approaches to school philosophy" [Jean-Toussaint Desanti, *La philosophie silencieuse*, Paris, Le Seuil, 1975, p. 21].

[22] I am thinking here of his famous "The Two Dogmas of Empiricism".

[23] See Léon Brunschvicg, *Les étapes de la philosophie mathématique*, *op. cit.* pp. 421–426.

[24] Notably in *De l'explication dans les sciences* (Paris, Fayard, 1921, 1995).

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take into account the views of the new epistemology, but oppose them with the movement of science and the image they favour. Granger and Vuillemin regard Frege, Russell and Carnap as important milestones to be discussed indefinitely, but refuse to establish the philosophy of science once and for all in the "positive", "naturalistic" and "analytical" language of the new movement. In his entry *Epistemology* for the Encylopædia Universalis, Granger discusses the possibilities and challenges of epistemology using names as diverse as Russell, Bachelard, Kuhn, Descartes and Kant, without forgetting to include Foucault's *Archéologie du savoir* in his bibliography: this is a far cry from the restricted space of the analytic debate[(25)].

But in another sense, the current of French epistemology, as we have assessed it, has features in common with the school that grew out of logical empiricism. Both sides turn against transcendental epistemology, seeing it as a straitjacket that has unfoundedly claimed to imprison science in one of its historical states. This conclusion was first asserted in the name of the pluralisation of geometries during the 19th century, understood as the irremediable refutation of Kantian transcendental aesthetics (an argument found in both series of texts). Admittedly, the emphasis of this disapproval was not the same: on the French side, the axiomatic character of all geometrical discourse is emphasised; on the side of logical empiricism, claims are made either for the inaccuracy of the underlying cognitive psychology (it is not true that three-dimensional Euclidean geometry is "wired into us"[(26)]), or for the *principle of tolerance*, authorising the scientist to fire any logical-mathematical systematisation, as long as he integrates experimental data by means of it.

If we look more closely at the currents over the long term, instead of sticking to what may have been their "original" contrast, it seems to me that the difference in tone between, on the one hand, a French-style philosophy of science obsessed with history and change and, on the other hand, an analytical epistemology stemming from logical empiricism and more concerned with the methodological characterisation of the transhistorical figure of scientific knowledge,

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[(25)] *Cf.* Gilles-Gaston Granger, *Epistemology*, in *Encyclopaedia Universalis*, vol.**7**, 1985, p. 61–68.

[(26)] As Carnap says, for example, in "Dreidimensionalität des Raumes und Kausalität" (*Annalen der Philosophie und philosophischen Kritik* **4**, 1924, p. 105–130).

although noticeable at the outset, is tending to fade. Quine's article[27] on the two dogmas of empiricism, already mentioned, was a turning point. From this point onwards, the adjustment between the a priori (even if logical) and the a posteriori (denounced as less immediate than first assumed) is now conceived on both sides in a dynamic and historical manner[28]. The fact that the Duhem-Quine thesis has this double name seems to indicate or confess a convergence[29]. Kuhn should also be cited here, as we did at the beginning of this article.

The idea of dynamic adjustment suits, it seems to me, a Hegelian-dialectical vision of the mind and its adventure. But American pragmatism, with an author like Dewey, started from there[30], and some recent specialists — developing a philosophy of experience that is originally a philosophy of language — return to the same point, such as McDowell or Brandom[31].

French epistemology should perhaps not be read solely and essentially as an antidote to the analytical orientation that has prevailed in the world since the Vienna Circle.

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[27] *Cf.* W.O. Quine, "Les Deux dogmes de l'empirisme", *De Vienne à Cambridge*, transd. franç. P. Jacob, Paris, Gallimard, 1980, pp. 93–121.

[28] Quine's article shows the bridge he builds in its very temporal organisation. In the first four sections, Quine investigates, in the purest analytical logico-linguistic style, the possibility of rigorously separating analytical judgements from synthetic judgements, considering criteria and counter-examples. He thus considers synonymy, definitions and semantic rules. Addressing the second dogma — that of reductionist semantic verificationism — in the fifth section, he leads the discussion more along the lines of the history of philosophy, evoking Carnap, of course, but also Locke, Hume and Bentham. Finally, in the sixth and last section ("Empiricism without dogma"), acknowledging that the first search is futile, he comes to describe the overall situation of the sciences in their confrontation with reality, outlining the arc of the collaboration of several disciplines (at least logic, mathematics and physics), and taking into account recent data such as quantum developments. In this way, it seems to embrace an attitude that is at once historical and philosophical of culture, which was natively that of French epistemology.

[29] The postulation of such an overlap can undoubtedly be criticised. It is true that Quine, on the other hand, fully embraces the "naturalism" claimed by the new epistemological school, notably in his essay "L'épistémologie devenue naturelle (in *Relativité de l'ontologie et autres essais*, trad. franç. J. Largeault, Paris, Aubier-Montaigne, 1977, pp. 83–105).

[30] *Cf.* J. Dewey, *Darwin's Influence on Philosophy, and Other Essays on Contemporary Philosophy*, trans. franç. Lucie Chataigné-Poutego, Claude Gauthier, Stéphane Madelrieux and Emmanuel Renault, Paris, Gallimard, 2016.

[31] *Cf.* J. McDowell, *L'esprit et le monde*, trans. franç. C. Alsaleh, Paris, Vrin, 2007; and R. Brandom, *L'architecture des raisons*, trans. Claudine Tiercelin and Jean-Pierre Cometti, Paris, Cerf, 2009.

§ 6. — Concreteness of epistemology.

That said, there is still one element that could complete the portrait of this movement to which we are attached without really knowing why. This would be the element of concreteness, or attachment to the particular. This element is circumstantially linked to the adoption of the historical perspective, but in my view it goes beyond it. The underlying question would be the following: is a philosophy of science supposed to reflect something of the particular scientific affairs on which it constructs a theoretical position, or must it stick to the general constituent features of this position in the conceptual and argumentative space of the philosophy that expounds it?

It seems to me that the spontaneous practice of French epistemologists is to give in to the particularity of the scientific moments commented on: this can be done by highlighting a historical context, by emphasising a biographical aspect of the scientist in question — Newton, Cantor, Einstein, etc. — or by elaborating a fragment of the technical content of the scientific event in question. Whatever philosophy's appetite for generality and universality, the French tradition senses that, when it comes to talking about science, it is also important to capture something of the particular case we are dealing with. Because, perhaps, the genius of science lies in the specificity of its procedure or its arrangements.

This does not mean that "French-style" epistemology refuses to speak at the most general level, that of a philosophy of knowledge in search of its conditions. It does so, as several of the authors already mentioned bear witness. But it does so by tolerating a kind of multi-modality in its texts, which is a constituent of their stretching. The writings of French epistemology are very often marked by a kind of tension between two extremes: they are expressed at the metaphysical level, where one wonders how a discourse can correspond to the fabric of being (or formulate the need for a fictional order), but also at the quasi-anecdotal level where one tries to grasp from the inside the approach of this or that great name of science grappling with this or that difficulty.

My last word will be to underline the extraordinary and in many respects "perfect" precedent provided by Albert Lautman's work, in the case of mathematics. In his *Essay on the Unity of Mathematics*, we find on the one hand the formulation of a more or less complete philosophy of mathematics (with a theory of mathematical

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objectivity and a theory of mathematical change at least)[32], on the other hand a fairly precise analysis of several recent developments in mathematics at the time he was writing and working[33], and finally some interpretative gestures at the level of the history of philosophy, intended to give us a better understanding of the conceptual elements involved[34].

For me, ending with such a tribute to Lautman is also a way of avoiding any self-satisfaction in the field we are dealing with. If there is a French tradition of epistemology of some value, that does not immediately mean that we can live up to the model thus drawn. Rather, it seems to me that what Albert Lautman achieved not so long ago has become very difficult for us to repeat or match, for a whole series of reasons that could include our own inertia.

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[32] See in particular Albert Lautman, *Essai sur l'unité des mathématiques et divers écrits*, Paris, Union générale d'éditions, 1977, pp. 135–147.

[33] See in particular Albert Lautman, *Essai sur l'unité des mathématiques et divers écrits*, *op. cit.* pp. 31–82.

[34] See Albert Lautman, *Essai sur l'unité des mathématiques et divers écrits*, *op. cit.* pp. 205–209 and 143–146.

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Jean Cavaillès's struggle with the problem of coordination

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Abstract. In this essay, I propose a new key to the interpretation of Cavaillès's "philosophical testament" *Sur la Logique et la théorie de la science* (1942/47), or at least to one of its main philosophical motives: to find a principled answer to the problem of coordination between pure mathematics and physical theory (and thus also to clarify, first of all, the status of mathematical physics). Cavaillès's manifesto, culminating in the idea of a "philosophy of the concept", not only articulates the core ideas of a new philosophy of mathematics around the dynamics of "paradigm" and "thematization"; it also contains a project of a new "doctrine of science" in general. The latter should explain the possibility of conceiving a worldly knowledge that incorporates the intrinsic dialectic of mathematical concepts, but at the same time supplements this internal conceptual development with something radically different: a reason-led action of experimenting and wagering on events in the world. I develop this view of Cavaillès against the background of the ideas of some of the leading thinkers on the coordination problem: Mach, Poincaré, Schlick, Reichenbach, Carnap, Brunschvicg, Gonseth, Suzanne Bachelard, Bas van Fraassen...

Keywords. Cavaillès, Brunschvicg, Carnap, coordination problem, epistemic status of physics, conventionalism, concatenation vs. event, necessity of internal development vs. contingency of history.

§ 1. — Introduction.

A historian of the philosophy of science examining the half-century between 1890 and 1940 will not fail to notice that a lot of essays and exchanges in the field were devoted to the so-called "problem of coordination".[1] A solution to this problem might even have seemed decisive for anyone aspiring to establish a solid general theory of science. To convince oneself of this, one need only mention a few of the philosophers who have struggled to state and then solve the problem: Poincaré, Duhem, Le Roy, Mach, Reichenbach, Carnap, Schlick, Brunschvicg. In this essay I claim that the role of this problem is also decisive if we want to understand Jean Cavaillès's late attempt towards a "theory of science".[2] Assuming that claim is defensible, it would reveal a fact that has gone largely unnoticed.

Nowadays, the problem of coordination hardly appears in treatises, and few philosophers would have a ready answer to the question of what exactly is meant by the term. Indeed, after the Second World War, the expression itself seemed to have disappeared from the scene. Has there, indeed, been a single well-defined notion and problem that is targeted when speaking of 'coordination' in the philosophy of science? And what would be the relevant meaning intended in Cavaillès's 'philosophical testament'?

As a preliminary, let's briefly recall the philosophical background of the question and give a general characterization of the

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[1] I thank Bas van Fraassen, Gerhard Heinzmann, and Marjolein Holvoet for inspiring comments on earlier versions of this paper.

[2] *Sur la Logique et la théorie de la science.* P.U.F. 1947, Vrin, 1962, 1976, 1984, 1997 (the 5th edition includes an extensive Afterword by Jan Sebestik). A translation by Theodore Kisiel appeared in 1970: *On Logic and the Theory of Science.* In J. Kockelmans & T.S. Kisiel (eds.), *Phenomenology and the Natural Sciences. Essays and Translations.* Northwestern University Press, p. 357-409. A new translation has been published by Robin Mackay and Knox Peden (with an introduction by Knox Peden). Urbanomic /Sequence Press, 2021, 136 p.

For a general introduction to Cavaillès as a philosopher of mathematics, see my "The Structure of Mathematical Experience According to Cavaillès", *Philosophia mathematica* 4(1996), p. 18-41; as well as, among others,

H. Benis Sinaceur, *Cavaillès.* Les Belles lettres, 2013 (*passim*); Id., *Cavaillès Philosophie mathématique.* P.U.F., 1994; G. G. Granger, *Pour la connaissance philosophique.* Odile Jacob, 1988, p. 70-88. For an in depth-essay on Cavaillès as a philosopher of science, see Elisabeth Schwartz, « Le 'testament philosophique' de Jean Cavaillès : vers une Logique de la création ? », *Revue de métaphysique et de morale* 2020 n°2, p. 165-198.

notion here at stake. The problem of coordination would, in its various appearances, have as its common core the question of how a purely mathematical theory can become a theory possessing a specifiable empirical content. How can concepts and propositions belonging to an abstract mathematical theory become concepts and propositions with a content that makes them suitable for representing physical magnitudes (preferably in the form of measurable quantities)? To take a classic example, how does abstract geometry translate into physical geometry, and what role do conventions play in the choice of geometry if the latter is intended to represent observable phenomena?

Next, we need an overview of certain aspects of the thought of authors who have each, to varying degrees, given a more specific interpretation to questions of coordination. This diversion will lead us to two main characters of a founding period in the philosophy of science: Ernst Mach and Henri Poincaré (§2), who are at the origin of a conventionalist vision. Subsequently, we will look at two representatives of logical empiricism in its first phase. Hans Reichenbach and Moritz Schlick wanted to define the problem of coordination along the same lines as their predecessors, but in a more rigorous way (§3). In the meantime, in a more typically French epistemological trend, represented here by Léon Brunschvicg, there was, on the contrary, a search for a broader interpretation that was more in line with the Kantian inspiration, developing the perspective of a "philosophy of consciousness" or "reflective analysis" (« *analyse réflexive* ») (§4). Here, we might ask to what extent such a more psychologising notion of coordination partakes in the same concept as the analyses of the 'logicians'. In Cavaillès's project, the more rigorous and the broader tendency will intersect, and both will be subjected to a more or less pronounced criticism. However, the examination of his own alternative (which is only formulated in a very embryonic form) will require a new passage through logical empiricism. In particular, we will see how Cavaillès understood Carnap's conventionalism as a challenge to define his own version of the coordination problem (§ 5).

But first, if it is true that this problem has been forgotten, could it be that it is obsolete, if not solved? If so, what interest beyond the historical would there be to revive it? One indication that interest should go beyond motifs peculiar to intellectual history is that the question has resurfaced. And indeed, the problem has been brought

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back to the fore by Michael Friedman in *Dynamics of Reason* (2001)[3] and by Bas van Fraassen in his 2008 *Scientific Representation: Paradoxes of Perspective*.[4] In particular, due to conceptual, historical and technical developments in van Fraassen's *magnum opus*, we might well see the problem of coordination rise from its ashes.[5] So, in a final section (§6), I show not only what might have been the path chosen by Cavaillès in his attempt to formulate at least a principled answer, but also how this path can be linked to later attempts to put coordination back on the philosophical agenda.

A second reason, raising the interest above the historical, is even more substantial for my purposes. It can be summed up in the form of a conjecture: once we realise the important role that coordination plays in *On the Logic and Theory of Science* [henceforth: *LTS*], what is at stake in that work will have to be seen in this light. This re-reading will force us to rethink the whole — or at least an essential aspect — of Cavaillès's 'philosophical testament'. How, then, would it affect its interpretation? My aim is to show that the intention of this last writing was not simply to bequeath to posterity a philosophy of mathematics that might or might not be at odds with the author's earlier writings, and that might or might not in turn have consequences for a tenable view of science. In this respect my reading is at odds with a lot of writing on Cavaillès. Many commentators see him as the philosopher who focuses exclusively on mathematics as he would regard it as the sole embodiment of the rigour of thought and knowledge. Now, of course, much has been said and written to the effect that the book should be read as the inauguration of a new epistemological programme with wider

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[3] Stanford Kant Lectures. CSLI Publications, 2001.

[4] Clarendon Press, Oxford, 2008, xiv + 408 p.

[5] Some of this renewed interest concerns the history of science and the philosophy of science; some of it concerns the philosophy of measurement; some of the literature deals with the fate of specific currents such as post-Kantianism or conventionalism; and finally, some of it deals with the link established by van Fraassen between coordination and the emergence of the 'structuralist programme' in the theory of science (a link which is also reflected in the realist and anti-realist variants of this structuralism). See, for example, Hasok Chang, *Inventing Temperature. Measurement and Scientific Progress.* Oxford U.P., 2004; Flavia Padovani, "Coordination and Measurement: What We Get Wrong About What Reichenbach Got Right", in M. Massimi, J.-W. Romeijn, G. Schurz (eds.), *EPSA 15 Selected Papers*. European Studies in Philosophy of Science 5, 2017, pp. 49-60; Id., "Measurement, Coordination, and the Relativized A Priori", in *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 52(2015), pp. 123-128.

implications than those concerning the foundations of mathematics. Under the aegis of a 'philosophy of the concept', this programme would put an end to attempts to found scientific knowledge in a 'philosophy of consciousness'.[6] But the formula invoking a philosophy of the concept lends itself to all sorts of interpretations and uses as long as we fail to specify its terms and relate it to the identifiable fields of knowledge to which it would apply. Against what background of theoretical choices and historical trajectories should we understand this opposition between a 'philosophy of consciousness' and a 'philosophy of the concept' as conceived by Cavaillès? My ambition is not to provide an answer to this question within the limited framework of this essay. However, the primary aim of this study is to prepare such an answer by highlighting the role of the notion of coordination in the perspective of a doctrine of science as understood by Cavaillès.

For Cavaillès did indeed have the ambition of outlining a programme for a "doctrine of science" (*LTS* 14, 24, 41, 52, 64f, 69, 78). Already the choice of the somewhat archaic expression "doctrine of science" does not seem arbitrary: it is an analysis that aims at a relevance that is not only descriptive but also normative of the concept of science, in other words: an analysis of what makes knowledge qualify as science. The expression thus evokes the sense of a '*Wissenschaftslehre*' as the term was conceived mainly by Bolzano and by Husserl. Moreover, the relation between mathematics and physics — more precisely, this relation as defined by Kant — remains decisive if we are to understand what tasks a theory of science will have to fulfil, according to Cavaillès. At the same time, it should be emphasised that (and we shall see that) nothing has been taken away from the central place of mathematical thought within this programme. Otherwise, the problem of coordination would be meaningless.

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§ 2. — Emergence of a problem.

Let's take a closer look at some of the contributions by Mach, Poincaré, and then Reichenbach and Schlick, to see both the unity

[6] The classic reference is Foucault's famous formula evoking this contrast, in his introduction to the American translation of G. Canguilhem's *Le Normal et le pathologique*: *The Normal and the Pathological*. Zone Books, 1991, p.8-9. See also "La vie : l'expérience et la science", in *Revue de Métaphysique et de Morale*, 1985/1, 90e Année, p. 3-14.

behind the appearances of a multitude of interconnected problems, and the conceptual evolution of an issue that (to put it mildly) has not received an invariant interpretation. As for Reichenbach's and especially Schlick's remarks on coordination, I limit myself to a few aspects relevant to Cavaillès's way of posing the problem.[7]

We find in **Ernst Mach** what may well be the first explicit appearance of the notion of coordination in an epistemological and technical sense, especially in the expression "coordination principle" (*Zuordnungsprincip*). It should be noted that an expression of such generality is first encountered in the context of a particular theory and specific experiments: in this case, the theory of *heat*, seen mainly from the point of view of *measuring* heat. It is obviously through the possibility of measurement that data that were previously only accessible in qualitative terms become physical quantities.

> "The number which, according to some coordination principle, *is univocally coordinated with a thermoscope volume indication and therefore with a state of heat* (*Wärmezustand*), *is called temperature.* In the following, we generally refer to this state as t. The same heat state will then be assigned a very different temperature number depending on the [chosen] coordination principle $t = f(v)$, where v stands for the thermoscopic volume". (*Principien der Wärmelehre*, 1st edition, 1896)[8] (emphasis in the original).

Mach also emphasises the conventional nature of the choice of a coordinating principle, and relates this emphasis to a certain philosophical attitude:

> "It is remarkable how long it took for the idea to take root that the indication of a thermal state by a *number is* based on a *convention* (*Übereinkunft*). There are thermal states in nature; the concept of temperature, on the other hand, only exists thanks to our arbitrary *definition*, which could also have turned out to be very different".

That insight prompts a rather ironic comment on his part:

[7] All translations of quotes in the article are mine unless otherwise indicated.

[8] Leipzig, Verlag von Johann Ambrosius Barth, p. 46 § 11. For this example, see also van Fraassen, *o.c.*, p. 116 ff.

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> "Until the most modern times, workers in the field seem to have been occupied with searching, more or less unconsciously, for a natural measure of temperature, a real temperature, a sort of platonic idea of temperature, of which the temperatures we read on the thermometer would only be an imperfect, imprecise expression" (*ibid*., p. 48 § 14).

Let's move on from *heat* to *time*. In a famous analysis of the notion of equality of time intervals, **Henri Poincaré** deploys more or less the same epistemological elements: starting from the context of a particular physico-mathematical theory — in this case on the conception and measure of time — the discussion is quickly generalised (from time to space: physical geometry, then to all of physics). If there is 'a' problem of coordination, it simply concerns the possibility of applying the concepts of pure mathematics to physical or more generally empirical phenomena; or conversely, it concerns the possibility of translating empirical problems into mathematised problems (and creating the possibility of solving them in this way). Then, and above all, there is the characteristic already highlighted by Mach in 1896: this problem of applicability or translation is a problem of measurement. Measurement instruments must be developed, and these instruments in turn embody physical theories that are often still in the process of being developed. What is more, there's the interdependence between the concepts that belong to the theory whose parameters we want to make measurable, such as the theory that posits a relation between time (duration) and distance (length): what defines length, for example, must already have an independent meaning before it can be used to measure time. Thus, Poincaré places the same emphasis on the essential role of convention and choice, but with the addition of a qualifier — convenience — which should remove the latter from the arbitrariness of mere decision.

Following a famous discussion of the difficulties encountered in attempts to arrive at a non-circular determination and measurement of time (*The Value of Science*, Chapter II, sections 3 to 6), here is Poincaré's conclusion:

> "There is no one way of measuring time that is truer than another; the one generally adopted is merely more *convenient*. Of two clocks, we do not have the right to say that one works well and the other badly; we can only say that

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> it is to one's advantage to refer to the indications of the first"[9] (emphasis in the original).

Translated into the terms we are dealing with here: we need to coordinate the abstract notion of time with the temporal phenomena to be measured in such a way that the values assigned by our procedures give a faithful and consistent empirical interpretation of this notion. This coordination is a matter of conventions. Soon, of course, Poincaré's conventionalism will be confronted with the new situation created by the theory of relativity. But even before this confrontation, we need to realise that two forces here at work are not necessarily in pre-established harmony. One simply invokes convention as a definition which is in principle arbitrary and interchangeable between several alternatives — a position further radicalized in Édouard Le Roy's version of conventionalism.[10] The other invokes a comparative judgement of pragmatic value, thus providing a principle justifying a choice between the different conventions at stake. Although obscured in Poincaré's subtext, in retrospect one may read the announcement of a tension between two possible interpretations of his thought, in so far as it points at the same time to the need for coordinating principles and to their relativity.[11] Clearly, this tension has been kept under the radar due to the spontaneous tendency to interpret convenience as a preference for "the simplest theory" — in other words, by translating it into a well-known language of "epistemic virtues" (in this case, pragmatic virtues). But then, the question is this: in the complete and complex edifice made up of theory and of the experiments that are supposed to put it to the test, exactly which aspect should be optimised in terms of simplicity?

By analogy, in geometry there will not be a single true system of axioms (no synthetic *a priori* as in arithmetic, according to Poincaré). But the Euclidean system is the simplest. So, it will be preferred, as long as it turns out to be empirically equivalent with alternative systems. In any case, there has to be a coordination, so there has to be a choice. Einstein did appreciate Poincaré's point of view:

[9] Poincaré, *o.c.*, p.44.

[10] E. Le Roy, "Science et philosophie", *Revue de métaphysique et de morale* 7(1899), 375-425 and 8(1900), 25-75.

[11] This tension probably resurfaced in the famous dispute on convention in science between Carnap and Quine.

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> "In order to be able to make [empirical] statements [about the behaviour of what are called practically rigid bodies] geometry must be stripped of its purely logical and formal character by coordinating objects accessible to experience with the empty conceptual schemes of axiomatic geometry".(12)

Einstein thus adopted Poincaré's (at least implicit) distinction between the empirical *assertion* made by a theory and the *frame of* reference (or "conceptual scheme") on the basis of which this assertion can be made; and in so doing, he also adopted the terminology of "coordination". However, it seems that Einstein diverted Poincaré's conclusions from their initial purpose by turning conventionalism into a principle of choice that favoured his new mechanics over the classical Newtonian one. This is because he was able to convert the privilege granted to the simplest *framework* into a privilege for the simplest *assertion* formulated within this framework (always subject to the empirical equivalence of the two theories): as we will see with Schlick concerning special relativity, the simplest frame of reference may well require extremely complex formulations to describe reality.

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§ 3. — The notion of coordination in logical empiricism (1st stage).

It was **Hans Reichenbach** who, after Poincaré's preoccupation with convention, explicitly made coordination his theme. *Relativitätstheorie und Erkenntnis A Priori* (1920) took up the question of the interpretation of the new mechanics. At that time, under the influence of Cassirer and others, Reichenbach saw this theory as a partial revaluation of the Neo-Kantian point of view. This, in retrospect, brings him closer to the philosophers of science we will examine further on: Brunschvicg and Cavaillès — at least in the way they too formulated the question.

For Reichenbach, the concept of synthetic *a priori* knowledge must not be abandoned but reinterpreted: and this must be done in terms of a relativised or historicised *a priori.* There are no principles or categories that are valid once and for all, but each time a new theory sets up an *a priori* framework that allows empirical-theoretical

(12) "Geometry and Experience" (1921), in *Ideas and opinions*. Transl. Sonja Bargmann, New York, 1982, pp. 234-235.

assertions to be expressed and put to the test. In that sense, he calls these frameworks "constitutive"[13]: this is the aspect of Kant that must be safeguarded — not the requirement to start from necessarily true statements. In this context, it is the principles of coordination that play a fundamental role. We must, however, recognise that

> "... the coordination established in a physical proposition is very peculiar. It differs distinctly from other kinds of coordination. For example, if two sets of points are given, we establish a correspondence between them by coordinating to every point of one set a point of the other set. For this purpose, the elements of each set must be *defined*; that is, for each element there must exist another definition in addition to that which defines the coordination to the other set. [But] such [independent] definitions are lacking on the side of coordination dealing with the cognition of reality. Although the equations, that is, the conceptual side of coordination, are uniquely defined,

[13] He was followed in this respect much later by Michael Friedman (*Dynamics of Reason*, *o.c.*) who, unlike Cassirer, also wanted to retain the constitutive (and not merely regulative) value of the relativised *a priori.* Here Friedman comes close to the first Reichenbach. Laurens Vanderstraeten describes Friedman's project as follows: "[he] develops the notion of relativised *a priori* principles, which cannot be tested directly in experience, but *rather define the space of empirical possibilities for a certain theory*. This notion is illustrated in the case of three theories of space-time: Newtonian mechanics, special relativity and general relativity. In these theories, three asymmetrically functioning parts can be distinguished. The first is the *mathematical* theories, representations or structures describing the space-time framework in question (Euclidean space, Minkowski space-time and Riemannian varieties respectively). The *physical* or empirical part (universal gravitation, Maxwell's equations, Einstein's equations) uses these structures to formulate precise physical laws for empirical phenomena. But for these mathematical laws to acquire a precise empirical value, *a third part is needed* (the Newtonian laws of motion, the principle of the speed of light, the principle of equivalence) in *order to establish a general correspondence or coordination between the mathematical and empirical parts. This part is made up of principles of coordination* that are relativised but *a priori*". (*Ernst Cassirer and a Transcendental Approach Towards Contemporary Physics.* RUG01-002349275_2017_0001_AC.pdf. Master's thesis, Ghent University/University of Ghent, 2017, p. 19 (my translation; emphasis added).) Schematising the example:

mathematics	Euclidean space	Minkowski space-time	Riemannian varieties
rational mechanics	Newton's axioms	speed of light	equivalence principle
physics	universal gravitation	Maxwell's equations	Einstein's equations

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> the 'real' is not. On the contrary, the 'real' is defined by coordination to the equations."(14)

Which means that the notion of coordination to be discussed here is quite peculiar. It seems likely that Reichenbach was also aware of a possible tension between convention and convenience. Thus, after a second phase in which he turned away from a Kantian interpretation in favour of the new trend — logical empiricism — he ended up adopting a position that was closer in many respects to (American) pragmatism than to Schlick, Carnap and Neurath. "Coordination of language and physical circumstances replaces his earlier coordination of Kantian concepts and sensation".(15) To simplify, we could say that, from the earlier to the later Reichenbach, a pragmatic attitude (combined with an empiricism that attempts to preserve some type of realism) has replaced the post-Kantian attitude. This step must also be related to a desire to distance himself from an epistemological foundationalism, a dissociation that was linked to his attempts to develop a probabilistic epistemology.(16)

Moritz Schlick was quicker than Reichenbach to move away from Kantian presuppositions in connection with the epistemology of contemporary physics. It was above all his philosophical interpretation of relativity that won Einstein's admiration — by distinguishing between the empirical assertion a theory makes and a scheme of representation (or conceptual framework) belonging to the theory.(17) This means that, when it comes to choosing between theories, he argues for the decisive role of the simplicity of the *assertion*, and therefore of calculations based on the prediction.(18) Thus, using the distinction initially introduced by Poincaré, he turns it against Poincaré, and therefore against Euclid and Newton, and in favour of

(14) English translation by Maria Reichenbach: *The Theory of Relativity and A Priori Knowledge.* Berkeley, 1965, pp. 37-38. See also for coordination according to the young Reichenbach: van Fraassen 2008, p. 118-121.

(15) Clark Glymour & Frederick Eberhardt, "Hans Reichenbach", *Stanford Encyclopedia of Philosophy* (2008 article). `https://plato.stanford.edu/entries/reichenbach/`

(16) It should be noted in passing that for Reichenbach the principles of the calculus of probabilities could also be interpreted as principles of coordination. *Cf.* Padovani, *o.c.*

(17) In the sense defined above, in the commentary on the Einstein quote mentioned in note 12.

(18) "The Philosophical Significance of the Principle of Relativity" (1915), in H. L. Mulder and B. F. van de Velde-Schlick (eds.), *Philosophical Papers* (Volume I). Dordrecht: D. Reidel, 1979, pp. 153-189.

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Einstein. Among the principles of coordination, it is therefore the one that simplifies the formulation of (accurate) predictions that should be favoured: as we have already noted, the scheme of representation that appeared to be the simplest may well require excessively complex formulations before arriving at a description of reality.

Of some relevance to our present reconstruction and objectives is further a discussion that took place in the correspondence between Schlick and Reichenbach.[19] The subject was precisely the status of coordination principles. The young Reichenbach would have liked to safeguard a last remnant of the synthetic *a priori.* So, in a letter from 1920 (very early on), Schlick challenged him to make up his mind: should convention (in the sense of Poincaré) be considered as *constitutive* or not? Schlick knew that Reichenbach considered it to be that way,[20] but he anticipated such an answer by a *distinguo* that we may paraphrase here: are the conventions 'constitutive', i.e. constitutive for the object, as you [Reichenbach] would have it? No! Constitutive, in that case, for the concept of the object? Perhaps. But then: the principles that Reichenbach calls "*a priori*" or "synthetic *a priori*" can be said to "constitute" a measurement or an observation. But then, what's the difference between such a principle and a convention? Schlick's version of conventionalism clearly runs counter to any kind of apriorism.

§ 4. — Meanwhile, in France...

1. A passage to reflective analysis. We still have to take a closer look at Carnap's version of conventionalism, since Cavaillès defines his own position in this matter to a large extent *vis-à-vis* Carnap. However, let us first turn to a *maître à penser* who occupied the opposite end of the epistemological spectrum at about the same time: Cavaillès's own mentor, **Léon Brunschvicg**. "Reflective analysis" (*analyse reflexive*) was one of Brunschvicg's preferred titles for the philosophical current he wished to promote. Today, this current is remembered, if at all, more under such names as "French spiritualism", "critical idealism", "spiritualist Kantianism" or even

[19] This correspondence is said to have taken place at Einstein's request. *Cf.* Thomas Oberdan, "Moritz Schlick", *Stanford Encyclopedia of Philosophy* (article revised in 2017). https://plato.stanford.edu/entries/schlick/

[20] This would mean, in the terminology we are using here, and which is also that of Michael Friedman (see note 13), that the principles of coordination are constitutive. See also L. Vanderstraeten, *o.c.*, p. 22).

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"spiritual positivism". In any case, this tradition is well and truly a thing of the past, but traces of it have survived, some of them unexpected.[21] Reflective analysis, moreover, is not a doctrine, but rather a method — the method that corresponds to what has also been called a 'philosophy of consciousness'. To understand what motivated it, we need to go back to the sources of its Kantian and Cartesian inspiration, rather than look for whatever kind of philosophical theory that might have emerged from the movement.

The basis of reflective analysis lies in the idea of the pure activity of consciousness and its expression in *judgement*, not in the thinking subject as substance or even in judgement as a logical entity or faculty, but precisely as the *act* of judging. **Jules Lachelier** (1832-1918) may be considered as the first great exponent of this movement. To anecdotally illustrate of what consisted, for him, the idea of doing philosophy: the story goes, that on Lachelier's desk the *Critique of Pure Reason* was always opened on the page that dealt with the *Ich denke*, and that all his classes began with an appeal to recall the meaning of the *Ich denke*, representing, of course, the pure reflective act. However, while Lachelier's method consisted of analysing this universal starting point and pushing through to the ultimate consequences of what the reflective act implies, this method finally led him to a metaphysics that had to reconcile idealism and realism, mechanism and teleology, freedom and determinism: the real existence of freedom, for instance, being warranted by the nature of that act. For Brunschvicg (Lachelier's successor through Emile Boutroux interposed), much as he sticked to Lachelier's almost Fichtean point of departure, such metaphysical excursions would no longer be conceivable. *Reflective* analysis as a method was neither a preamble to metaphysics, nor an analysis of immediate intuition (as opposed to Bergson's claim), but an epistemological interpretation of what makes possible the purest form of thinking: *mathematical* analysis. The *tertium comparationis* between the two types of analysis is the fact that the mathematical statement is a judgement of relation, just as, for Brunschvicg, any judgement worthy of the title of knowledge is relational — not predicative. To grasp Brunschvicg's meaning here, however, it is imperative to study the "reflective" revolution in mathematical thought that started with Descartes and that defined the movement of modern

[21] Traces of this can be seen, for example, in Xavier Roth's *Georges Canguilhem et l'unité de l'expérience. Juger et agir 1926-1939*. Vrin, 2013; or in Ricoeur, and studies on Ricoeur.

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mathematics in a series of historical shifts and breakthroughs. This history is told in detail in the masterly synthesis of *Les Étapes de la philosophie mathématique* (1912).

Science, however, is mathematical, but does not coincide with mathematics. Although he may be seen as a 'mathematician'-philosopher, Brunschvicg was not a 'mathematist', as some had labelled him after the *Etapes*. In *L'Expérience humaine et la causalité physique* (1922), his second monumental study, Brunschvicg obviously maintained the central place of mathematics in the history of science and reason: it is mathematics that has given us the norm of truth; but this place is no longer exclusive. It is the need to 'know the world' that is at stake (an expression that will be applied to Brunschvicg's 'programme' in Cavaillès's last essay (*LTS* 19)).[22]

It could be said that, in *L'Expérience humaine*, Brunschvicg draws an epistemological moral from an entire history of scientific thought.[23] But he wants to move on to the most recent stage the story and reap its rewards. In *L'Expérience humaine*, in search of the latest lessons of history, he turns to Poincaré and Einstein. In *Les Âges de l'intelligence* (1934), inspired by his somewhat dissident disciple Gaston Bachelard, even Louis de Broglie guides us towards the final, and obviously always provisional, stage of the scientific adventure.

Notwithstanding the radically historicizing tendency of his work, Brunschvicg's contribution to the philosophy of coordination relates above all to the purely epistemological requirement to understand the conditions of possibility of the mathematization of the sciences, starting with physics. Also in this respect, Brunschvicg's theory of scientific knowledge remains close to Kant's. The two initial questions of the *Transcendental Analytics* remain the same: how are pure mathematics and theoretical physics possible? Translated into the system of sciences: from geometry we have to move on to physics through the medium of rational mechanics. It is the link between the two that constitutes the problem of coordination. (And, anticipating the sequel, my suggestion would be: at this precise point, the initial questions of Cavaillès's last essay remain the same, but the answers do not.) Now, in a "philosophy of consciousness" such as Brunschvicg

[22] See also *infra*, note 33.

[23] A history that pays much attention to French thinkers such as Cournot, Comte, Lachelier, Duhem, Poincaré, the Boutroux's, Brusnchvicg's own opponent Meyerson...

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wanted to develop, all these notions should be translatable into the terms of a "reflective analysis".

In order to show that the question of coordination plays a substantial (though little noticed) role in Brunschvicg's work, I'll limit myself to a passage from *Les Âges de l'intelligence* (1934), and a comment on a fragment from *L'Expérience humaine*.[24] So let us first look at a specific example — that of modern geometry in its relation to reality — and see how the "reflective attitude", initially expressed in a language that was anything but technical, could nevertheless be translated into a philosophy of contemporary science equivalent to its subject:

> "The very freedom with which the mind created groups of relations that were specifically geometrical, without however implying any corresponding representation, was interpreted as if in science the mind were simply echoing itself, signing "conventions" only with itself and in the sole interest of its "convenience". Henri Poincaré had no hesitation in predicting that Euclidean geometry, by virtue of its privilege of simplicity (...) "has nothing to fear from new experiments".[25] But this presumption has been belied by the theories of generalised relativity. The mind makes nature partake of the freedom it has regained over space; it provides nature with the means to decide for itself between the types of analytical coordination proposed by the mathematician (...) The Einsteinian system of the world is a *cosmometry* in which analytical coordination is moulded directly on the data of observation, according to the coefficients they provide".[26]

Recently, Pietro Terzi has convincingly shown[27] that already in *L'Expérience humaine* Brunschvicg had come out as an opponent of Poincaré's conventionalism. This, however, Terzi also argues, hardly means that he would dismiss the role of coordination. In

[24] After *Les Étapes de la philosophie mathématique* of 1912 (Paris, Alcan).

[25] Quotation from *La Science et l'hypothèse*, *o.c.*, p. 93.

[26] *Les Âges de l'intelligence*. Alcan, 1934, pp. 117-118.

[27] In his monograph *Rediscovering Léon Brunschvicg's Critical Idealism. Philosophy, History and Science in the Third Republic.* Bloomsbury, 2022; and more explicitly so in his contribution to the conference "The Orientation of Reason: Léon Brunschvicg on Philosophy, Mathematics, and the Sciences", Bristol, October 2023(forthcoming).

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L'Expérience humaine, Brunschvicg had explained in detail — as well as reinterpreted! — Poincaré's contribution (after Klein's) as an unwitting clue to a solution of the problem:

> "The true interpretation of geometry (...) was given by Felix Klein in the *Erlanger Program*. It consists in considering geometry as the study of properties that are invariant with respect to a fundamental group of transformations of the plane or space. It is through Klein's work that Poincaré's thinking (...) has come to its critical momentum: "It is the study of a particular 'group' that constitutes the object of geometry; but the general concept of group pre-exists in our mind, at least potentially. It imposes itself on us, not as the form of our sensibility, but as a form of our understanding."(28) The progress compared to Kant [consists in] having transposed the *a priori* synthesis from the plane of intuition to the plane of the understanding, and it is decisive for the transition to physics."

This transposition finally allowed to break the "insoluble alternative of absolutely absolute space [*i.e.* Newton] and absolutely relative space" [*i.e.* as conceived in a cartesian vein]. Yet the way out of the impasse may sound surprising from the mouth of a (self-proclaimed) 'intellectualist', who states in the closing lines of the passage that both the origin of the problem and its solution have to do with the fact that

> "*we have uprooted space from the coordinating activity*, which man is undoubtedly capable of extending to infinity, but *which has its origin*, its centre of perspective, *in the organism. Space is relative to our body, and relative to that body it is a given.*"(29)

What is remarkable is that 'reflective analysis', after having been translated into an analysis of scientific acts and processes, seems able to transform and re-translate these epistemological themes into considerations heralding phenomenological analyses (in this case of space). After dismissing conventionalism, the notion of

(28) Quotation from *La Science et l'hypothèse*, *o.c.*, p. 90.

(29) *L'Expérience humaine et la causalité physique.* Alcan, 1922, pp. 487-488 (emphasis added).

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coordination was in need of another anchorage; one, perhaps, that could be found in the structure of the human organism. Not only the conception of space but also the activity of coordination is relegated to a perceptual and organic human origin — the coordination of the different senses in the constitution of an experienced world. It seems to represent the subjective or reflective basis of this coordination of the physical with the mathematical via the human body. Clearly, from this perspective, we are moving in the direction of theories of perception and of philosophical and genetic psychology, which interested Brunschvicg. It is no coincidence that Jean Piaget was a pupil of Brunschvicg; and it is no coincidence either that Merleau-Ponty, also a student of Brunschvicg, still referred to the latter (despite sharp criticism of his 'intellectualism') in his last courses devoted to the philosophy of nature.[30]

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2. From Master to disciple. Among Brunschvicg's students, it was certainly not Gaston Bachelard who would have been tormented, in this respect, by scruples about a possible drift towards psychologism. Those were more likely to come from the side of Cavaillès. To extend his horizon beyond "reflective analysis", rather than on philosophical psychology, Cavaillès had drawn on mathematical and philosophical logic. In this sense, it is all the more intriguing that before 1940, we can nevertheless find echoes of such a "genetic" epistemological influence in Cavaillès, and precisely in a context that involves coordination. There are passages which, without taking Brunschvicg's influence on this point into account, would remain rather mysterious. They appear in the conclusion of his main *thèse*,[31] where he contrasts mathematical experience with physical experience. They also appear in "Du Collectif au pari"[32] from 1939. There, he ends by linking his interpretation of the probability judgement as a wager to a rather suggestive characterisation of physical experience. The periphrasis he uses seems rather adapted to an analysis in terms of phenomenological anthropology.

[30] *La Nature. Notes du Cours au Collège de France* (followed by the corresponding Course Summaries). Edited and annotated by Dominique Séglard. Paris, Editions du Seuil, 1995, p. 47-57. Cavaillès, it should be remembered, was Merleau's *agrégé-répétiteur* at École Normale.

[31] *Méthode axiomatique et formalisme*. Hermann, 1938, 1981.

[32] *Revue de métaphysique et de morale* 47(1940), p. 139-163. Translated as "From Collective to Wager" (by Robin Mackay), in *Collapse* 8(2014), 65-106.

> "To know the world[33] is to wager — to wager that certain acts, laboratory experiments or industrial techniques, will succeed (...) It is the law of [vital] interest that is guiding here: to insert oneself into nature, living at the heart of becoming, to invent the movements that will succeed, invention itself being a part of the becoming, an element of a dialogue, like the gestures of the body when climbing. It seems that an explication that is faithful to the physicist's intention would have to follow this line — the cosmogonic intent, the status of description and of knowledge of physical theories apparently not matching either with the use of probabilities, nor indeed with other features of modern physics (for example, Einstein's analyses of space and time). *The mathematical elaboration of theories can be said to represent a systematic coordination of effective gestures, of processes of retardation, of ameliorations of action which, following the observations of psychologists, can take* [*themselves*] *as an aim, forgetting the finality which gives* [*them*] *sense.*[34] But even the most refined theories — lacking practical value in the usual sense — can only find their justification in an effectively realised act, in the agreement observed between two measurement results. Physical experience is an event in history; its prognosis, a wager; its success, the possibility of novel acts."[35]

This half-psychological characterisation — once is not a custom — is justified by the distance between what is intrinsically mathematical and that whole "complex of many heterogeneous elements"[36] that make up what is called physical experience. Cavaillès insists on this point; this distance explains the otherwise remarkable fact that in this case such an anthropological or psychological approach

[33] This expression reappears in *LTS* (as already indicated in the text to note 22): *LTS* 19, in a short passage which would require an *ad litteram* commentary; a passage, in fact, about Brunschvicg.

[34] (My emphasis.) It is probable that in making this observation, Cavaillès had in mind Janet and his "psychology of tendencies", which he had mentioned in his article "Education morale et laïcité", in *Foi et vie* n°. 20(1928), 1 December 1928, pp. 1166-1175.

[35] "Du Collectif au pari", *o.c.*, pp. 160-161 (emphasis added).

[36] "La Pensée mathématique", *Bulletin de la Société française de philosophie* 40(1946) n°1, p. 1-39, p. 9. Translated as "Mathematical Thought" (by Robin Mackay); `https://www.urbanomic.com/document/mathematical-thought/`

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does not fall under his prohibitions. All the same, the problem of a coordination between these two types of experience is emerging on the horizon. However, it seems possible to be even more puristic about mathematical experience and therefore, retroactively, even more severe about physical experience — as in this passage from the Conclusion of *Méthode axiomatique et formalisme*:

> "... separation between authentic experience, which is knowledge, and can therefore only be the experience which governs mathematics, and experience in the ordinary sense, or physical experience. The latter is a superposition of heterogeneous elements. Without claiming to analyse these elements, we can limit ourselves, in order to avoid any confusion, to pointing one thing out: assuming there is nothing to think in physics other than the mathematics it contains,[37] the technical intention — in the sociological sense: the affirmation of human life in the world, or the fact of bringing man (...) face to face with things — intervenes to halt the normal dialectical process, to fragment or coordinate diverse experiences at their first stage (the one that is privileged for the living being) (...) In this tangle the notion of pure experience or consciousness disappears. As for the application of mathematics to 'reality', *i.e.* to the system of vital interactions between man and things, it is clear (...) that it no longer concerns the problem of the foundations of mathematics: the child in front of his abacus[38] is a mathematician, and everything he can do there is mathematical; but the order followed, as well as the link with other experiences, can be directed by a technical intention of a primarily negative role: *putting an end to the deepening of consciousness demanded by each experience separately*".[39]

[37] An allusion to Kant's famous formula: "There is no science except the mathematics that lies within it".

[38] An allusion to the criticism levelled by Abraham Fraenkel (*Einleitung in die Mengenlehre*. Berlin, Springer, 3e ed.,1928, p. 383) at formalism, which purportedly cannot justify why arithmetical laws apply to reality; a remark quoted by Cavaillès in the same Conclusion, p. 168.

[39] *Méthode axiomatique*, *o.c.*, pp. 179-180 (emphasis added). "Claimed by each experience separately" seems to evoke the idea that, as mathematics, each experience requires an elaboration of what, purely mathematically, it implies, hence

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Two things stand out here. First, the fact that here Cavaillès has not yet left the framework of a 'philosophy of consciousness'; so, even for mathematics, the 'philosophy of the concept' has not yet taken the lead. Second, the impression is left that, in this respect, Cavaillès was at risk of falling prey to supererogation: he seemed ready to drive the master's urge for 'mathematism' to the limit — or rather, ready to qualify for that label that, as we saw, did not fit the master. What I want to suggest, is that Cavaillès, by following this path, could find himself confronted with a prospect that would present itself as a failure: the prospect of having to conclude that physics is impossible as a science; that is to say, of arriving at a negative answer to the questioning that he acknowledged as his own: "I am trying, by means of Mathematics, to understand what it means to know, to think; that is basically (...) Kant's problem".[40] But, of course, Kant's problem concerned physics as much as pure mathematics. This is the dilemma: it is hard to see whether all this will not lead to posit conditions of impossibility rather than conditions of possibility for theoretical physics.

Faced with possible failure concerning an inescapable question, and faced with the criticism that such a stance had provoked from his friend **Albert Lautman**,[41] as well as from at least one other speaker (the renowned mathematician Maurice Fréchet)[42] at the Société française de philosophie in February 1939, it was better not to take a stance that would render the cognitive ambitions of the 'mundane' sciences futile... But there was more: there was the polemical debate with **Ferdinand Gonseth** at the Entretiens

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requires an extension that remains within the sphere of mathematical questions it raises. In this sense, too, subsequent mathematical conceptualisations in the course of "logical time" — would represent "deepenings of reflection"; "deepening" will still be mentioned on the final page of *LTS*, no longer, however, associated with the term "reflection". It should also be noted that the expression "deepening of reflection" appeared literally 1° in Cavaillès's review of *Les Âges* (*o.c.*, p. 406) in 1935 — that is to say, as a characterisation of the master's thought; 2° in the Discussion, above all, with Gonseth in the *Entretiens d'Amersfoort*. Actualités Industrielles et Scientifiques no. 850, Paris, 1939, p. 42 (see below, this §).

[40] "La Pensée mathématique", *o.c.*, p. 34.

[41] Within the scope of this essay, I cannot cover this discussion. What is clear, is that Lautman saw in his own version of Platonism an answer to the main questions about the foundations of mathematics, including the relation between mathematics and physics.

[42] Maurice Fréchet (1878-1973, one of the founders of the theory of abstract spaces) was clearly opposed to the idea of the autonomy of mathematical development granted by Cavaillès in relation to the development of physics.

d'Amersfoort.[43] On that occasion, Cavaillès addressed Gonseth's "idoneist" thesis, which seemed to call for a unitary idea of scientific truth — unifying mathematical truth and physical truth in a vision where the totality of knowledge is subject to a set of criteria that express a dialectic between *experimental objectivity* (relation to the external world), *axiomatic structure*, and *intuitive content*. In short, Gonseth had his own solution to the problem of coordination — and it is no coincidence that his favourite examples are geometrical. Cavaillès agrees with the idea that all *a priori* claims should be subject to experiential control and to the demands of rational evidence. But he returns to the question of the specificity of mathematical experience (and the autonomy of mathematical knowledge). He challenges the idea that physical experience and mathematical experience can be homogenised under the same concept — that of truth as a submission to facts, to 'the real'. In mathematical experience, there is indeed an "apprehension of an unforeseeable result", which is the basis of objectivity, as in physical experience. But in mathematics this apprehension "occurs in the encounter of regulated gestures whose accomplishment, because regulated, is not an event" (*Entretiens*, *o.c.*, p. 42); whereas in the case of physical experience, we find that familiar character of

> "[being] a *sui generis way* of apprehending an event, and however difficult it may be to formulate it exactly, of *pointing* towards something that is not thought (...) The activity of physicists only takes on its full meaning when extended by that of the engineer: here, what is true is, in the end, what 'works"' (*ibid*.; emphasis in the original).

As for the divergence between the "two experiences", Cavaillès, taking advantage of his familiarity with the discussions within the Vienna School, in particular their dispute with Popper,[44] adds an "essential difference":

[43] *O.c.*, p. 40-48. The conference (held in September 1938, and interrupted because of the mobilisation) can be seen as an important stage in the attempt to found a movement in the philosophy of science that would constitute an alternative to the Vienna School by rejecting both empiricism or radical positivism and logicism. In this respect, this meeting can be seen as the cradle of the journal *Dialectica*, finally to be founded in 1947 by Ferdinand Gonseth, Gaston Bachelard and Paul Bernays.

[44] And, no doubt, with the complications associated with the process of falsification, which had already been raised by Pierre Duhem.

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> "Physical experience is *negative*, it can only invalidate a theory *en bloc* without saying anything about its content. Mathematical experience is *positive*, it is the very fulfilment of the theory." (*ibid.*, p. 43).

This is followed by a discreet remark, almost in parentheses, which nevertheless cannot leave us indifferent. Not only does it seem to reflect a 'Viennese' influence, but it also recalls and reinforces a clear reservation about psychology that we had noticed before:

> "Both [kinds of experience] stem from sensuous intuitive activity, and they each represent (...) the point of arrival of one of two diametrically opposed developments. The description of these developments (...) seems to me to belong more to general anthropology than to epistemology" (*ibid.*).

All in all, in the heat of the discussion, Cavaillès had pushed to the limit the duality between mathematical experience and physical experience in their relationship to knowledge and truth:

> "I don't think it's possible to unite mathematical experience and physical experience under one and the same concept. There is an autonomous mathematical knowledge which is self-sufficient and therefore requires an idea of truth unrelated to physical truth." (*ibid.*, p. 41)

But he already must have sensed that such a radical contrast would put him in difficulty, particularly "because there is nothing in the physicist's activity — including measurements and the use of apparatus — that can be noted as extra-mathematical" (*ibid.*, 41-42). This concession led him to formulate what appears to be his first explicit attempt to pose the problem of coordination and, at the same time, to sketch out the direction in which an answer should be sought:

> "... on the one hand, the majority of mathematical disciplines are authentic (…) activities of the scientist [which remain] (...) external to any physical activity; on the other hand, the latter, insofar as it coordinates diverse mathematical operations — borrowed from mathematically independent theories — is oriented entirely by its *sui generis* character of apprehension of an event" (*ibid.*).

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Note that here Cavaillès seems to be formulating a type of coordination problem of his own. This coordination concerns different mathematical theories — or their constitutive operations — that need to converge or at least co-operate (in combination with other, for example experimental, elements) within one specific physical theory: this question is certainly not identical to the problem as defined by Reichenbach, Schlick or Carnap, i.e. how to fill a formal system with content (*cf. infra*, § 5.2). At the same time, it also differs from the idea of coordination between different sensory orders (Schlick) or the organic origin of the notion of space (Brunschvicg). In any case, the subsequent wording of 1942 (§§ 5, 6) will go far beyond the formulas spoken in 1938, which constitutes something like its preliminary.

Be that as it may, the problem of coordination is now an urgent one: if the idea of truth is not "given in a determination prior to a given body of acquired knowledge", as Gonseth himself had stressed, that idea becomes, says Cavaillès, "relativ[e] to the system of acts of the researcher in a given discipline". But this must "entail for it an essential polymorphism". [(45)] But that cannot be the last word in the matter — otherwise physical knowledge, once again, threatens to evaporate. The urgency of this problem for Cavaillès therefore seems to be linked to the fact that he finds himself in a defensive position — after having manoeuvred himself, notably by attacking Gonseth — in that position.

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§ 5. — A problem in the 'doctrine of science'.

1. The decisive text. Let us now look at how the question of coordination arises, at first sight almost in the margin of the complex reasoning of the second part of *LTS*. Here the iconic figure of the "adversary" is Carnap. After setting out his own fundamental concepts for understanding the historical-logical development of the formal sciences — paradigm and thematization; posited meaning (*sens posé*) and positing meaning (*sens posant*) (*LTS* 27-33)[(46)] — Cavaillès undertakes a critical examination of the "*two* [major]

[(45)] *Ibid*, p. 41.

[(46)] For an analysis of these notions, see, among others, the works mentioned at the end of note 2.

difficulties" that he sees arising for this variant of what, throughout the essay, he calls "logicism". First, an examination of the difficulties associated, in his view, with the attempt to found *mathematics* — along with logic itself — through radical formalisation. Next, Cavaillès poses a more or less analogous question concerning *physics* (*LTS* 40ff): can physical epistemology be founded on logicism, taking the latter in its Viennese version? In sum, for the exemplary case of Carnap, the critical question arises: can mathematical and physical epistemology be constituted by means of an in-depth formalisation such as the one *The Logical Syntax of Language* achieved in principle?

For the "first difficulty", the relation between *logic and mathematics*, we find these few pages (*LTS* 36-39) where the otherwise already legendary density of the text is pushed to its climax — pages requiring a literal commentary that cannot occupy us here. (In any case, the analytic commentary of those pages is only indirectly related to our problem.) To give a brief idea, however, it is first of all the idea of logical-mathematical systems conceived as formal languages with their respective syntaxes, as deployed in the *Logical Syntax*, that is put to the test of criticism. Among other things, Cavaillès targets the idea of the self-sufficiency of the syntactic approach, as well as Carnap's pronounced conventionalism, expressed in the famous "principle of tolerance" of syntax (*infra*, 5.3). To restore a balance with the latter principle, Cavaillès seems to suggest, the idea is invoked of a *general* syntax. The latter, again according to Cavaillès, is characterized by the viewpoint of a total abstraction of content, resulting in a mathematics that would be nothing more than a set of analytical statements (in a sense to be elucidated, *cf.* §5.3). This idea is then subjected to an equally profound and intransigent critique.

All this is decisive if we are to grasp the essence of Cavaillès's ultimate reflection on mathematics and logic. But what we are concerned with now is "the second difficulty of logicism" (*LTS* 40): that which concerns the relationship between such a logicized mathematics and *physics*. It wouldn't be surprising to read a critique of this logicist conception as implying the image of a separation between form and matter[47] (in this case: empirical content) of knowledge: such a conception would simply be an extension of the tautological or purely

[47] This criticism was already present in the explanation of the "first difficulty", which obviously did not deal with empirical content. It is, moreover, in the sense of this criticism of a radical separation between form and content, that Cavaillès

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analytical view of mathematics. But the criticism is more subtle, and in fact marks a new element in the expression of Cavaillès's thought: nowhere else in his published texts do we find in such explicit form this idea concerning the need to found physical thought. But even here, in pages that are in turn hyper-condensed (*LTS* 40-43), the expression of this thought will not be found in the elaborate form that it would demand — and this is no doubt another reason why in the commentaries this point is so often passed over in silence.

Here, then, is this fragment — my 'decisive text' — which we will have to analyse more closely later on:

> "*... the second difficulty of logicism, the problem posed by physics. For the notion of co-ordination is no more directly usable than that of description, but on the contrary presupposes it: one only coordinates things which, in the same sense, are part of a superior whole;*[48] *there is no coordination from the physical to the mathematical until after a mathematisation of the physical, that is to say, a descriptive work that logicism is powerless to define. The "protocol statements" invented by its naive realism presuppose what is at issue, namely mathematical relations that would be a translation or reduction of physical experience. But the physical concatenation has no absolute beginning, no more than the mathematical concatenation: on the one hand, as mathematical relations, its statements only take on their meaning in a system that has already been posited and that already possesses, in a more or less precise way, an experimental meaning; on the other hand,* [physical] *experience itself as a system of acts is internally organised in such a way that it is impossible to interrupt its unfolding, except by way of a superficial abstraction: experimental acts give rise to new ones by way of a* sui generis *concatenation which, at least as such, is independent — because it is of a different essence — from the mathematical concatenation. At the crossings of these two processes the physical relationship emerges; from their more or less complete coordination arises the physical theory, whose place and meaning it is up to the doctrine of science to determine*" (*LTS* 40).

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seems to use the term "logicism" in *LTS*, thus extending the application of the term to (most) formalists.

[48] Compare with Reichenbach's comment in the quoted passage corresponding to note 14.

How could these compact and lucid lines be dismissed by the commentators or at least treated as if they were of minor interest? Is it perhaps because their significance extends beyond the framework of what can be developed in a pure philosophy of mathematics, the framework of "mathematical experience" in which it was considered possible to enclose their author's projects? Despite the pages (indeed, the most precious pages: *LST* 27-33) on the subject of the *paradigm* and *thematization*, let us not forget that, in 1942, as Cavaillès had written the previous year,[49] the "mathematical experience" project was undoubtedly still asleep — at least as an independent project designed for the short-term.[50] In any case, it is already important to clear up the misunderstanding that the philosophy of physics was of little interest to Cavaillès — and it is implausible to read the lines just quoted as stating that the issues of coordination and of description would be based on muddled thinking. Insofar as the posthumous title of the book proves adequate to the themes addressed — logic and theory of science — it is at least necessary to provide an answer (which, of course, had to remain programmatic in this case) to the question of the status of mathematical physics, without forgetting the status of experimental science; which, in the context of the topicality of the epistemology developed in the first half of the century, presupposes an answer to the questions that persist: in particular that of coordination.

Cavaillès takes as his starting point the answers given by logical empiricism. We have already seen how the problem was

[49] Letter to Brunschvicg, 1941, published in G. Ferrières, *Jean Cavaillès. Un philosophe dans la guerre.* Le Seuil, 2ème edition, 1982, p.158 (undated but apparently, given the context in this biography, written in 1941): "L'expérience mathématique dort..."; the sentence continues as follows: "I am only half angry about it. Previously I would have liked to attempt an old quarrel against transcendental logic, especially that of Husserl (...) In the *Krisis there is* a rather exorbitant use of the *cogito*". It is clear that the "attempt" he is anticipating here refers to *LTS*. The reference to the *Krisis* is not without relevance, since in addition to *Formal and Transcendental Logic*, it is also the Husserl of the *Krisis* who will return in *S:* 54, 57, 66-69, 76-77; and this precisely concerning the relation between mathematics and physics.

[50] Another possible interpretation is that, in the urgency and uncertainty of the situation, Cavaillès finally attempted to merge the two projects ("mathematical experience" and "theory of science") into a single essay.

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approached by Schlick and Reichenbach. But it is above all in relation to Carnap that Cavaillès defines himself.(51) The first step is therefore to refer to Cavaillès's reception of Carnap. This concerns the aspect of *Logical Syntax that* relates to physical epistemology. Without going into the details of Carnap's syntactical project in relation to the empirical sciences, we shall see how Cavaillès understood the decisive idea and reacted to it.

2. Carnap's conventionalism (logical empiricism, 2nd stage). For **Rudolf Carnap**, the question of coordination inevitably plays a crucial role in his physical epistemology and his philosophy of unitary science (*Einheitswissenschaft*), since in his conception it is imperative to bridge the gap between logic (analytical statements) and substantive science (synthetic statements). And this already for the simple reason that the two were initially radically separated. Now, Carnap had implemented the notion of description — the very notion we just encountered in the passage (*LTS* 40) — thus, one might say, injecting a pseudo-semantics into the project of syntax. This he did by introducing the notion of descriptive syntax, next to that of pure syntax. The aim was to build a bridge between pure syntax and the discursive fragments which — after eliminating statements in the "material" mode(52) — were inevitably and formally used in scientific treatises. But for Carnap, it was also and above all a first step towards resolving the question of the applicability of logic to the content of experience; without this applicability, logic would hardly be distinguishable from a formal game.(53)

That is a step towards the desired goal, since once we have defined the *descriptive* syntax for a formal language, we can see the analogy with the relationship between *geometry and physics*. In fact, the transition from abstract form to synthetic content takes place

(51) Letter dated 4/11/1942 to Lautman from the Saint-Paul d'Eyjaux camp (Ferrières, *o.c.*, p. 164): "... your offer [to bring] books is admirable (...) Do you also have our old enemy, the *Logische Syntax der Sprache*?" (my transl.). It is unlikely that Lautman would have been able to meet the request for Carnap (and several other technical works), given the lack of quotations in *S*: there are many from Kant, especially Husserl, but not from Carnap, nor from Brouwer, Brunschvicg, Leibniz, Bolzano...

(52) The mode, according to Carnap, in which language functions fluently according to the rules dictating *use* in a descriptive and referential sense — as in "Fido is a dog" — as opposed to the "formal mode" in which words are mentioned rather than used ("'Fido' is a dog's name").

(53) The comparison with the game of chess can also be found in Carnap (and not only in Husserl). See also Fraenkel's objection to Carnap *c.s.* (note 38).

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in the same direction in the case of geometry: for Carnap, there is first an "arithmetical" geometry, i.e. a purely formal geometry whose symbols and statements can be arithmetised according to the Gödelian procedure, which is amply demonstrated in the entire work.[54] Then, there is a *first descriptive* geometry, *i.e.* a formal and axiomatic theory, which starts from primitive concepts such as "point" and "line" the sole interpretation of which is given by axioms. And finally, there is *physical descriptive* geometry, which determines which symbols of the physical language correspond to which symbols of the axiomatic system. For this purpose, there are the *Zuordungsdefinitionen* or "coordinative definitions". At the same time, there is the same development in syntax: after pure, arithmetised syntax, there are two consecutive forms of descriptive syntax: axiomatic syntax and physical syntax! Since it seems possible to develop the two descriptive geometries, it follows that we can also deploy two descriptive syntaxes: as a standard example belonging to physical geometry is given "a physical segment is said to have length 1 if it is congruent with the segment between two marks made on the gauge in Paris";[55] as a parallel example in physical syntax: "a symbol consisting of two horizontal marks is found at place *c* in this book".[56] It's easy to see where we are heading: if the transition from analytic formulae to synthetic formulae is possible and justified in one case by means of coordination rules or definitions, it is also possible and justified in the other case. And we will have "justified" physics as a science that deals with content without really leaving syntactic territory.

Not surprisingly, it is the famous notion of *Protokollsatz* that is announced here. Having decided to abandon the language of the *Logische Aufbau*'s 'Erlebnisse' and to replace it with the physicalist language[57] as the basis of the epistemology of empirical knowledge — a language, moreover, in which the notion of description is used in a highly idiosyncratic sense — it was only a short step

[54] *Cf.* also *LTS* 35, about Carnap: "it is a result obtained in the effective act of formalism that any system containing arithmetic can formalise its own syntax".

[55] *Logical Syntax*, *o.c.*, p. 80.

[56] *Ibid*, p. 81.

[57] In order, of course, to avoid any metaphysical drift, in this case probably a 'constructivist' one; just as Carnap and Neurath also strongly opposed Schlick's so-called 'realist' proposal to replace protocols by *Konstatierungen* (see §3). For Neurath and Carnap, protocols and the other statements of the unitary language were nothing more than sequences of signs (acoustic or graphic), their relations syntactic relations, and their truth, coherence with other sequences of signs.

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to the notion of protocols. Complete the sentence "symbol consisting of two marks..." above, and you get the particular form that sentences expressing scientific observation will have to acquire according to Neurath and Carnap: "observer *O* noted a symbol consisting of two horizontal marks found at place *c* in this book", or in the example formulated by Cavaillès: "the sentence materially written in an astronomer's notebook 'at such and such an hour such and such a star passes at the zenith"'(*LTS* 41). What is to be made of the trick Carnap is playing here? Could it be that sentences like these should form the observational basis of natural science? That it is statements like these that should be coordinated by rules and definitions with the set of physical statements and ultimately with the entire language of science, including logical languages? According to Cavaillès, if the idea of coordination ends up like this, it is because it has fallen into the "absurd" (*LTS* 41): such a statement

> "has no relation to a physical proposition, for the simple reason that even if the book is designated as a specific copy situated in space and time, the other elements involved (line, statement, composition) are cultural objects that no physical experience can claim to reach. It is just as absurd to 'coordinate' this with the sentence physically written in an astronomer's notebook..." (*ibid.*)

In 1935, Cavaillès published a fairly detailed report on the intervention of the new school at the 8th World Congress of Philosophy in Prague (1934).[58] He made only a discreet allusion to the question of protocol statements,[59] but he did already discuss the coordination between different types of statements which should guarantee the unity of the linguistic edifice of science and in particular the applicability of logical languages to physics; a process in which we can clearly see the place given to protocol statements. After presenting the *Logical Syntax* project as a whole, he makes special mention of a "particularly vigorous critique" by Roman Ingarden, the phenomenologist and disciple of Husserl:

> "If we admit that all propositions are either tautological or physical (...) metalogical propositions can only

[58] "The Vienna School at the Prague Congress", *o.c.*, pp. 137-149. The congress took place in September, *Logische Syntax* had been published (in Prague) in March.
[59] *O.c.*, p. 145f.

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> be nonsense or counter-sense[60] since their coordination with stains on paper or sound waves has nothing to do with what they claim to say: "We must therefore distinguish between the verifiability of a proposition and its meaning (...) the meaning of a proposition has nothing physical about it". ("L'École de Vienne au Congrès de Prague", p. 145).

It seems that Cavaillès took these remarks on board: seven years later, he expressed himself in a spirit very similar to that of Ingarden on this point. In his 1935 essay, he had continued:

> "In fact, according to Carnap, we know that metalogical propositions are coordinated with arithmetical or physical propositions; *perhaps what remains to be done is to specify more precisely the meaning of this notion of coordination* (which is not a translation since, before it, there is no syntactic proposition),[61] a notion that also comes into play in relating physical propositions to the concrete experienced"[62] (*ibid*., emphasis added).

Here, Cavaillès was expressing himself much more cautiously than later, when he would be forced to clarify his point of view unequivocally; nevertheless, we can already sense the direction in which he was heading.

3. The principle of tolerance. Regarding Carnap and his reception in *LTS*, one important aspect remained to be clarified: the role played by the principle of tolerance. The principle is paraphrased[63] by Cavaillès as follows: "In logic there is no canon but

[60] An allusion to the distinction made in Husserl's logic between *Unsinn* and *Widersinn*.

[61] It seems to me that this should be interpreted in the sense of Reichenbach's passage: "... the coordination established in a physical proposition is very peculiar (...) For this purpose, *the elements of each set must be defined*" (quoted above (text corresponding to note 14)).

[62] Indeed, Carnap repeated on several occasions that his analyses should not be applied exclusively to the language of science, but also to the language of 'common sense'.

[63] The only textual echo of Carnap's wording of the "principle of tolerance" (as enunciated in *LTS* 33-34) is not textually correct, as the editors (Canguilhem and Ehresmann) pointed out as early as 1947: the formula in question was probably written from memory after a sentence that Cavaillès quoted in "L'École de Vienne

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only unlimited possibility of choice among canons".[64] Whereas, for example, intuitionists introduce their criteria and principles in the form of prohibitions, for Carnap the choice between formal languages, as they are used both in mathematics and in science, is conventional. There is no place for logical principles of a 'metaphysical' nature; there are only rules of the game in *calculi*. Applied to our general question, this means that the rules of coordination are choices of conventions — of definitions (*Zuordnungsdefinitionen*). Of course, they will always be choices imposed by the usefulness of a certain language in relation to a certain goal (like the choice for a language called by Carnap "indefinite",[65] for example if we want to reconstruct classical analysis)[66] — so pragmatic value plays a certain role. The nuance between convention and convenience (and the ensuing discussion about different types of simplicity), already mentioned in connection with Poincaré, reappears. However, here the focus is on the idea that all languages, insofar as their syntaxes are formalised, are equivalent in principle. But in this case, what about the proclaimed aim of constructing a unitary language of science? In 1935, Cavaillès did not seem to make much of this question.

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au Congrès de Prague" (*Revue de métaphysique et de morale* 42(1935), p. 142 *cf. below*). There, being in possession of the book in question, he had stuck to a literal translation: "en logique, il n'y a pas de morale : chacun peut construire sa forme de langage comme il l'entend" (p. 142). (Here is Carnap's exact formulation: "In der Logik gibt es keine Moral. Jeder mag seine Logik, d. h. seine Sprachform, aufbauen wie er will. Nur muß er, wenn er mit uns diskutieren will, deutlich angeben, wie er es machen will, syntaktische Bestimmungen geben anstatt philosophischer Erörterungen"). On the other hand, note that the expression "unlimited possibility of choice among the canons" (*S* 34) is probably a reference, from memory, to an image found in Carnap's Prologue, see *The Logical Syntax of Language* (trans. Amethe Smeaton, Kegan Paul 1937, Routledge 2000), p. xv: "The first attempts to cast the ship of logic off from the *terra firma* of the classical forms were certainly bold ones (...) But they were hampered by the striving after 'correctness'. Now, however, that impediment has been overcome, and before us lies the boundless ocean of unlimited possibilities."

[64] The terminology of "canons" used here should come as no surprise: it refers to an unexplained aspect of the general plan of *S*: the tension between two traditional conceptions of logic (at least since Descartes and Port-Royal, and more explicitly in Kant) (see *S* 11, 13, 14, 17, 18): that of a *canon* opposed to that of an *organon*.

[65] Called by Carnap "*die indefinite Sprache II*", it arises from an extension of the expressive capacities of the more restricted "*definite Sprache I*" (let's say intuitionist) by admitting quantifiers with unlimited scope.

[66] And then if we want to reach physical theory, insofar as the latter needs a language capable of expressing the entirety of this classical analysis.

On the contrary, he seemed to appreciate the technical solutions provided by Carnap:

> "There is no simple juxtaposition between languages. Two particularly important notions come into play here (...): 'partial language' and 'translation'. S_2 is said to be a partial language of S_1 if : 1° any proposition of S_2 is a proposition of S_1 ; 2° any relation of entailment between classes of S_2 is preserved in S_1 . Similarly, an L-application (univocal or bi-univocal) from one language to another is a mapping that preserves the relation of entailment between classes of propositions. S_1 is said to be translatable into S_3 if there is an L-application of S_1 to a partial language S of S_3 . (...) Consequently, the principle of unity that seemed to be held in check by the principle of tolerance can be satisfied if all the different languages are translatable into a universal language. This universal language will be the physical language."(67)

In short, we can say that it is the notion of translation — along with that of hierarchy — that saved the global perspective. "There is indeed a single language, but it subsumes a hierarchy of languages with diverse syntaxes, responding to the needs of specialist scientific researches" (*ibid.*).

At first sight, there was no major change in Cavaillès's attitude to the principle of tolerance in 1942:

> "... "unlimited possibility of choice" (...) — hence the solution to the problem of the relationship with mathematics and physics(68) : mathematics being the whole set of formal systems, while physics would be a certain privileged system thanks to the principle of choice constituted by experience. *Coordination occurs between formal relations and sensible phenomena*" (*LTS* 34; emphasis added).

(67) "L'École de Vienne...", *o.c.*, pp. 142-143. (My transl.)

(68) One wonders whether Cavaillès did not mean to speak here of the relationship *between* mathematics and physics (some such questions have to do with the impossibility of checking against the *LTS* manuscript!).); but the two readings are compatible: it may indeed be a question of the relationship between mathematics and physics (as we shall see immediately), but at the same time what is intended here is the relationship between logic (or metalogic as the study of the syntaxes of formal languages) and, on the other hand, these formal systems themselves — starting with those that make up mathematics, and then taking those that, among the first, are chosen to represent physics.

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In the "ocean of unlimited possibilities" everything seems calm at this level; and Cavaillès goes on — but suddenly in a much more speculative mood — to discuss this coordination, stating that it is achieved "by means of an evidence that belongs to the other distinguished kind of demonstration, the demonstration that adheres to the demonstrated: *physical theory being the one theory which is formally determined in this way*" (emphasis added). At first sight, everything seems to be settled — except that it remains extremely difficult to grasp what Cavaillès might have been thinking when he wrote the last lines quoted — and therefore how the problem mentioned would have been settled.(69)

However, a few pages later (*LTS* 40), as we have seen, it is said that this is where the real problem lies (rather than immediately in the question of plurality as such). In this case, it is not logical pluralism as such that poses the problem, but rather the conventionalist thesis in the framework of which it features, and which presented itself as a solution to the problem of coordination. The plurality of formal languages is a phenomenon created in the act of formalisation, which must not be mistaken for the entire reality of mathematical acts themselves. Only when the latter confusion is in fact made, the decisive characteristic which Cavaillès calls "the necessity of concatenations" is lost: that moment of the becoming which is beyond choice and which, for him, defines the specificity of the development of mathematics. In 1935, quoting Carnap's reply, Cavaillès had been content to describe the state of affairs within logical theory *anno* 1934, as the goal elaborated and affirmed in the project of logical syntax. In 1942, the goal itself is rejected. Carnap's conventionalist response was an illusory solution.

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§ 6. — A new perspective on an old question?

1. Conceptual time and historical time. We are now familiar with Cavaillès's criticisms of the main players in our subject. But where might a positive response lie? The text of *LTS* contains unexploited indications, which we discover by relating it to an unexpected source: the Course taught by Cavaillès at the Sorbonne in the spring of 1941. Our approach will be determined by the question of what

(69) I will not venture here into the literal commentary that would be required in order to decode these lines.

in phenomenology would be called specific modalities of intentionality on the part of the mathematician and the physicist respectively. The stance of each towards the event in history is different; if we call these stances types of (temporal) intentionality, the suggestion is that if there is a problem of coordination, it concerns first and foremost the coordination between these two intentionalities.

Let us start again with our 'decisive text' (*LTS* 40), this time marked in relation to a passage that immediately precedes it. Mathematical concatenation knows no absolute beginning, it was said, referring to the Carnapian illusion of founding mathematics on its syntax alone. Cavaillès concluded that such an attempt was tantamount to postulating an absolute beginning of mathematical intelligibility, the "creation *ex nihilo* of an intelligible universe" (*LTS* 38), point zero of the birth of the logical, its basis being identified, in the end, with the sign (*LTS* 39). But such imaginings are unacceptable:

> "The sign is not an object of the world, but although it does not refer to something else of which it would be the representative, it does refer to the acts that utilise it, indefinite regression being of the essence here".

Mathematical practice knows no starting point outside the totality of acts that are always already mathematical,

> "the fundamental character of the mathematical symbol — digit, figure, even stick — [being] that it is present only as an integral part or basis of application of an activity that is already mathematical: the symbol is internal to the act, it can neither be its point of departure nor its authentic outcome (which is the engendering of other acts) (...) [and what logicism][(70)] takes for an absolute beginning is only a surreptitious evocation of earlier acts and concatenations" (*LTS* 38-39).

I have no intention here of going into the complexities of the relationship between syntax and semantics in mathematics, as they are so briefly described by Cavaillès. I merely recall the fact that a similar character — of resisting any attempt to find an absolute beginning — is attributed to *physical* concatenations:

[(70)] In this stage of his development, which can also be described as radically formalist, he has been able to develop his own style.

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> "*on the one hand, as mathematical relations,* [*physical*] *statements only take on their meaning in a system that has already been posited and that already possesses, in a more or less precise way, an experimental meaning; on the other hand,* [*physical*] *experience itself as a system of acts is internally organised in such a way that it is impossible to interrupt its unfolding*" (*LTS* 40; already quoted (§5.1)).

The same denunciation of the illusion of the "absolute departure"; and this even though the nature and the unfolding of physical experience — the physical concatenations — are essentially heterogeneous to the development within pure mathematics:

> "*experimental acts give rise to new ones by way of a* sui generis *concatenation which* (...) *is independent — because it is of a different essence — from the mathematical concatenation*" (*ibid.*).; but moreover, "[t]he real experimental process is (...) in the plans, uses, and actual constructions of instruments, the entire cosmico-technical system where its meaning is revealed and whose unity as well as its relation with the autonomous mathematical unfolding pose the fundamental problem of physical epistemology" (*LTS* 41).

So far for the 'decisive text', written during captivity in 1942 — now to the Sorbonne, back in 1941.[71] If we may already be tempted to use the term "historical" — in a sense yet to be defined — to evoke the "dialectical" character of mathematics in its development, [72] we would have to apply that term *a fortiori* to the epistemology of physics. Whereas mathematical concatenations seem to display an internal organisation in their development over time, in a sequence of conceptualisations without beginning or end, developments in physics are constrained by external controls. Physical knowledge, assuming that there is such a thing, is entirely situated in history — if "history" means what comes from the world outside such an internal dialectic. Its development takes place in the world of events; so, it is associated with the theme of "necessity

[71] Since no lecture notes by Cavaillès have been preserved, I quote from the only source currently available: the notes taken by Mme Louise Gouhier-Dufour. I was able to revise them with Mme Gouhier in 1989.

[72] With all the reservations we know ("There is nothing so unhistorical as the history of mathematics", *Méthode axiomatique*, *o.c.*, p. 176).

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and contingency". To begin with, the object targeted by physical knowledge is external to the concepts and theories used to target it: in the Course on "Causality, Necessity, Probability",[73] for example, we find statements like these, which contrast the terms [mathematical] "thought" and [physical] "positing" or "apprehension" of objects/reality/event:[74]

> "positing a movement that would be entirely explained would lead to its negation, even to the denial of its reality" (Notes Gouhier, p. 11).

This assertion is in line with the response given to Gonseth's comments at the Entretiens d'Amersfoort in 1938: physical experience was attributed a

> "*sui generis way* of apprehending an event, and however difficult it may be to formulate it exactly, of *pointing* to something that is not thought (...): the world in which we live and where something is happening, the world of animals, of industry and of history" (*Entretiens*, *o.c.*, p. 42).

The Course contains recurrent remarks about the contrast between conceptual thinking and the affirmation or "positing" of reality, often in the context of a discussion of Kantian epistemology:

> "How to coordinate reality to the thought of reality?" (Notes, p. 12); "the relativity of movement (...) does not eliminate the reality of movement (...) the reality of movement as an event — apprehended as an event in an experience" (*ibid.*).

The introduction to the themes and authors studied in this and a parallel course (on Logic and the philosophy of logic) already contained crucial remarks:

> "The physicist thinks thanks to a mathematical system. What is this thought outside these systems? (...) physical experience — for example, thought about the fall

[73] Logique et philosophie générale, Sorbonne 1941, (unpublished) notes written by Marie-Louise Gouhier-Dufour.

[74] Often they are critical expositions of an author's thought, but by their convergence with published texts by Cavaillès, we can assume that these commentaries reflect and reveal aspects of his own thought.

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> of bodies = thought about mathematical relations but not pure ones (because of the first moment from which [they] derive) — therefore *thought that is both mathematical: thought about relations; and non-mathematical: the positing of objects* (...) Thought, the physical system: it is thought about the relations that characterise it: in other words, [it contains] non-physical thought. What is physical in it, is the physicist's effective action in history. Physical experience is situated in history, whereas mathematics is not. What, then, is the objective validity of [physical] experience? The experiencing subject *is not carried away: an absolutisation occurs thanks to the link with mathematics* (otherwise [the link would concern] the subject's own historicity). (...) [There is] an arbitrariness in the development of physics — because of a certain autonomy [which] is not merged with mathematical necessity, *e.g.* the formula $[F = k\, mm'/d^2]$ (?) is not the positing of a mathematical problem — [Purely] mathematical and physical thought are mutually opposed (*necessary concatenation on the one hand; purely historical linkage on the other*)" (Notes, p. 4; emphasis added).

The motif of the dependence of physical thought on the order of events ("l'evénémentiel") will be reformulated in the Course mainly in terms of the notion of *singularity.* This notion is closely connected to causal thinking (related, among other things, to the consideration of the cause of an individual event), but that in turn creates a problem for scientific understanding:

"Causality can only be understood with reference to a notion of a singular individuality, in the midst of a homogeneous mass — but [then] the notion of homogeneous mass disappears (...) while mathematisation, which recreated a homogeneous environment, suppressed the notion of individuality" (Notes, p. 14).

In the following parts of the Course, the question posed in terms of a duality between physics and mathematics will be taken over by that of the interpretation of probabilities, another major motif of Cavaillès's physical epistemology: the gradual dissolution of causality in physical thought after Kant (and its replacement by probabilism). But the motifs remain linked by their similar roles in the tension between concatenation on the one hand, and existence, singularity, position, event and history on the other:

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> "Is there an autonomous concatenation in physics? Appearance of the notion of existence, i.e. singularity (in this context a number refers to something other than itself [= than numbers] — this notion of singularity is the one that characterises physical thought, *cf.* Pascal's Wager: God is a radical existence — any effort to grasp him is a wager. What one is reaching here is different from the thought that reaches it. The very type of physical thought [is] the thought of an existence, that is, of a singularity" (Notes, p. 17, end of the course).

Thus, it is the eternal opposition between necessity and contingency that is here at stake, and which is identified with that between thought of an object and positing of an object, structure and event, concept and history, system and singularity. The internal necessity of mathematical concatenations is not, of course, a necessity in the logician's sense, but a "dialectical" one, that is to say, part of a movement of thought, a development (a development that inevitably takes place in history but cannot be reduced to its realisation in history). One conceptual structure inevitably calls forth another that extends or surpasses it, without having to pass a reality check. This is a close variant of one of the famous formulas on the final page of *LTS* : [(75)]

"What comes after contains what came before, but with a new meaning." (Notes, p. 3).

In contrast to this 'immanent' concatenation (immanent, that is, in the order of concepts, not in that of consciousness), there is the relativism of physical thought that occasionally seduces Cavaillès into even more surprising formulations, be it only in the guise of question marks:

"What is the link between the physicist's experience[s]? Can it be characterised internally, or does it arise accidentally? Does it just develop with a certain culture?" (Notes, p. 4).

We will not find such remarks with Brunschvicg, nor, for another, with Gaston Bachelard: such a sense of relativity and contingency — but of course we are talking about physics here, not mathematics! Nevertheless, there are nuances; distinctions remain in this extradition of conceptual thought to the "pure historical": unlike what happens in "evenemential" history, out there, beyond the walls

[(75)] "What comes after is more than what came before, not because it contains it or even prolongs it, but because it necessarily emerges from it and bears in its content the singular mark of its superiority" (*LTS* 78).

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of the Sorbonne, where the chaos of war and occupation reigns, the subject of physical experience and concatenation "is not carried away" (Notes, p. 4). *Enchaînement* is not the same as *entraînement.* Otherwise, the subject of experience would be *dragged along* by its own historicity. The latter not being the case, the two concatenations, each of which has, in a different sense, an "autonomy", can be interconnected — coordinated — although the development of physics, highly mathematised as it may be, remains heterogeneous and extrinsic with respect to the mathematical becoming as such.

2. Cavaillès and beyond. The question today is whether Cavaillès's work, so abruptly truncated by the history of events, can obtain a new relevance. Before being able to even start answering that question, the ideas expressed in the fragments just quoted would of course need to be translated into a language more suited to scientific, theoretical or experimental practice. Moreover, there are still more things to discover about Cavaillès and *his* coordination problem, more connections to explore.

In the first place, as regards the relationship between mathematics and physics in Cavaillès's perspective, we should also include the complex relations that Cavaillès maintained — particularly on this precise point — with the work of Husserl (*cf. LTS* 66-69). Closely connected, there was also the work of Suzanne Bachelard. It should be remembered that *La conscience de rationalité*[(76)] is just a continuation of the themes addressed by Cavaillès on this subject. From my point of view, this work is not only an attempt to restore Husserlian phenomenology to its relevance for a philosophy of physics after dealing with the fundamental questions left unanswered by the *Krisis* and by transcendental egology. It is also an attempt to reconcile the path chosen by Husserl with that of Cavaillès. In particular, the relations, not simply between mathematics and physics, but also between *mathematical* physics and *theoretical* physics,[(77)] and then between theoretical physics and experimental physics, should be thematised as relations between several types of intentionality (in the sense suggested above). Using Suzanne Bachelard's own phenomenological terminology, we could extend her thesis as follows: since there

[(76)] *The consciousness of rationality. A phenomenological study of mathematical physics.* P.U.F., 1958 (S. Bachelard's complementary thesis).

[(77)] A relationship that has received little attention, but which was also the subject of a pertinent remark by T.S. Kuhn in his essay "Mathematical Versus Experimental Traditions in the Development of Physical Science", in *Id.*, *The Essential Tension*. University of Chicago Press, 1977, pp. 31-65, p. 65.

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is a passage from pure mathematics to mathematical physics that would leave the *thematic focus* of inquiry (*i.e.* the mathematical core) unchanged despite the change in the *domain* of objects (the physical objects intended), the answer to the question posed above — does something remain of the 'dialectical necessity' of mathematics in mathematical physics? — will be affirmative: this necessity is at least partially transferred to physical concepts and reasoning. In the subsequent transition from mathematical to *theoretical* physics, on the other hand, there is invariance of the *domain* despite the change of thematic focus — and in this sense, there will be, once again, partial transport of the necessity of concatenations. A reading hypothesis of this kind obviously cannot be decided at once and will have to be the subject of a separate study. Be that as it may, if this hypothesis proves relevant, it seems to imply that Cavaillès is closer to Husserl (or to a certain reading of Husserl) on this point than is usually acknowledged.

But secondly, there is another connection to be made. A philosophical analysis of the measurability of physical properties — a topic not developed by Cavaillès — should undoubtedly constitute one of the mainstays of a response to the problems of coordination. Now, while admitting that one of the essential links between mathematical thought and physical experience must be found in the actual process of measurement, it would be wrong to say that mathematical concepts can be translated into physical quantities simply "by measurement", as if there were a method that could be achieved once and for all. Measurement, the measurability that enables a physical property to be effectively conceived as a quantity, must in each case be conquered or reconquered. This can only be achieved through a complicated, trial-and-error historical process of conceptual adjustments and experimental procedures involving the development of empirical, and therefore unpredictable, relationships between different mathematical (or rather, mathematised) concepts such as temperature — volume — pressure (Mach's example above). What's more, the process involves the invention of new instruments that give rise to a possibility of measuring what only thereby *will become a* physical quantity. So, a profoundly historical process must be presupposed before a specific coordination problem can be solved. In other words, we cannot solve the problem in its generality and once and for all by working out an answer 'from the outside'. Doesn't this view have a remarkable affinity with one of the central ideas expressed by Bas van Fraassen[(78)] in *Scientific Representation*:

[(78)] So as not to abuse the reader's patience, I abstain from parallel comparisons (which would be appropriate) with the work of, say, Ernst Cassirer and Michael Friedman.

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> "The term ['problem of coordination'] had appeared in Mach's writings on mechanics and thermodynamics; was salient in the discussion of the relation between mathematical and physical geometry that extended from the 19th century into the 20th; and came to special prominence through the writings of Schlick and Reichenbach when logical empiricism was beginning to break with the neo-Kantian tradition. (...) The questions *What counts as a measurement of* (*physical quantity*) *X?* and *What is* (*that physical quantity*) *X?* cannot be answered independently of each other [which] brings [up] the famed 'hermeneutic circle'. We shall examine this apparent circularity (...) [and come to] the conclusion that pure or presuppositionless coordination is neither possible nor required."(79)

Van Fraassen then offers us an in-depth analysis of the issues involved in establishing a measurement (or rather *measurements*) for quantities such as temperature, time, length and distance, with Mach, Pascal, Dalton, Poincaré, Einstein and many others. It was only at the end of each of these respective processes that the phenomena in question became 'observables'.

> "The rules or principles of coordination that can be introduced to define particular sorts of measurement cannot even be formulated *except in a context* where some forms of measurement are already accepted and in place (...) [M]easurement practice and theory evolve together in a thoroughly entangled way. (...) [O]ne might say that the measured parameter — or at the very least, its concept — is *constituted* in the course of this historical development. Choices are made, and once made may encounter resistance, whether in experiment or in theory-writing or (more usually) in combination of the two."(80)

From this point of view, it is clear that the choices to be made will be motivated more by elements of the history of science and by pragmatic considerations than by a logic of science or by simple conventions.(81) For the point I want to make, it is enough to note

(79) Bas van Fraassen, *Scientific Representation. Paradoxes of Perspective*, *o.c.*, p. 116. This quotation is taken from the beginning of chapter 5: "The Problem of Coordination".

(80) *Ibid*, p. 138-139. (Emphases in the original).

(81) In the overall plan of the book, van Fraassen elaborates this moderate pragmatism through an analysis of the entire context of '*use*' in any research, including

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that van Fraassen has expressed in a precise and detailed form, by means of a study of historical cases, what Cavaillès may have thought about coordination only in general terms. *Mutatis mutandis*, this would even apply to the philosophy of mathematics conceived in Cavaillès's perspective: you always need a multitude of notions that are already established ('*in place*') and accepted if you want to develop even a so-called elementary notion.[82]

There are undoubtedly major differences between the concern for necessity that characterizes Cavaillès's work and the perspective that van Fraassen would take regarding mathematics as such as well as regarding its role within the empirical sciences. The concept of a core of necessity does not seem to arise in the perspective of constructive empiricism, although the latter is also focused on the constitutive role of mathematical concepts, theories, models, and structures in physics.

Apart from the emphasis on coordination, there are still several convergences between the two perspectives: they have in common the defence of a probabilistic point of view about physics as well as the interpretation of probabilities as wagers; the tendency towards structuralism in the theory of science — a reference, of course, *avant-la-lettre*, as far as Cavaillès is concerned, but which seems very real, given the role of what we might call, following van Fraassen, the "paradox of Hermann Weyl"[83] in the birth of this type of structuralism. Above all, there is the importance of the study of the genesis of concepts, theories and techniques, and their intertwining, which is decisive for both authors; the historical character of epistemology, which, in van Fraassen's case, was particularly decisive in his last major work of 2008. This brings us to an even more remarkable affinity between the two authors: if they are both attracted to a certain structuralism, it is certainly not, in both cases, to its realist version, but clearly to its alternative, now called 'anti-realist',[84] or, one may prefer, 'constructive' version.

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the researcher's position from his or her first-person point of view and all that this implies in terms of indexicality, intentionality and intensionality.

[82] For mathematical epistemology, we need only think of the role of transfinite induction in the proofs of a theorem about natural numbers like R.L. Goodstein's.

[83] In other words, a science is capable only of determining its subject-matter "up to isomorphic representation". See van Fraassen, *o.c.*, pp. 208-210.

[84] "Du Collectif au pari", *o.c.*, p. 159: "The difficulty arises from the fact that classical epistemology is underpinned by a realist ontology (...) [a] hypothesis of an 'in itself' (« d'un en soi ») of the things that the scientist must describe...".

§ 7. — Conclusion.

To sum up, the problem, as defined, or redefined, by Cavaillès, is made up of a complex of motifs and themes. Generally speaking, it can be understood in terms of Kant's classical problem (which in turn was already a transformation of the much older question of the applicability of mathematics to our knowledge of the world). On the other hand, it can be broken down into partial and more specific problems, such as the problem of coordination between abstract concepts and experimental acts (including measurement operations), then the problem of the meeting of mathematical concepts of different origins in the same physical theory, or again the somewhat inverse problem of the insurmountable gap between the unique, organic and internal concatenation of mathematics and its diffraction and externalisation into a plurality of systems of concepts and acts; systems or sequences which, in the 1942 essay, would eventually merit the name of "physical *concatenations*". Of course, there remains an irreducible divergence between mathematical concatenation and physical experience, which is directed towards action in the world; a divergence that we have been able to interpret in terms of the contrast between concatenation in the "time of concepts" [(85)] and historical time, the "time of events". In the theory of science, this contrast is reflected in the question of the subsistence of a core of necessity even where convention seems to prevail; necessity, to be sure, not in the classical sense of logical necessity, but in the sense of a succession of conceptual stages imposed each time from the stage of development previously achieved. Now, through the constitution of a mathematical physics and then a theoretical physics, this asserted necessity of mathematical concatenations is partially transmitted to the physicist's acts, experiences, and experiments, in such a way that these experiences can be organised into physical concatenations. This transfer of cognitive value means that we can speak of conditions that effectively make the existence of a science of nature possible and intelligible — which was an essential part of what was at stake in Cavaillès's "philosophical testament". His affirmative response to the problem of coordination, reinterpreted in this way, could only be bequeathed to us in the embryonic stage it had finally reached in his 1942 essay. Doubtless, the approach "leaves too many (…) questions open and is too vague in its conclusions"[(86)] to be considered a

[(85)] See my "The Structure of Mathematical Experience According to Cavaillès", *o.c.*

[(86)] As Cavaillès himself commented about the "epistemologies of immanence" (Brouwer, Brunschvicg) (*LST* 14).

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solution of the problem. However, subsequent works, such as those by Suzanne Bachelard, Michael Friedman, and Bas van Fraassen, have opened perspectives that will enable to develop his thinking on this aspect of a theory of science in directions that may be directly or indirectly related to it, but which undoubtedly each have their own orientation.

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Lautman, mathematical critic

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Abstract. We offer a new perspective on Lautman's work, envisioning him as a "mathematical critic" rather than a so-called "philosopher of mathematics". We propose a distinction between Philosophy and Criticism, and, building on a study of Lautman's sources and mathematical studies, we situate the main bulk of his work in the realm of Mathematical Criticism.

Keywords. Lautman, Hilbert, Herbrand, philosophy, criticism.

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§ 1. — Introduction.

Analysis and synthesis always deserve to be thought of in counterpoint. In the case of Albert Lautman (1908-1944) it is interesting to *analyse* (disjoin) his work from the point of view of a "mathematical criticism" similar to literary, artistic, or musical criticism, before *synthesising* (conjoining) this approach with the more traditional views of "mathematical philosophy". *Section 2* offers a short discussion of the contrasts between "Philosophy of Mathematics" and "Criticism of Mathematics". *Section 3* looks at Lautman's sources, both quantitative and qualitative, and reveals his strong focus on mathematics at the expense of philosophy. *Section 4* looks at Lautman as a critic of the central mathematical masters of the period 1830-1930: Galois, Riemann, Poincaré, Hilbert. *Section 5* traces Lautman's references to his contemporaries, in particular to the work of Jacques Herbrand. Finally, *Section 6* studies the connections of the "critical Lautman", highlighted in the previous sections, with the better known "philosophical Lautman".

§ 2. — Philosophy disjointed from Critique.

We consider mathematics as a *thought*, i.e. a "mixture" [197][(1)] between techniques and ideas[(2)]. This thought can be looked at from several points of view, depending on how one responds to the simple adverbs *what, who, where, when, how, how much, why*. A mathematician will be particularly sensitive to what-how-many; a historian of mathematics will be sensitive to who-where-when; a classical philosopher will be sensitive to what-how-why; an analytic philosopher will be sensitive to what-how[(3)]. On the other hand, a *mathematical critic*, if such a being exists[(4)], will have to be attentive to *all* the what-why-where-when-how-how-why modalities, and will have to carry out a *(de)construction of mathematical thought on all the possible registers of creation*[(5)].

The analytic philosophy of "mathematics" has *reduced* its questions to the philosophy of logic and set theory, and focused on *flattened* versions of what (number) and how (deductive reasoning). With this reduction and concentration, analytic philosophers have constructed a precise and self-referential discourse, linguistically and logically powerful, but mathematically *empty*. The distancing from living and complex mathematics (space-form-structure, around geometry, topology, abstract algebra, the complex variable, analytic and algebraic number theory, functional analysis, differential equations, etc.) has been complete. With this, not only did the historical (who-where-when) and metaphysical (why) spectra of mathematics disappear, but the objects (what) and methods (how)

(1) References of the type [page] lead to the Vrin edition of Lautman's "Works" [Lautman 2006]. References of the type [name year, page] lead to the bibliography, indicated at the end of the article.

(2) As is well known, the notion of "mixture" comes from Lautman. We have extended this Lautmanian conception through the notion of *bundle*: mathematics comprises (at least) a "folded" space of techniques (definitions, axioms, proofs, examples) and an "unfolded" space of ideas (images, concepts, intuitions) which become "mixed" and project onto the techniques. *cf.*[Zalamea 2021a].

(3) Of course, these simple disjunctions only try to give a *broad orientation* for *communities of* scholars. Beyond any disjunctive analysis, there may be special cases of historians of mathematics who amply cover the full register (such as the exemplary case of Jeremy Gray), or philosophers of mathematics who also cover it (such as Gilles Châtelet).

(4) For a survey of the situation, see Zalamea's article in the first issue of *Annals of Mathematics and Philosophy*, [Zalamea 2021b].

(5) In this perspective, and going beyond the usual classifications, we could say of Gray and Châtelet that they are *mathematical critics* in the most accomplished sense.

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were singularly impoverished. Perhaps this flattening was necessary in an attempt to achieve an (illusory) precision of language, but the fact is that this philosophical approach has left out the essential forces of mathematical creation[6].

The work of the *mathematical critic* must explicitly counter any reductive attempt, and deepen the *multidimensionality of* perspectives and interpretations. In this sense, a *healthy disjunction* with the philosopher must be made (before attempting a later conjunction, see *Section 6* below). The critic must, above all, (A) *know how to look at* mathematical works. Just as a literary critic who wants to study Proust, for example, must have read *À la Recherche* directly, a mathematical critic who wants to study Riemann, for example, must have read his *Doctoral Thesis* directly. The return to the sources, to the works, is a *sine qua non* for the critic, a condition often important for the historian, but often neglected by the philosopher. The what-when-where-when becomes really central for the critic: he cannot restrict himself to a secondary literature, far from the works, and develop abstract linguistic discourses, far from the mathematical structures. Secondly, having been confronted with the complex objects and processes of mathematical creation, the critic must (B) *accurately describe* the central ideas and associated techniques of the works submitted to his analysis (mathematical thinking as a "sheaf", answers to the how-how much). Finally, the critic must be able to (C) *explain the great forces* (whys) that sustain mathematical creation. In this sense, he goes beyond the mathematician, just as the literary critic goes beyond the writer, or the art critic goes beyond the artist.

Thus, thanks to a *full immersion in the broad adverbial panorama*, the critic (mathematical, literary, artistic, musical) will really approach the discipline studied (mathematics, literature, art, music). His or her work will be more *synthetic*, necessarily broader, since he or she will have to deal with a wider spectrum of perspectives. It will therefore be a complement to *analytical* work that is necessarily more limited. The *back and forth* between analysis and synthesis, between locality and globality[7], between language and vision, will have many advantages for a more *faithful* understanding of mathematics.

[6] The analyses of mathematical creativity by the French school — [Poincaré 1908], [Hadamard 1943], [Grothendieck 1985-86] — are still fundamental today.

[7] This is one of the recurring themes in Lautman's work, *cf.* [Lautman 2010].

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§ 3. — Lautman's sources.

The point (A) mentioned above — looking at the works — is imperative for the mathematical critic. *Working on* mathematical *work* is one of his fundamental labours[8]. Lautman's mathematical knowledge has always been appreciated[9], and it is easy to look in detail at (*i*) his preparatory studies and readings (*source* study), and (*ii*) their use (*reference* study). Lautman's sources are multiple and complex[10]. On the one hand, his mathematical sources appeal to *treatises that are* now considered *classical*: mathematical logic (Hilbert & Ackermann 1928, Hilbert & Bernays 1939), number theory (Hecke 1923, Hurwitz & Courant 1925, Ingham 1932, Herbrand 1936), abstract algebra (van der Waerden 1931, Glivenko 1938), differential equations (Bieberbach 1923, Kähler 1934), complex variables (Picard 1896, Osgood 1907, Weyl 1913, Montel 1927, Nevanlinna 1936), functional analysis (Fréchet 1928), topology (Lefschetz 1930, Seifert & Threlfall 1934, Alexandrov & Hopf 1935), physics (Weyl 1928, Broglie 1932, Hilbert & Courant 1937, Julia 1938). The *dates* are remarkable: except for three treatises on the complex variable, *all* the others were published in the 1920s-1930s, highlighted as crucial by Dieudonné. On the other hand, Lautman is very attentive to *research papers* of his time: Hilbert 1923, Cartan 1924, Gödel 1930 & 1931, Herbrand 1931, Pontriagin 1931, Hopf 1932, Tarski 1935, Birkhoff & von Neumann 1936, Gentzen 1936, Weil 1938. Lautman also attended the *Séminaire de Mathématiques* of the Institut Henri Poincaré and benefited from brilliant lectures by Possel 1934-35, von Neumann 1934-35, Weil 1934-35, Cartan 1937. The doctoral theses of his friends Jacques Herbrand (1930) and Claude Chevalley (1934) are included in his work. References to

[8] Simone Weil has always defended the crucial importance of *work* in human experience, *cf.*[Labbé 2018]. For views on Simone's *mathematical eye, cf.* [Lafforgue 2014], [Zalamea 2022].

[9] According to Dieudonné's well-known phrase, "in contact with his comrades and friends Jacques Herbrand and Claude Chevalley (two of the most original minds of the century), (Lautman) had acquired a much broader and more precise view of the mathematics of the 1920s and 1930s than most of the mathematicians of his generation, who were often narrowly specialised; I can testify to this in my own case" [35]. Curiously, Yvon Gauthier reproaches him for this extension of his views: "his mathematical culture was broad, too broad perhaps not to remain superficial in many cases" [Marquis 2010, 159]. There is no reason for this rebuke: extension, precision and depth go hand in hand in Lautman's work.

[10] For an exhaustive study, see my "Noticia sobre las fuentes de Lautman", *cf.*[Lautman 2011, 543-558].

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the *Mathematische Annalen* and *L'Enseignement Mathématique* show his desire to be well informed about current mathematical work.

From the point of view of (*ii*) *the* use of sources, it might be appropriate to describe Lautman's work as a sustained commentary, both analytical and synthetic, on Hilbert's profound work. Hilbert is cited 25 times as a source and mentioned in 47 pages of Lautman's body of work[(11)], both around his work in functional analysis, and in number theory and logic, where Herbrand's influence will have been decisive. From the quantitative point of view, the attention towards Hilbert is thus very sustained, but this attention is also very fundamental qualitatively: Hilbert's space is for Lautman the *sine qua non* example of a mixture, between continuous and discrete, local and global, finite and infinite. The second most cited mathematician is Riemann (1 reference as source, 38 pages of mentions). The other most studied authors are Élie Cartan (27 pages), Henri Poincaré (25 pages) and Hermann Weyl (23 pages), whose importance may have been pointed out to Lautman by Ehresmann, who studied with Weyl (1932-1934) and completed his thesis under Cartan (1934). Thus, under the aegis of Riemann-Hilbert-Weyl, as opposed to the French Galois-Poincaré-Cartan school, we see how Lautman's enormous mathematical culture is constituted, and how he moves towards a kind of *mixed structuralism*, very close to the mathematical boom of the 1920s and 1930s, which will become the true mark of his philosophical approach.

Indeed, far from an ingenuous "Platonism", which has poorly oriented the reception of Lautman's work, it is on a *complex combination of global structuralism and local mixtures* — prefiguration of the theory of sheaves, very clear from his "Rapport Bouglé" (1935), *cf.*[Lautman 2010] — that his original vision of mathematics is based. With Lautman, as with Cavaillès, effective (local) mathematics and conceptual (global) reflection go hand in hand, and it is precisely the evolving structures that allow the passage between the concrete and the abstract. It is enough to look at (*i*) his sources and (*ii*) his philosophical uses, to realise that Lautman can be much better understood as a *mathematical critic* than as a mathematical philosopher. In the following list, next to each philosopher, the first number indicates the number of references as source and the second number indicates the number of pages where the author is

(11) The source and reference counts here refer to the Spanish edition of Lautman's "Works" [Lautman 2011], which is more complete than the French editions (Hermann, PUF, Vrin).

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studied: Plato (7 — 10), Descartes (3 — 3), Leibniz (3 — 6), Kant (3 — 9), Carnap (8 — 17), Wittgenstein (1 — 6), Heidegger (6 — 13), if we go through the most cited philosophers. The *myth of* the "Platonist Lautman" invented by the secondary literature is therefore immediately destroyed if we look closely at his writings. Not only are his philosophical perspectives *multiple*, far from being centrally Platonic, but they are far *fewer*, both in quantity and quality, compared with his mathematical mentions.

§ 4. — The critique of the Masters.

The task (B) for the mathematical critic mentioned in *Section 2* — to describe techniques and ideas with precision — concentrates the best of Lautman. His vision of the "proper movement of a mathematical theory" [66], of the "genesis of effective mathematical theories" [237], acquires great accuracy in his writings. Lautman deals with an enormous spectrum of mathematical constructions, as shown by a short list[(12)] of the scholars he studies: Bernays, de Broglie, Cartan (Elie), Dirichlet, Einstein, Euler, Galois, Gödel, Herbrand, Hilbert, Hopf, Klein, Lebesgue, von Neumann, Pfaff, Poincaré, Riemann, Schrödinger, Weierstrass, Weyl. In particular, there are magnificent descriptions of the major techniques and ideas introduced by the Masters of modern mathematics in the period 1830-1930: Galois, Riemann, Poincaré, Hilbert.

Galois' work is approached as an example of "the ascent to the absolute" [165]. According to Lautman, "the interest of the logical scheme of Galois' theory is considerable" [167], expressing the jump "from an imperfect basic domain" to "the existence of an extension where this imperfection has disappeared" [167-168]. The association of various numbers and measures at each stage of the ascent between two bodies then fulfils a *structural completion* fundamental to the Lautmanian vision: effective mathematics embodies a movement of *abstract intermediate mixtures*[(13)] which gives the discipline its deepest raison d'être. If the extensions of fields and its Galois groups are very useful to grasp the *variations*

[(12)] This list includes, in alphabetical order, mathematicians (and physicists) mentioned *on at least seven pages* in his work (reference to appearances in the complete Spanish edition [Lautman 2011]).

[(13)] "Study of possible modes of organisation of elements of indifferent nature", *cf.*[Lautman 2011, 378].

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and invariances of mathematical thought, the meromorphic transformations and the representation theorems of Riemann around its multiple surfaces become for Lautman the perfect example to unveil the dynamic richness of modern mathematics. Indeed, the study of the uniformisation of Riemann surfaces in Lautman's Main Thesis [174-178] is a true masterpiece of exposition and understanding. The young "philosopher" becomes at this point a formidable *critic*, when he explains how "it is still a question of eliminating the imperfections of certain mathematical beings by passing from what they primitively are to an ideal of absolute simplicity whose existence is implied in the very entanglements of their structure" [174]. *The existence by decomposition and recomposition of the structure* (point (C) synthetic, explanatory of the critical labour) emerges as a result of long and precise preliminary analyses of the effective mathematics (point (B) of its descriptive labour). The analysis of multiformities, ramifications, overlaps of functions of complex variable, exemplified around algebraic functions and n-th roots [175], is linked to the topological characteristics of the associated Riemann surfaces [176], before arriving at the conformal representation theorems (Riemann, Poincaré, Hilbert, Koebe) of a simply connected Riemann surface "either on the whole complex sphere, or on this sphere from which a point is removed, or on the unit circle of the complex plane" [177]. Dazzled "by the immensity of the new horizons" [177], the critic Lautman succeeds in his triple task of *opening up mathematical intelligence*: (A) to look, (B) to describe, (C) to explain major turning points in thought.

Lautman studies — looks at, describes, explains — several central aspects of Poincaré's work: duality theorems in algebraic topology [160-163], topological methods in differential equations [215-218], hyperbolic metrics and the uniformity theorem of Riemann surfaces [101-102]. In addition to constant references to the links between topology and algebra invented by Poincaré, Lautman notes the interest of particular theorems in understanding general problems: the "properties of internal structure" opposed (and conjoined) with the "extrinsic properties of situation" [163], the "conditions of existence of fixed points" which depend as much on the "structure of the basic domain" as on the "nature of the internal transformation" [217], "the primacy of geometric synthesis over that of 'numerical' analysis" [105]. Here we see the critic in all his technical strength, capable of unveiling vast panoramas from close studies of mathematical works.

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Around Hilbert, Lautman highlights his contributions to infinitary forms and functional equations [93-95], metamathematics and proof theory [129-131], the theory of class fields [168-169], infinity and the universal counterexample logical operator τ [180-181], Hilbert space [200-207], the Dirichlet problem [213-214], quantum mechanics [286]. Thus, Lautman traverses the vast spectrum of Hilbertian mathematics and really immerses himself in the *task of the critic*, attentive first (A-B) to the delicate technical expressions (fundamental forms of mathematical creativity) and then (C) to the great forces that unfold in it: "unification of mathematical disciplines" [93], "duality of planes (...) between formalised mathematics and the mathematical sciences" [94], "the development of the mathematical sciences" [95]) "between formalised mathematics and the metamathematical study of this formalism" [130], "solidarity of structure between the elements of a whole and the whole to which they belong" [169], legitimisation in the same way "to speak of the existence of the object as of the existence of the point at infinity in geometry, imaginary numbers, or ideal elements of a number field" [181], recognition of Hilbert space (always named in the singular, before the general axiomatisation of von Neumann) as "mixed (..) homogeneous to the continuous by the nature and topology of its elements, and to the discontinuous by its structural decompositions" [207], recognition of the Hilbert proof of Dirichlet's principle as a "mixed intermediate between the structure of the domain and the existence of the function" [213], the place of Hilbert space in the study of processes of evolution and propagation of physical quantities in mechanics [286]. *A continuous pendulum back and forth between descriptive analysis* (*A-B*) *and explanatory synthesis* (*C*) is one of the critic's own and characteristic strengths, beyond philosophical positions that would rather assume a firm categorical perspective.

§ 5. — The criticism of contemporaries.

The list of mathematicians of his time studied by Lautman is very indicative of his desire to be at the forefront of research: Ahlfors (2), Alexander (5), Alexandrov (5), Bernays (10), Bieberbach (5), Birkhoff (2), Caratheodory (2), Chevalley (5), Courant (5), Ehresmann (3), Fréchet (6), Glivenko (2), Gödel (9), Hasse (2), Hecke (6), Herbrand (17), Hopf (12), Lefschetz (3), Lukasiewicz

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(2), von Neumann (7), Takagi (4), Tarski (3), van der Waerden (5), Weil (2), Weyl (24)[14]. Two names in the list are particularly important for Lautman: Hermann Weyl and Jacques Herbrand. Both cases are strong examples of *transitions* between the Hilbert school and the contemporary mathematics where Lautman is located. On the one hand, Weyl contributes to the understanding of *space* (via his monograph on Riemann surfaces (1913), quoted 5 times by Lautman) and *geometric* magnitudes (via the continuous-discrete dimensional analysis from Hilbert spaces). On the other hand, Herbrand contributes to the understanding of *number* (via his monograph on the theory of number fields (1936), cited 1 time) and, above all, of *arithmetic* magnitudes linked to his proof theory (via his Thesis (1930), cited 5 times). Thus, taking advantage of Weyl and Herbrand, Lautman obtains a good vision of the *space-number* dialectic, always crucial for a full understanding of mathematical thought.

The Weyl case is linked to central themes of modern mathematics (1830-1930): intuitionism [42], group theory [48, 83, 85-87, 117, 143-145, 193-194], Riemann surfaces [85, 101-102, 135, 171-178, 188], topology [113, 117, 121, 154, 188], functional analysis [154, 206]. The *critic* Lautman draws on (A) Weyl's work to (B) describe geometric techniques in counterpoint to abstract algebra, and to deduce (C) a deep understanding of the modern "war" between space (the "angel of topology", according to Weyl) and number (the "devil of algebra"). Lautman's *very detailed descriptions* of Weyl's work on group theory and Riemann surfaces help us to really *grasp* the techniques involved. This work (B) of the critic — often misunderstood by mathematicians, philosophers, historians — is very useful to come to a better understanding of mathematical thinking. Often, the mathematician himself is unable to (B) describe his creations, which the historian or philosopher simply (A) does not look at. The critic, *forced* to join the analytical steps (A) and (B) to arrive at the synthetic explanation (C), turns the whole situation around, and develops a task *complementary* to other approaches to mathematics.

The Herbrand case is one of the gems of Lautman's *analysis-synthesis.* Very close to his friend, Lautman remarkably explains the construction of *Herbrand's fields* in his theory of demonstration, following Galois' fields. The two precocious geniuses, Galois (19

[14]The numbers in brackets correspond to the *number of pages* in the Spanish edition [Lautman 2011] where mathematicians are mentioned.

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years old) and Herbrand (22 years old), come together under the vision of the young critic (30 years old). Herbrand, a disciple of Hilbert [43], constructed his method of fields to ensure in certain cases the non-contradiction of a theory [45-46]; the theory cannot be too "high" (*e.g.* cover analysis), but works for *bounded strata of arithmetic*; what results essential is the construction of an "intermediate schematic, that of individuals and fields considered not so much for themselves as for the infinite consequences that the finite calculations operated through them allow" [46]. Lautman presents "Herbrand's theorem" as a profound structural-syntactic connection: "the existence 'in extension' of an infinite field where *non-P* is feasible is equivalent to the structural fact that *P* is not part of the set of provable propositions of the theory" [186]. It is "a pure case of solidarity between a set of formal operations defined by a system of axioms and the existence of a field where these operations are realizable" [180], which responds to a fundamental creative conception: "the essence of a form being realized within a matter that it would create, the essence of a matter giving rise to the forms that its structure draws" [186]. The construction of Herbrand's fields is described in detail (indices, functions, iterations, growth) [198-200] as a *mixture par excellence*[15] between mathematical infinitude (individual values) and metamathematical finitude (recursive functions before the letter). Thus, (A) immersing himself in an arduous doctoral thesis, (B) describing the main mechanisms emerging from it, and (C) reflecting on the method, combinatorics and structuring of the theory of proof proposed by Herbrand, Lautman accomplishes the supreme and complex task of mathematical criticism: to *see in detail and in depth, in order to make us see better*.

§ 6. — Criticism in conjunction with Philosophy.

[15] "Intermediaries between signs and their values, these fields are, on the one hand, homogeneous to the finite discontinuity of signs since to a sign of a variable corresponds only one value a_i and, on the other hand, they symbolise an infinity of mathematical values since the letter a_i represents any possible mathematical value of the variable y when it intervenes in the particular form. A mediation is thus carried out by these fields from the finite to the infinite, which makes it possible in the cases treated by Herbrand to dominate the infinite and such is the role that we will recognise in the mixed fields (...)". [200].

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The work of the philosopher extends to a fourth level of (D) conceptual reflection, speculative discourse and dialogue with the history of philosophy. Lautman is a magnificent example of the *conjunction* between the critic (A-C) and the philosopher (D). Some of Lautman's strong conceptions (notions, Ideas, mixtures, dynamic Platonism, effective mathematics) are situated in this more philosophical level, but always *building on his profound critical work*. However, the situation is far from being the same with other "philosophers of mathematics". Many divergent situations can occur: (*i*) weak or no mathematical level (A-B), and strong (C-D) but restricted to logic, (*ii*) strong (A-B-C) mathematics, devoid of (D) philosophical considerations, (*iii*) strong (B-C-D), far from analyses of the original mathematical works, (*iv*) strong (A-B-C-D)

It is as a *critical philosopher* — arguably the major one of the 20th century — that Lautman deserves to be evaluated. And it is probably because of this difference with standard philosophy that the work of the young French thinker, *critically* focused on the *mathematics of strata* (*A-B*), has not really been understood by the so-called "philosophy of mathematics", essentially Anglo-Saxon, turned entirely to the *logic of strata* (*C-D*). Indeed, to make a philosophical critique of mathematics is a completely different task from making a philosophy of logic. It is *thanks to* his critical and mathematical vision (A-B-C) that Lautman can propose his most original philosophical ideas: (1) the construction of *Ideas* as partial resolutions of *notions* (*e.g.,* the Cantorian Idea of the continuum as a resolution of the notions of completeness and discreteness, but also, reciprocally, the Brouwerian Idea of the continuum as an *inverse* resolution to the Cantorian one, and so on), (2) the development of *mixtures* as a central force of mathematical creativity (e.g., Galois theory, Riemann uniform surfaces, Hilbert spaces, Herbrand fields, etc.), and (3) the understanding of a *dynamic Platonism as an iterated back-and-forth between various strata of* understanding (*e.g.*, particular *effective mathematics* in contraposition with general Ideas).

A rich and concrete mathematical material (A), a close analysis of this material (B), a deep synthetic understanding of the established forces (C), and a *broad* philosophical reflection (D) based on these labours, constitute the specificity and originality of Lautman's work. We should hope that the *young French philosopher-critic of mathematics* will be set as an example of this "difficult thinking", hailed by Bachelard, which tries to reflect and understand in depth mathematical invention.

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Rereading Léon Brunschvicg.
Critical idealism and Russell's method of analysis

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Summary. The aim of this article is to re-read Brunschvicg's critical discussion of Russell's philosophy. We attempt to highlight the philosophical reasons behind Brunschvicg's distrust of logic, showing that in the final analysis his criticisms of Russell's logicism relate to the difficulties Russell faces in thinking about the application of mathematics to the sciences of nature.

Keywords. Brunschvicg, Russell, Duhem, neo-Kantianism.

§ — Introduction.

Léon Brunschvicg is one of those philosophers who gave Russell's philosophy and logic a critical reception in France. Les *Étapes de la philosophie mathématiques*, as we shall see, thus regard the Principles as the latest avatar of the fundamental philosophical error of pursuing a " 'false ideal' of universal and absolute deduction"[1]. In contrast, les *Étapes* and *Human Experience and Physical Causality* set out a philosophy of the history of science, its progress and the renewal of its methods. However, judging by the posterity of the two men, it would seem that history has largely decided in favour of Russell over Brunschvicg. Even in France, Brunschvicg's position is not glorious, and Vincent Descombes's investigation in *Le même et l'autre* opens with the younger generation's rejection of the old master's teaching[2].

The point of studying Léon Brunschvicg would therefore seem to be essentially historical. A recent book on the subject presents him as one of the key figures in the history of twentieth-century French philosophy[3]. A glance at the list of philosophers who attended his lectures at la Sorbonne, or who defined their philosophical project in relation to his, suffices to give one an idea of his general influence: Bachelard, Cavaillès, Gueroult, Merleau-Ponty, Gusdorf, Hyppolite and others. As far as French philosophy's relationship with so-called analytic philosophy is concerned, Brunschvicg's disdain for "logistics" may have been responsible for a certain reticence[4].

The idea that it is possible to reread with profit someone who has earned himself a veritable reputation as a mandarin — for example by attributing a mental age of seven to Aristotle or by judging Lévinas's accent incompatible with the agrégation de philosophie[5] — immediately comes up against a number of objections. First of all, as far as the history of logic is concerned, Alonzo Church notes in his review of the 2^{e} edition of Les *Étapes de la philosophie mathématique*

[1] Brunschvicg, Léon. *The Stages of Mathematical Philosophy*. A. Blanchard. Paris, 1972, p. 411.

[2] Descombes, Vincent. *Le même et l'autre*. Minuit. Paris, 1979, p. 17.

[3] See the introduction by Terzi, Pietro. *Rediscovering Léon Brunscvhicg's critical idealism. Philosophy, History and Science in the Third Republic*. London-New York: Bloomsbury, 2022.

[4] Soulez, Antonia. "La réception du Cercle de Vienne aux congrès de 1935 et 1937 à Paris ou le 'style Neurath"'. In *L'épistémologie française 1830-1970*, Michel Bitbol and Jean Gayon (eds), pp. 31-66. Science & Philosophie. Paris: Éditions matériologiques, 2015, p. 47.

[5] Terzi, *op. cit.* pp. 210 and 128.

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that, as the author's documentation stops in 1912, the book is only of historical interest[6]. Clearly, a century later, the scientific informations on which Les *Étapes* or *L'expérience humaine et la causalité physique* rely are no longer as up-to-date as they might have been at the time. It should also be added that Brunschvicg's method in the history of science may seem too monumental and teleological today in many respects — although it should be remembered that he rejected the title of historian of science. Worse still, a large part of his philosophical work is devoted to discussions of several philosophers who, it would be no exaggeration to say, have not established themselves as key figures in contemporary philosophical thought: Hamelin, Rauh, Milhaud, le Roy, Meyerson, Boutroux and so on. Finally, his idealism seems to border on the absurd when he asserts that "the history of Egypt [...] presupposes before it the history of Egyptology"[7].

Having recalled these elements, there are at least two — closely related — reasons for thinking that it is still possible to read Brunschvicg with profit today. The first is that Brunschvicg's criticism of Russell does not simply amount to an unmotivated rejection of logic — the value of which, as we shall see, he acknowledges. His criticisms are philosophical and concern the method of analysis, which is why, in essence, Brunschvicg's objections to the *Principles* go beyond the scope of Russell's work, and thus acquire a more general scope. Fundamentally, the latter would necessarily lead to a form of scepticism incapable of understanding the workings of the natural sciences and, surprisingly, Brunschvicg detects an "analogous inspiration"[8] in Russell's method of analysis and in the famous thesis defended by Duhem. However, given the popularity of the theses of the author of *Physical Theory,* particularly since their recovery by Quine, we have a second reason to believe that Brunschvicg's texts are not solely oriented towards the nineteenth century.

Finally, before getting to the heart of the matter, let us make it clear that Brunschvicg's radical idealism is not necessarily as absurd as it might first appear. In fact, a number of texts lay claim to a genuine empirical realism and castigate pragmatism or philosophers that are inclined to drive a wedge between reality and our

[6] Church, Alonzo. Les *Étapes de la Philosophie Mathématique* by Léon Brunschvicg. *The Journal of Symbolic Logic* 13, n° 4, 1948, p. 216.

[7] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 520.

[8] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 85.

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knowledge of it. The aim is to understand the real significance of Brunschvicg's critical idealism when his realist claims are taken seriously[9].

In order to carry out this programme, we will start from the criticisms that Brunschvicg and Russell exchanged in order to determine their exact scope (I). It will then be possible to trace the real disagreement back to Russell's approach to mathematics through the method of analysis (II). We will then examine the nature of the link that Brunschvicg establishes between Russell and Duhem to identify the erroneous conception of experience that constitutes, in the eyes of the French philosopher, the heart of the problem (III).

§ 1. — Brunschvicg's critique of logistics.

Brunschvicg was never interested in Russell's intellectual development and always stuck to the philosophy of the *Principles*. It is therefore necessary to identify the theses of this text that he criticizes, to determine whether these criticisms reach their target in 1903, and in the rest of the work. In order to pinpoint the disagreement between Brunschvicg and Russell, we need to list the principles on which Russell's first philosophy is based. There are five of them[10]. The principle of external relations asserts that a logic of relations is necessary for the logicist project of reconstructing the totality of mathematics on the basis of strictly logical notions. In metaphysical terms, this amounts to saying that relations have an existence in their own right, irreducible to their foundation in the terms they link. The second principle is that of analysis, which aims to reconstruct knowledge on the basis of the fundamental notions that make it possible to account for it. The third principle, which Russell attributes to Moore, is that of realism, which asserts the independence of the cognitive content of propositions from the mind in which they are thought. The principle of logico-grammatical parallelism, which Russell soon abandoned, asserts that each term in a sentence — with the exception of the denotative terms *a*, *every*, *any*, *all* and *some* — has a corresponding meaning.

[9] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. VII.

[10] We borrow this reconstruction from Vuillemin, Jules. "Can a Frenchman still understand philosophers from across the Channel?", Baptiste Mélès (ed). *Les Études philosophiques* 1, nº 112, 2015, pp. 9-30.

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Finally, the principle of parsimony states that we should not multiply entities unnecessarily. When it is possible to reconstruct an entity from more fundamental notions, it is then possible to dispense with the entity thus reconstructed. The aim is then to apply the method of analysis to determine the ultimate constituents of reality.

In the new logic, Brunschvicg made a clear distinction between its philosophical uses and its purely technical interests. Peano's work — which was so important to Russell — came at the end of a process of increasing rigor in mathematics throughout the 19th century. Thus, les *Étapes de la philosophie mathématique* agree that when Peano "rewrites mathematics in this way" he is "rethinking" them[(11)] and the reason why this rewriting is necessary is internal to the field of mathematics. It is therefore a logical reflection motivated by a technical problem that does not bother Brunschvicg. On the other hand, this strictly mathematical motivation seems to be overshadowed in Russell by other considerations. As Dufumier pointed out to Brunschvicg, "Russell's dogmatism lies above all in doctrinal exposition" and since he is content "to record results" the reader is kept far from the "research" which, in fact, makes them so valuable [(12)]. Brunschvicg replies that the problem with logicism lies in the fact that it "places its point of departure in the highest generality"[(13)], which prevents it from understanding what is going on inside mathematics. Solving a problem has nothing to do with stating *a posteriori* the notions from which the notions involved could be formulated. "Symbolic logic, intervening like poetic art after the spontaneous works of genius, can only consecrate victory or record defeat"[(14)]. The attempt to reduce the totality of mathematics to logic would therefore seem to be only philosophical, or at least not mathematical in the strictest sense of the term. This is how the "transition from the method of logistics to its system"[(15)] takes place. The materials used by both are identical, but the objectives are different. Brunschvicg's rejection of one does not necessarily include a rejection of the other.

[(11)] Brunschvicg, Léon. *The Stages of Mathematical Philosophy*. A. Blanchard. Paris, 1972, p. 381.

[(12)] Brunschvicg, Léon. *Écrits philosophiques, tome III : Science-religion*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1958, p. 91.

[(13)] *Ibid*, p. 102.

[(14)] Brunschvicg, Léon. *The Stages of Mathematical Philosophy*. A. Blanchard. Paris, 1972, p. 426.

[(15)] *Ibid*, p. 381.

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> I believe, however, that if Mr. Russell had not begun by "swallowing whole" Moore's realism, if he had not transformed specific and generic ideas into self-sufficient entities, he would never have considered the advent of Cantor's theories, or the identification of logic and mathematics, as the great events of the century. [...] It is only fair to add that these observations are not aimed at Mr Russell's work in the strict sense of logic: I apologized, when I finished my book on logistics, for not having had the opportunity to say enough about the admiration I felt for this work. You have given me the opportunity to repeat this apology, and I take it with alacrity[16].

Moore's realism is the natural complement to Russell's logicism. It allows him to conceive of the primitive notions of logic in a realistic way and then, through analysis, to extend this realism to the whole of mathematics — if not even mechanics in the last part of the *Principles*. For Brunschvicg, this is the philosophical error par excellence. It consists in constructing a philosophical system on the basis of the state of knowledge of a given epoch and granting this construction an eternal value. In his historical works, which aim to determine the metaphysics of each philosophers on the basis of their conception of science, Brunschvicg associates Aristotle's doctrine of the incommunicability of *genus* with the distinction maintained by the Greeks between geometry and arithmetic, the Cartesian doctrine of clear and distinct ideas with his algebra, and Kant's critical philosophy with Newtonian mechanics. Then there is the question of sorting the wheat from the chaff within each of these works. To the philosophical tendency to build systems, les *Étapes* oppose a philosophy grounded on the insight that the methods of science are constantly being renewed, so that the very idea of ultimate categories of reality must be abandoned. Philosophy must become "immanente to science, with the aim of becoming aware of what intelligence and truth have been incorporated into it"[17]. It is therefore easy to understand the distinction between logistics taken as a method and logistics considered as a system, and the rejection of Moore's realism. In Brunschvicg's strictly historical perspective, there is little point in trying to fix the meaning of certain

[16] Brunschvicg, Léon. *Écrits philosophiques, tome III : Science-religion*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1958, pp. 93-94.

[17] Brunschvicg, Léon. *The Stages of Mathematical Philosophy*. A. Blanchard. Paris, 1972, p. 426.

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terms definitively. It would then be possible to oppose a dynamic philosophy of history to a dogmatic and fixed logicism.

With regard to the definitive nature of the analysis and the possibility that the philosopher could go back to primitive notions, it should be noted that this is a thesis that Russell quickly abandoned in the course of his philosophical career, as Brunschvicg knew[18]. Thus, when he defended logical atomism, there was no longer any question of logical atoms, which should necessarily be located at the most fundamental level of reality, as the biological metaphor shows:

> What are taken as unanalysed units in one stage are themselves exhibited as complex structures in the next stage. The skeleton is composed of bones, the bones of cells, the cells of molecules, the molecules of atoms, the atoms of electrons, positrons, and neutrons; further analysis is as yet conjectural. Bones, molecules, atoms, and electrons may each be treated, for certain purposes, as if they were unanalysable units devoid of structure, but at no stage is there any positive reason to suppose that this is in fact the case[19].

In short, Russell's method of analysis does not necessarily go hand in hand with the project of a complete and definitive analysis. Consequently, of the five principles identified by Vuillemin, it is the principles of analysis and parsimony that will have the greatest importance in the Russellian corpus, to the detriment of the others. Brunschvicg's criticism therefore seems to miss its target, if not to affect only the *Principles*. Once the non-definitive nature of the analysis has been accepted, Russell seems, *a priori,* to be in a better position to take charge of the renewal of scientific methods that history manifests. On the other hand, he interprets analysis realistically, and nothing is more alien to him than the Kantianism advocated by Brunschvicg. For Russell, realism was the only way of taking science seriously and escaping scepticism, and Kantianism, which he always considered as some form of psychologism, never found favour with him. The first step is to examine the way in which the question of realism pits the two philosophers against each other, and then to ask whether admitting the provisional nature of analysis allows Russell to escape Brunschvicg's criticisms, or whether the latters have simply lost any relevance.

[18] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 152.

[19] Russell, Bertrand. *My Philosophical Development*. London: Unwin Books, 1975, p. 165.

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§ 2. — Areas of disagreement: analysis and empiricism.

If it is not possible to oppose Brunschvicg's dynamic philosophy to Russell's static approach, then it seems that the reference to Kant could constitute another way of approaching the question. The reference to Moore, decried by the French philosopher, was in fact a critical tool against all forms of idealist philosophy at the beginning of Russell's philosophical career. If we now turn to the very end of Russell's intellectual career, the criticism of Kant is still present. In the introduction to *Human Knowledge,* Russell declares that we should start from the results of the natural sciences, disregarding any prejudices that might arise from adherence to any epistemological theses[(20)]. The point here is to criticize Kantianism, in which he sees not a Copernican revolution but a "Ptolemaic counter-revolution" that restores the anthropocentrism that Copernicus had dispelled[(21)]. If human knowledge is always rooted in subjective experience, insofar as the results of the natural sciences are presented as objective, it is up to the philosopher to understand the way in which this objectification takes place. There would then be a contradiction between the Kantian conception of the subject and the fact of science. However, there is a sense in which it is possible to say that Brunschvicg's Kantianism is not directly opposed to Russell's realist intuitions. Russell contrasts the specular conception with the pragmatist conception of the mind[(22)]. Both options point to something correct: the mind reflects facts and, insofar as facts generate habits, the subject is active in finding out what general relationships facts have with each other. However, this distinction is resolved in favor of the specular conception insofar as knowledge is irreducible to behavior, so that it is possible to say, somewhat bluntly, that this activity of the subject is of interest, from the point of view of truth, only insofar as it enables real relationships to be brought to light[(23)]. Russell's rejection of idealism, in the name of the natural sciences, is therefore complete.

Yet *Human Experience* repeatedly affirms the thesis that "transcendental idealism does not exclude empirical realism"[(24)].

[(20)] Russell, Bertrand. *Human Knowledge — Its Scope and Limits*. Philosophical Texts. Paris: Vrin, 2002, p. 31.

[(21)] *Ibid*, p. 34.

[(22)] *Ibid*, pp. 457-8.

[(23)] *Ibid*, p. 138.

[(24)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 307.

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Brunschvicg's interpretation of Kant is not an isolated one. Russell's accusation of anthropomorphism would then depend on an erroneous psychological interpretation of Kant that misses its target. However, it is important to understand how Brunschvicg manages to conceive the subject's activity without falling into psychologism and abandoning empirical realism.

> Kantian idealism does not form a couple with realism. It does not respond to the solution of an alternative whose two terms are determined in the abstract by their mutual opposition. In fact, whereas realism stops at the appearance that makes being the object of an immediate presentation, transcendental idealism sees this presentation as the product of an elaboration which, in order to be understood, requires us to penetrate into the depths of the activity proper to the knowing subject. We will cease to compare the mind to a mirror, to ask it for a faithful image of being in itself. The object of knowledge bears the imprint, impossible to eliminate, the definitive mark, of the original activity that constitutes it as an object[(25)].

The solution is to reject the alternative, on which Russell's critique is fundamentally grounded: it is not a question of asking whether such and such an object exists in consciousness or in a world that is external to the subject. If the debate were posed in these terms, Brunschvicg's position would simply be contradictory. When, during a session of the *Société française de philosophie*, Dominique Parodi opposed idealism on the grounds that the mind "comes up against heterogeneous matter" that forces it to question itself and develop new theories, Brunschvicg did not take this remark as a major objection[(26)]. The real terrain on which Brunschvicg's philosophy, which he presents as the "intellectual awareness of knowledge"[(27)], should be read is that of the history of science. On the other hand, in the field of common experience, critical idealism seems to limp along, as when it leads its defender to assert that *autrui* "is a piece in the system of my judgements of

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[(25)] *Ibid*, p. 301.

[(26)] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 289.

[(27)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 318.

existence"[28]. To which his interlocutor is at liberty to reply: "I do not admit in any way that I can be reduced to a judgement of the existence of Mr Brunschvicg's consciousness"[29].

On the theoretical level, then, both philosophers agree that the subject is active to a certain extent. On the other hand, Brunschvicg emphasises the fact that this activity produces *sui generis* realities throughout history. Reducing this creativity to the application of an *organon* — such as Mill's *Logic* or the postulates that Russell would later set out in *Human Knowledge* — is futile[30]. To take one example, when Jean Perrin compiled the evidence for molecular reality, and began by stating the "law of definite proportions" according to which "the proportion in which two elements combine cannot vary continuously"[31], he was working, on the one hand, with data that were intelligible only insofar as they had been developed through experimentation and, on the other, he conceived a new way of fitting them into a theory — by detecting the existence of a discrete phenomenon in a reality that seemed continuous. This makes it possible to give meaning to the seemingly absurd idea that "the existence of nature as a unity of interrelated phenomena depends on thought"[32]. The very formulation of a problem presupposes terms that have meaning only for the mind, and leads to the creation of a new entity — the theory — which is itself irreducible to the data of the situation that gave rise to it. To reinterpret this idea by arguing that any theoretical activity presupposes a background theory is completely foreign to Brunschvicg's philosophy, and in fact presupposes a conception of experience that he rejects, as we shall see. In short, it is not a question of placing nature under the dependence of the mind, but of recognizing the existence of a type of reality specific to theoretical constructions.

It therefore seems possible to understand how it is possible for Brunschvicg to argue without absurdity against Russell that critical idealism is a form of empirical realism. The question that then arises is what kind of reality should be accorded to these theories,

[28] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 321

[29] *Ibid*, p. 327.

[30] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 77.

[31] Perrin, Jean. *Les atomes*. Champs sciences. Paris: Flammarion, 2014, p. 43.

[32] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 301.

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which, although produced by the subject, do not call empirical realism into question. Before going any further in this direction, let us take a diversion, for while Russell accuses idealism of being incapable of explaining how science works, Brunschvicg returns the accusation. Thus, Russell's conception of mathematics would necessarily lead him to establish a gap between reality and the theories supposed to represent it — and to miss the important lesson of Kantianism. The preface to *Human Knowledge* formulates the problem that will occupy the author: understanding how important theories can be developed from the very poor empirical basis available to us[33]. This discrepancy between the poverty of the data and the importance of the speculative construction then lead Russell to consider the intrinsically conjectural character of all theoretical knowledge[34]. This small dose of scepticism is, however, in his eyes, in complete accord with the empiricism he professes elsewhere.

This position is opposed to the Kantian thesis that theories make it possible to arrive at "a necessity [...] in which the proper character of reason is recognised"[35], which cannot be reduced either to probable knowledge or to logical necessity. Brunschvicg locates the origin of this disagreement with Russell in the latter's philosophy of mathematics. Russell would have conceived of mathematics and reality as two different types of heterogeneous entities, comparable to Aristotelian form and matter, so that the development of an abstract mathematical theory from experience could only be conceived as a distancing from the empirical basis[36]. The heart of the matter is the principle of analysis. According to this principle, entities must be reconstructed using the resources of logic. To use Brunschvicg's Aristotelian notions, it is a matter of going back to the order of science to find the order of things, to go back from the natural integers to the logical notions, or from a science to its fundamental notions. Once the analysis has been completed, the philosopher is faced with a Cornelian dilemma that faced the mechanics of the nineteenth century[37]. The first option is to grant

[33] Russell, Bertrand. *Human Knowledge — Its Scope and Limits*. Philosophical Texts. Paris: Vrin, 2002, p. 31.
[34] *Ibid*, pp. 36 and 543.
[35] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 59.
[36] *Ibid*, p. 468.
[37] *Ibid*, p. 440.

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the first principles of a science — mechanics in this case — the status of rational evidences, and thus to follow Lagrange, who considered that his *Mécaniqe analytique* constituted a propoer chapter of Analysis. The second is to declare that mechanics is an empirical science and to deny the rational specificity of these principles, thus creating an unbridgeable gap between experience and theoretical constructs. The natural sciences are thus struck by an irreducible contingency. It should be noted that between the two branches of the dilemma there is no place for the specifically Kantian solution of real necessity. In Brunschvicg's view, this is obviously a false dilemma, and the virtue of Kantianism is that it avoids it.

> Until the Kantian critique, philosophy was condemned to oscillate between two conceptions equally inadequate to the fruitfulness of modern thought: Hume's psychological atomism and Wolff's scholastic panlogism. From our point of view, we should say even more. The uncertainty and confusion created by the divergent paths taken by Leibnizian thought did not cease with Kantianism; quite the contrary. In Kant himself, the spirit of critical reform was altered to the point of allowing either a return to a system of conceptual and ontological hierarchy at the same time, or a return to a representation of isolated and discontinuous elements[38].

It should be noted here that the two conceptions reflect each other. Conceiving of the sciences — whether natural sciences or mathematics — through the prism of analysis leads us to question the status of primary notions in their relation to reality. Now, insofar as the natural sciences are obviously not *a priori*, it seems necessary to consider that the application of mathematics to nature has something contingent about it. The conception of analysis and experience are therefore two sides of the same coin. The Aristotelian dualism of the form of matter is found in "the disproportion between knowledge of a world that appears endowed with a stable organisation and the bare form of affirmation that

[38] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 17.

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is like the root of perception"[39]. The key to Brunschvicg's critical idealism lies in his rejection of the idea that experience should be conceived as something radically heterogeneous to thought in favour of "human experience", which gives its title to his book on physics. Nature is not a fundamentally irrational entity that resists thought. Experience itself already possesses a form of unity that a philosophical approach that proceeds by analysis dissolves at its peril.

> Causality cannot be compared to a rope that can be grasped and drawn before being rolled around the parcels it is used to unite, because causality in no way has the function of linking objects that have already been given. It is exercised by an act of linking that will give the objects, without the linking and the object being able to be considered and represented separately[40].

What makes Brunschvicg's thesis counterintuitive is nothing other than the ease with which it is possible to consider mathematics as a hypothetico-deductive system on the one hand, and brute experience on the other. Once the two are separated, the question of how one applies to the other necessarily arises, leading to the two antagonistic conceptions mentioned above. Our two authors both claim the title of realist. For Russell, Brunschvicg's realism is simply not a realism — or rather, it is incomprehensible — whereas for Brunschvicg, Russell's realism is naive and leads him to an erroneous conception of experience. It would seem, then, that there is a deeper disagreement between the two philosophers on the question of analysis, and that this disagreement should be assessed by examining the relationship between the natural sciences and the experience that is supposed to follow from them. In this way, it will also be possible to clarify the nature of the empirical realism that Brunschvicg claims.

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[39] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 468.
[40] *Ibid*, p. 468.

§ 3. — Duhem and Russell, their "common inspiration".

Brunschvicg states on several occasions that there is a "common inspiration" between Russell and Duhem[41], that is to be found in the essentially deductive conception of mathematics that they share. In the case of Russell, we have seen that Brunschvicg attributed his conception to the method of analysis that he adopted. As far as Duhem is concerned, it is necessary to reformulate his thesis to understand where this conception of mathematics operates. In short, physical theories do not refer to reality directly but via a set of necessarily approximate measurement results. Consequently, a physical theory is a symbolic construction that makes it possible to systematise a body of information in the simplest possible way. What's more, a physical theory always involves a large number of factors, so that in the event of an experimental setback, it is not necessarily possible to pinpoint the problem.

> Physics is not a machine that can be taken apart [...] The watchmaker who is given a watch that doesn't work separates out all the cogs and examines them one by one until he has found the one that is faulty or broken; the doctor to whom a patient is presented cannot dissect him in order to make his diagnosis; he has to guess at the seat of the illness simply by inspecting the effects produced on the whole body; it is the latter, not the former, that the physicist charged with straightening out a flawed theory resembles. *Experimentum crucis* is impossible in physics[42].

Duhem's thesis has two sides. On the one hand, crucial experience is impossible. On the other hand, theories are underdetermined by sensible experience, so that a plurality of theories can correspond to each set of observations. Given a series of observations represented by a cloud of points, it is possible to pass a plurality of curves in the vicinity of all these points. Although Russell does not generally cite Duhem, he does use an example in the same vein in *Human Knowledge*. The argument consists of maintaining that it is possible, with the help of Fourier series, to propose

[41] See Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 85 and *L'Expérience humaine et la causalité physique*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 452.

[42] *Ibid*, quoted on p. 442.

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a theory that simulates the results already obtained by a theory but predicts radically divergent new results[43]. There is therefore nothing to say whether the world is governed by the laws exhibited so far or by these divergent laws that it is possible to formulate. Russell thus agrees with Hume's conclusions and gives probability to the information provided by the laws[44]. In a sense, both arguments amount to relativising human knowledge of the world by pointing out the possibility that it is in fact constituted in a radically different way from what our theories say. In both cases, there is something formally irreproachable about the analysis: it is always possible to find competing theories. How are we to understand Brunschvicg's comparison between Russell and Duhem? Both arguments focus on the fact that there is a plurality of mathematical entities that can account for the information we have and add another set of predictions of their own. This amounts, in short, to introducing a distance between the world and the theory. Since the principles of the theory cannot be *a priori*, they must be *a posteriori*, and there must therefore be an element of arbitrariness in the choice of theory.

For Brunschvicg, the drama of Kantian philosophy torn between Wolff's panlogism and Hume's empiricism is replayed in Russell's philosophy[45]. However, the letter of Kant's solution, which is bound up with Aristotelian logic and the frameworks of Newtonian mechanics, cannot serve as a remedy for Brunschvicg[46], he chooses another path, which has several complementary facets. First of all, it should be noted that his solution does not claim to be able to confer on theories a greater certainty than that conferred by empiricism. In both cases, it is a question of betting on the future on the basis of information from the past, and there is no rational argument to show that it would be contradictory and logically impossible for the laws of nature to suddenly change[47]. Between the indubitable truth and the contingency of experience, there is nevertheless a place for Kant's real necessity.

The first aspect of Brunschvicg's thesis lies in the field of psychology. He opposes the atomism of the fact of consciousness, the

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[43] Russell, Bertrand. *Human Knowledge — Its Scope and Limits*. Philosophical Texts. Paris: Vrin, 2002, p. 514.

[44] *Ibid*, p. 35.

[45] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, pp. 451-2.

[46] *Ibid*, p. 452.

[47] *Ibid*, pp. 65-66.

idea that experience is devoid of any structure, with a psychology of judgement[48]. There is no such thing as brute experience, and to experience something is to judge that something exists. If it is then possible to analyse and separate *a posteriori* what the judgement links, such a philosophical analysis always runs the risk of being arbitrary. The opposition of raw experience and theory is a by-product of the method of analysis, as can be seen from the fact that the simple delimitation of an *explanandum* already constitutes a conceptual elaboration in itself.

The second aspect that particularly sets him against Duhem is his rejection of the distinction, underlying his thesis, between symbolic mathematical construction and judgement of existence. The thesis of *Physical Theory* depends on the opposition between these two notions, which allows its author to reduce the ontological value of theories. The domain of truth seems to be reserved for judgements of existence, of which everyday life provides paradigmatic examples, whereas theoretical principles, whose function is to systematise data that are already mathematical, can only said to be practical. Historically, this distinction has been embodied in the distinction between the mathematical astronomer and the physicist, who is responsible for finding the causes[49]. In contrast, Brunschvicg argues that rationality makes it possible to determine what really exists. This idea underpins the history of science, and in particular the Copernican revolution. To use Brunschvicg's favorite example, there are not two suns, the one of sensible experience, which measures a foot in diameter, and the one whose nature is revealed by reason[50]. Idealism requires us to choose between intelligence and sensibility. This is not an absurd idealism, and there is something trivial about the remark. On the other hand, it does make it possible to question the way in which common sense uses the term 'exist' or the notion of causality. The Cartesian revolution, hailed by Brunschvicg, consists of a "transfer of evidence" from the sensible to the intelligence[51]. In both cases, if we are to trust rationality — which is embodied in the history of science —

[48] *Ibid*, p. 466.
[49] *Ibid*, p. 172.
[50] Brunschvicg, Léon. *The Progress of Consciousness in Western Philosophy*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1927, p. 204.
[51] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 7.

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it makes no sense to look to ordinary discourse for a way to question what the natural sciences are achieving. The true meanings of causality and existence are revealed by mathematical physics. This does not, however, commit Brunschvicg to a kind of Pythagorean ontology. Such an interpretation would still depend on the error he denounces: it would mean continuing to consider as paradigmatic the objects of everyday experience, to judge mathematical physics on the basis of them, and to hypostasize numbers. This theoretical gesture is simply another form of anthropomorphism.

> [Langevin] has shown us admirably how, at the starting point, the images of vulgar representation and the simple forms of mathematics are supports for science, and how there comes a time when they abandon it, and turn into obstacles for those who persist in trusting them. And there is a profound reason for this: it is that neither the model of sensitive imagination, nor the ideal of mathematical expression, is homogeneous to itself; on both sides, we come up against a duality in the definition of what should be, either pure intelligence, or pure experience[(52)].

And Brunschvicg concludes by quoting Jean Perrin: "I think it is anthropomorphism to try to draw what happens inside a hydrogen atom". Basically, Duhem's thesis is based on establishing common experience as the norm of existence. If a theoretical construct deviates from this norm, it cannot claim to exist. It is important to note that while, at first glance, the atom — and the way in which it seems easy to represent it to us — may seem to satisfy the requirements of common sense, to be an entity like tables or chairs, this is not the issue for Brunschvicg. For his, there is no opposition between asserting that something is and grasping it by means of a mathematical construction. This dilemma is implicitly based, in Duhem, on the idea that mathematicians have to manipulate abstract ideas, which have no real consistency, whereas they enable us to understand the world by establishing "connections of intellectual relations"[(53)]. The impression that theoretical constructs are far removed from reality therefore depends on a misconception of

[(52)] Brunschvicg, Léon. *Écrits philosophiques, tome III : Science-religion*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1958, pp. 149.

[(53)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 167.

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mathematisation and, more fundamentally, on a misconception of experience. The explanation, then, consists neither in inventing a mysterious entity, on the model of the objects of common sense, which would lie beyond sensible experience, nor in imposing an abstract mathematical schema on a restive reality — two symmetrical errors — but in linking together certain aspects of reality in such a way as to say what is. The empirical realism claimed by critical idealism finds its explanation here.

The third aspect of Brunschvicg's response leads him to associate Duhem and Russell insofar as the latter two conceive of mathematics solely from the point of view of deduction. After Hume, Russell was able to write that "facts, however numerous and well selected, never imply other facts"[(54)]. This is obvious, he adds, "to anyone who has grasped the nature of deductive arguments". Without going into the details of the argument, when B is deduced from A, it is logically contradictory for A to take place without B taking place. But it is never logically contradictory for two events to follow each other or not. In Russell's case, the deductions made within logic are therefore heterogeneous — as in Hume's case — to reality. This heterogeneity is also found in Duhem, in that the deduction allowed by a physical theory never reaches reality. Conversely, for Brunschvicg reality is not heterogeneous to the "ideal of mathematical expression". This does not, of course, mean that science is *a priori*. If the *explicandum* is no less intellectually rich than the *explicans*, it is possible to conceive of the development of a theory neither as the elaboration of deductive reasoning, which does not constrain an experience that escapes them, nor as a symbolic construction alien to the question of existence — nor even as the invention of *posits*, as Quine have conceived it. The distance between reality and theory disappears if the causal relation is conceived as a way of linking together the most significant features of reality. For Brunschvicg, Russell and Duhem missed this function played by mathematics.

For all that, Brunschvicg's rationalism does not claim to provide more logical guarantees for knowledge, and nothing is more alien to him than a naively dogmatic conception of the experimental method. He quotes approvingly Schuster when he write: "a theory is worthless if it cannot be shown to be false"[(55)]. This theme is

[(54)] Russell, Bertrand. *Human Knowledge — Its Scope and Limits*. Philosophical Texts. Paris: Vrin, 2002, p. 212.

[(55)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 446.

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nevertheless marginal in his work. Yet it seems that his conception of experience allows him to reconcile a certain form of fallibilism with the idea of real necessity. In fact, when the mind judges the existence of an entity, it is affirming the existence of an already structured reality. It is then easy to see how even an erroneous theory captures certain real relationships. The question of theoretical error becomes dramatic when the existence of *posits* is thought of on the model of common sense entities, in which case the third-excluded applies, and an entity must be said to exist or not to exist. Brunschvicg's perspective does not prevent him from asserting the radical falsity of a theory. On the other hand, it does weaken the notion of truth, which is not a problem if the scientific enterprise is understood as a perpetual historical effort. The question of what science would be like at the end of its history leads back to the equivocation of common sense. "It is not philosophically acceptable for the human mind to be equipped to have the last word on things"[(56)].

So how did Brunschvicg understand Duhem's use of convenience? It is certain that conditions of simplicity come into play in the determination of a law and that empirical data do not univocally dictate which curve to choose. Brunschvicg's answer is twofold. The first is simply to admit part of Duhem's thesis, namely that conditions of simplicity come into play in natural sciences. On the other hand, this is not an argument against empirical realism. It only follows that "the notions to which the analysis leads and from which the synthesis will be based" are not "susceptible, I do not even say of an intrinsic justification, but of an intuitive representation, or even of a clear and distinct definition that would allow them to be isolated as elements"[(57)]. Duhem's criticism is in fact directed at a conception of physics that would have theories bring to light entities that would be like the bricks that make up reality, whereas intelligence does not proceed in this way[(58)]. Here again, the break between ordinary knowledge and mathematical knowledge plays a crucial role in Brunschvicg's construction. Finally, pragmatism cannot take advantage of this thesis either[(59)].

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[(56)] *Ibid*, quoted on p. 392.
[(57)] *Ibid*, p. 600.
[(58)] *Ibid*, p. 129.
[(59)] *Ibid*, p. 433.

> Need I add that this is no concession to pragmatism either? Precisely because what pragmatism lacks, whatever form it takes, is the ability to recognise the specific character of reason, a speculative and disinterested function that carries with it a capacity for *internal* justification, for verification, in the full sense of the word[(60)].

The fact that the notion of "convenience" comes into play does not provide an argument for the pragmatist, insofar as "convenience is a relative, and one would like there to be convenience in itself"[(61)]. Against instrumentalist interpretations that would like to substitute the notion of convenience for the notion of truth, based on Duhem's thesis, Brunschvicg stresses the fact that the notion of simplicity that comes into play here — especially when theories reach a certain level of sophistication — refers to a strictly intellectual notion and only has meaning in relation to correlative notions, such as that of understanding, which only have value for the mind. While this *sui generis* notion of simplicity would require further elaboration, it is possible simply to underline the equivocality of the term when it is used to designate both the relationship of an individual to his or her environment and the choice of a highly speculative theoretical hypothesis.

It is impossible to suspect Russell of pragmatism, but this diversion brings us closer to a final point on which he and Brunschvicg were in total disagreement — at the time of *Human Knowledge* at least. Russell's last major text emphasises man's place in nature, particularly as regards the origins and development of his knowledge of the world[(62)]. Conversely, for Brunschvicg, the mind and its creativity manifests themselves in the history of sciences, which he sees as an argument in favour of a spiritualism that distinguishes man from the rest of nature. "Man is not a given"[(63)]. Perhaps the most profound opposition between the two philosophers lies in the fact that, when it comes to science, Russell reduces history to nature, whereas Brunschvicg sees it as the very essence of the human being and the foundation of his own spiritualism.

[(60)] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 98.

[(61)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 450.

[(62)] Russell, Bertrand. *Human Knowledge — Its Scope and Limits*. Philosophical Texts. Paris: Vrin, 2002, p. 543.

[(63)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, p. 642.

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§ — Conclusion.

The discussion we have outlined is not yet a thing of the past. In his *Kant Lectures* (1999), Michael Friedman argued for a return to neo-Kantianism, which would make it possible to escape from the impasses of a philosophy of science trapped between Kuhnian historiography and its relativist avatars, and Quine's holism[64]. Although Michael Friedman does not seem to be interested in Brunschvicg's case, the latter's work is not far removed from these questions insofar as he discussed the theses of Duhem and Russell, which indirectly structure these debates. It is well known that in the *Logical Structure of the World* Carnap the project that Russell outlined in *Our Knowledge of the External World*, and that the theoretical projects of Quine and Kuhn were conceived in relation to their reading of Carnap's projects. It is therefore not surprising that Brunschvicg, who discussed both Duhem's and Russell's theses, was already discussing problems that interested Quine and Kuhn.

We have emphasised the role played by the comparison between ordinary knowledge and scientific knowledge in Russell's work. It is possible to show that Quine subscribes to a similar thesis.

> Considered relative to our surface irritations, which exhaust our clues to an external world, the molecules and their extraordinary ilk are thus much on a par with the most ordinary physical objects. The positing of those extraordinary things is just a vivid analogue of the positing or acknowledging of ordinary things: vivid in that the physicist audibly posits them for recognized reasons, whereas the hypothesis of ordinary things is shrouded in prehistory[65].

By considering theories from the point of view of their ontology, Quine in fact dismisses what, in Brunschvicg's eyes, is precisely what should hold the philosopher's attention. For Quine, physical theories are *more or less on* the same level as everyday entities; for Brunschvicg, the entire rationality of the sciences lies in this gap between science and common sense. It is therefore possible to

[64] Friedman, Michael. *Dynamics of Reason. The 1999 Kant Lectures at Stanford University*. CSLI Publications. California, 2001.

[65] Quine, Willard Van Orman. *The Word and the Thing*. Flammarion. Paris, 2010, p. 53 (emphasis added).

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understand why Quine has no difficulty in considering that there is an analogy between the gods of Olympus and the *posits* of a theory, whereas such a comparison would be completely foreign to Brunschvicg. To consider a theory from the point of view of its ontology is to dismiss the conceptual ingenuity that underlies it and makes it rational.

It seems possible to compare the case of Quine with that of Mill, to whom Brunschvicg addressed the following criticism: the empiricism displayed in the *System of Logic* is only a façade, since when it comes to establishing rules of induction, its author has no difficulty in abstracting from the rigid rules to which the sciences are supposed to conform themselves[(66)]. Brunschvicg can therefore claim to be more of an empiricist than Mil, insofar as he expects the history of science to tell him how human rationality really works. Quine has a similar duality between the naturalistic credit given to the sciences, on the one hand, and a determined, perhaps naively realistic, way of conceiving the ontological task of these sciences, on the other. The rejection of such a duality is characteristic of Brunschvicg's philosophy: philosophy "becomes immanent to science with the aim of becoming aware of what intelligence and truth have been incorporated into it" without seeking to develop criteria of rationality, of ontological commitment, which would transcend the scientific enterprise itself[(67)].

In the same way, it is possible to note that in many passages of his work, Kuhn's argumentation is based on a similar schema. His rejection of the neo-positivist conception of scientific knowledge, and the idea that there can be no algorithmic procedure for judging the merits of a theory, play a pivotal role in his conception of history[(68)]. Rejecting a certain reading of the neo-positivist is not incompatible with a historical approach that attempts to uncover the mechanisms by which the rationality of certain theoretical gestures is manifested. This is a real *leitmotif* of Brunschvicg's philosophy: very often scepticism stems from an erroneous and over-ambitious conception of the way in which the sciences are supposed to proceed.

[(66)] Brunschvicg, Léon. *Human Experience and Physical Causality*. Bibliothèque de philosophie contemporaine. Paris: Félix Alcan, 1922, pp. 67 ff.

[(67)] Brunschvicg, Léon. *The Stages of Mathematical Philosophy*. A. Blanchard. Paris, 1972, p. 427.

[(68)] Kuhn, Thomas, *The essential tension: selected studies in scientific tradition and change*. Chicago University Press. Chicago, 1977, p. 326.

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> Is intelligence capable of *understanding*? The affirmative answer, which has been much contested in our day, would, in our opinion, be obvious if we could first distinguish between a false ideal of intelligence and its true reality[(69)].

In both cases, then, it seems that the sceptical consequences that can be drawn from Quine's or Kuhn's theses stem from a purely philosophical conception, independent of the history of scientific rationality, an approach that Brunschvicg constantly warned against throughout his work.

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[(69)] Brunschvicg, Léon. *Écrits philosophiques, tome II : L'orientation du rationalisme*. Bibliothèque de philosophie contemporaine. Paris: PUF, 1954, p. 285.

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The mathematical philosophy of Maximilien Winter (1871 – 1935)

Didier Lesesvre

Abstract. Maximilien Winter was at the heart of French epistemology, shaping it by co-founding the *Revue de métaphysique et de morale* alongside Brunschvicg, Couturat, Halévy, and Léon. This study explores his unique philosophy through an examination of his extensive contributions to the *Revue*.

Winter's philosophy directly opposed the dominant metaphysics, positivism, and logicism of his time. Instead, he advocated for a critical and historical epistemology of ideas and methods. For Winter, the role of philosophy was to clarify and organize the principles of science through a technical analysis of scientific texts and the development of ideas.

Keywords. Maximilien Winter, epistemology, history of epistemology, principles of science, opposition to metaphysics, opposition to positivisme

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§ 1. — Introduction: The purpose of the philosophy of science.

Overview of his life, influences and orientations. Maximilien Winter (1871 – 1935) studied at Lycée Condorcet in Paris, where he became friend with Léon Brunschvicg, Louis Couturat, Élie Halévy and Xavier Léon. This relationship led to the founding of the *Revue de métaphysique et de morale* in 1893, whose stated aim was to provide philosophy with an effective way to defend its status against

the overwhelming rise of the sciences, while opposing positivist speculation [26][(1)].

Winter studied law and became a lawyer, giving ground to some of his future work on legal controversies and their relations with mathematical logic ([4] and [6]). He had a passion for mathematics and philosophy of sciences, and published various articles on epistemology in the *Revue*. He has been influenced by his teacher Alphonse Darlu (see [24]), who tainted his philosophy with Platonic and neo-Kantian ideas. Winter defends a systematic philosophy of science, based on a dialectic whose rigor is modeled on logic and guarantees the objectivity of the arguments.

Because of these aspects, Winter is far from supporting the re-establishment of metaphysical and moral philosophy, ubiquitous in the editorial line of the *Revue*, which for him is, on the contrary, a pitfall to be avoided. His strong rejection of metaphysics or any argument based on absolute or theological postulates depict his interest in Auguste Comte and Hegel ([22]). However the philosophers of the *Revue* remain united by the acceptance of a central role of science in philosophy, although Winter does not share the visions of a superiority of philosophy over science — he gives on the contrary an essential place to the study of scientific works themselves. The young philosopher found his place in the *Revue*, which, unlike its counterparts, refrained from promoting any particular doctrine, and wished to become a relatively free forum for philosophy in France. Winter's affinity with the other founders of the *Revue* undoubtedly favored his participation.

Winter altogether seeks to rehabilitate the role of philosophy as a reflection on the principles of science against the positivist approach, to reintroduce scientific and technical practice against the metaphysical approach, tracing a path between the metaphysical orientations of the *Revue* and the positivism it faces.

[(1)] The provided reference focuses on the lives of Léon Brunschvicg, Élie Halévy and Xavier Léon. Little information is available about Maximilien Winter: his own writings — nearly forty years of articles published exclusively in the *Revue* — and the obituary dedicated to him; the articles the articles [24], [26] and [27], which deal essentially with the history and philosophy of the *Revue*; and two recent works: the book of Castellana [25], which embeds Winter into the historico-critical epistemology; and the study of Alunni [23], who shed a new light on the philosophy of Winter as "a central piece of the *surrationalist* connection between philosophy and mathematics" (our translation). We regret that we did not have the time to study in depth the philosopher's correspondence, which is preserved in the Victor Cousin library at the Sorbonne.

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An interest in modern and scientific issues. From his earliest writings, Winter devoted himself to an in-depth study of scientific theories, recounting the evolution of ideas, and focusing on the scientists' own judgments about their discipline and the fruitfulness of their methods. This proximity, which he defends throughout his work, between philosophy and science, between thought and its object, will be the common ground of his studies. The constant to-ing and fro-ing between the two worlds would remain the driving force behind his reflections, set up as the only legitimate way to unify science and philosophy, in line with a certain intuitionism dear to Kant and Poincaré.

Winter was particularly interested in the scientific upheavals of his time. Although the scientific subjects of his work appear eclectic (analysis, arithmetic, mechanics, quantum physics, relativity, biology, law), they are distinguished by a certain topicality. His studies focus on the contributions of new theories, both within the sciences and insofar as they call into question many philosophical positions: Frege's logic, Cantor's set theory, Hilbert's axiomatization, Heisenberg's quantum physics, Hadamard's analysis, and Einstein and De Sitter's relativity.

His research focuses on the axiomatization of science, seen through critical and rationalist glasses. The problem was in vogue at that time when logical theories were blossoming, and while originality was not a feature of his subject, his position was less common. Like Comte, Winter not only categorically rejects the metaphysical reasoning of the philosophers and mathematicians of the time, but, as Kant's heir, is careful not to sink on the other hand into the absolute logicism that Winter would later say was only a blinding moment for mathematicians and philosophers. On the contrary, his task is to reveal a new role for philosophy and history in the development of science.

An organizing and clarifying philosophy. Scientific and rationalist in spirit, Winter sets out to clarify the possible methods for studying the principles of science. He depicts the philosopher's mission as follows: "Philosophical thought is neither demonstrative nor merely critical; it is organizing, or more precisely systematic, and aims essentially, if not to define [the systematic principles of science] in the scientific sense of the word, at least to determine them sufficiently for the organization of our ideas" ([2], pp. 615-616). This approximation in determination, marking the

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mourning of absolute definitions, is a first sign of a philosophy that will remain deeply rooted in the practice of science in the making, and to which an organizing philosophy, however incomplete, can contribute. His *new conception of the philosophy of science* is spelled out in his seminal article [2]: "All sciences must strive to realize the type of certainty: A is A. But reality offers us only heterogeneous facts; the problem that science will have to solve will therefore be to find relations of identity between heterogeneous facts. But [...] it cannot be directly, only by means of detours, that the mind can establish relations of identity between heterogeneous facts. The task of the philosophy of science will therefore be to determine these detours of the human mind." ([2], p. 607) The role of the Winterian philosopher thus appears above all as a quest to clarify the principles underlying the sciences.

This does not mean embracing a philosophical relativism in which every object is bathed in a specific philosophy. On the contrary, "we admit [...] that there is room outside mathematics for different orders of things, for distinct methods: but we don't give up on establishing that the different principles on which methods are based are not arbitrary conceptions, but rational principles. The philosopher's task will not be to catalog the subterfuges of the human mind, but to rationally determine the principles on which the various methods are based, and to show their systematic sequence." ([2], p. 613) Winter's quest, then, is not merely to uncover principles existing in practice, but to identify their consistence and, if not to establish a system, at least to clarify their relationships.

Such a task is multifaceted, and throughout Winter's work it resembles an investigation into the philosophical postures governing science, followed by a discussion of the value of these principles, in order to avoid certain biases and identify new interpretations. More precisely, "there is an important role for the philosopher to play in science, without confusing it with the work of the scientist. [...] A small number of new attempts are indeed interesting discoveries, most are irrelevant and many are false. It is important that these attempts are sorted out, that a critic judges the theories as they are born. This criticism is to the sciences what dramatic criticism is to plays [...] and will be better done by a philosopher with a solid scientific education, than by a learned inventor [...] There is a second object of study for the philosopher, and that is the philosophical history of science, [...] history conceived as the

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very genesis of scientific theories, where the filiation of fundamental ideas would be established [[7], p. 326]. To sum up: "criticism of science, philosophical history of scientific theories, contribution to the formation of scientific methods: these are, in our view, the three forms in which philosophical thought manifests itself" ([7], p. 327). Winter's work instantiates a philosophy deeply rooted in science and its history.

A precise analysis of philosophical postures on science. Winter goes beyond the search for unification of the sciences, and instead places primary importance on the study of the variety of methods and principles. He points out that the syllogism, mathematical equality, the method of geometric superposition, experiments in physics and legal reasoning are all applications of this principle [of identity], yet the relationships between concepts, the equality of quantities, the coincidence of figures, the experimental method of physics and discussions of civil law are all very different methods. Consequently, to abstract from the sciences everything that cannot be reduced to the pure form of identity is to examine only one facet of the philosophical problem, to identify the element common to all the sciences; but philosophy must also determine the constituent and specific differences between principles and methods." ([2], p. 610) This eclecticism of subjects, reflecting a diversity that should not be masked, became and remained fundamental to his work.

Winter identifies three dominant philosophies and approaches to thinking about science and the search for its foundations, which he discusses and confronts throughout his work, and against which he draws his philosophy of science. We summarize these stances and dedicate this article to clarifying Winters' positions and arguments.

The first posture is metaphysics, a method of reflection that is thousands of years old and never exhausted, whose impact and ubiquity he notes as much in philosophy as in scientific reflection and in the arguments of scientists themselves. Winter calls metaphysics "any theory of perception and knowledge, or any system of explanation of the ultimate constitution of matter, which employs neither the mathematical method nor the experimental method of the physical sciences, and which will not expand the domain of positive science." ([3], pp. 509-591) This metaphysics is alien to

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experiments, and is for Winter rather a philosophy of first principles, rising into a theology above scientific reasoning and objects, but thereby outside science.

Positivism is a movement that emerged with Auguste Comte in the 19th century, establishing confrontation with facts and experimentation as the ultimate criterion for scientific judgment. In the positivist view, science aims to describe precisely the relationships observed in facts, in opposition to metaphysical speculation. It is thus of the same nature as science, using the same tools of exploration, and aiming to order the findings into laws of nature. Although Winter adopts a philosophy closer to science, refusing to dissociate himself from experiments and observations, he nevertheless gives the philosopher the role of thinking about the principles of science, clarifying and organizing them, going beyond mere positivist observation.

Finally, logic has progressed throughout history, becoming almost totally dominant at the beginning of the 20th century, after a dazzling conquest of most mathematical and philosophical circles. Its development within mathematics spread to other scientific fields and philosophical schools. This new logicism, driven by the axiomatic theories of Frege and Cantor, as well as the formalist efforts of the Hilbert school, postulates that formalization is the way to embrace the rules of truth and mechanize thought. This leads Winter to ask: "Shouldn't we assume that a logic, conceived in the same spirit, will one day, as it develops, absorb the philosophy of all the positive sciences?" ([3], p. 591) This enthusiasm for a formal, axiomatic science is very much present in Winter's work, but with restraint and restriction, considering logic as a tool for clarification and control rather than as a unique and sufficient method for founding the sciences.

Winter rejects each of these three conceptions of the philosophy of science, and sets himself the task of finding a middle ground between these radical positions. "The question is whether scientific judgment is purely abstract, general and analytic (in which case the very movement of scientific thought would tend to eliminate all empirical content), or whether it is synthetic (i.e. whether it concretely unites the law with a particular content that cannot be separated from it). Perhaps we should say that scientific judgment is neither analytic nor synthetic, but that it lies somewhere between the two. It is this latter thesis that we shall attempt to demonstrate."

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([1], p. 171). This declaration of intent, from his very first study, would remain his constant philosophical program.

Winter describes his philosophical approach as the "critical history" of ideas and methods. The scope of this approach is described by Winter as huge and not taken into consideration enough. It is about establishing a detailed understanding and close examination of scientific paradigms and objects, and judging them through the glasses of a historical epistemology. Winter claims philosophy's right to address technical issues, defending a philosophical way of thinking shaped by "[the] critique of science, [the] philosophical history of scientific theories, [and the] contribution to the formation of general methods and theories" ([7], p. 327). It is thus an a posteriori method, essentially based on a justification derived from experience, practice, understanding of the ideas of science and their fruitfulness.

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All Winter's work, his precise studies of scientific theories and his reflections on science, are geared towards comparing these methods, which are often combined in practice, in order to clarify their underlying values and principles. Winter restores a strong place to technical scientific studies, without limiting philosophy to a positivist attitude of description, and gives full scope to the reading of the history of science as the only way of thinking about scientific development by drawing out a natural movement of principles (see part 2). From the methods he outlines, he banishes metaphysics as an epistemology (see part 3), and severely restricts the philosophical and absolute framework of logicism without denying its primordial importance in reasoning (see part 4), paving the way for a new philosophy.

§ 2. — A philosophy close to science.

A critical philosophy for fruitful principles. The constant in Winter's philosophy is to maintain a close relationship with the object of science. This object is external to thought, and science only develops models of it, but it remains the criterion for judging principles and results. His stance could thus be described as scientific criticism, in line with a certain positivism. "The criticism keeps the experimental shape of the content to avoid that metaphysical

principles, mere condition of thought, are transformed into dialectic ideas" ([2], p. 619). Experience is not, however, the absolute criterion, as positivism claims: "philosophical positivism, which condemns human thought to becoming more and more limited, is belied by the very progress of science [...] Only the rational ideal which affirms the supremacy of calculation and the possible resolution of the universe into mathematical principles, conforms to the spirit of science, and can constitute a principle of enthusiasm and excitement in the search for scientific truth" ([3], p. 619).

The philosopher's role is not, therefore, to confine himself to the observation of facts, as positivism would claim. While it is not his role to interfere with the scientist's work in the application of methods and techniques, whose virtuosity he cannot match, "it is in the determination of new principles that the philosophical spirit intervenes" ([8], p. 912) and guides the scientist, helping to identify research directions, if not to shed new light on results and methods. Thus, "The overall exposition of the elementary principles of a fundamental science like Analysis, when it is made by a great mathematician, always has a philosophical scope" ([21], p. 607), marked by a critical retrospect on ideas. By combating a fragmented presentation of mathematics, which only serves to conceal its interest and distort its harmony, Hadamard is the model of the Winterian philosopher-mathematician, "striving to link solutions, by rational methods, to the fundamental principles" ([21], p. 608) that he extracts from the many problems and aspects of mathematics to illuminate its unity.

Critical philosophy is not just an abstract posture, but also the best way to judge the progress of science. Indeed, the real significance of new facts needs to be identified philosophically, by drawing out their fruitful principles independently of the emotions aroused within the field. He notes that, "when a new scientific theory appears, it sometimes determines our general ideas in a way that seems to shake the very foundations of science. Then, as time goes by, things settle down and sort themselves out. We realize that the scientific universe has not been turned upside down, but that a special truth has simply been added to the truths already known" ([14], p. 268). The position of the philosopher as thinker of the sciences thus guarantees a certain objectivity to scientific reflection, avoiding dazzlement and overly vivid conclusions. The exteriority of philosophical reflection frees thinking about science from its internal cultural conditioning. This presence is all the more necessary because, given the perpetual accumulation of new ideas and results in science, "it is important [...]

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that a critic judges theories at their birth. This criticism [...] will be better done by the philosopher with a solid scientific education, than by a learned inventor" ([7], p. 326). The importance of technical and scientific ability in understanding the essence of scientific work with sufficient finesse to be able to discuss it stands in stark contrast to many philosophical practices, which remain remote from technical details. Winter endorses this role throughout his work, analyzing highly technical details, synthesizing principles and judging many of the scientific developments of his time.

This reflection on principles should not, however, engulf science, but merely enlighten it to a necessarily limited extent: "whenever a problem can be solved with existing methods, by more or less complicated artifices of calculation, but without new principles, philosophy has absolutely nothing to do in this work [...] let us add that results of capital importance for science have been obtained in this way" ([8], p. 912). We must thus "determine [the principles] solely for the scientific use for which they are intended [...], highlight only those principles that are necessary for positive science, and seek nothing else in this work on elementary notions, than to give a sufficient determination of them for scientific use" ([8], p. 913). This new point of view, this reflexive critique, must therefore be carried out by identifying the principles that really have their utility, by seeking a "determination of them that is sufficient for scientific use" ([8], p. 913), affirming the criterion of utility as the philosopher's judge.

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A historical method for ordering ideas. Winter's chosen approach to this critique is the historical epistemology, studying the evolution and arrangement of ideas and principles in the course of the evolution of science. This posture preserves the link to the discipline, while retaining external attachments so as to be able to judge ideas and postulates with hindsight. It also goes beyond a simple description of results, seeking to determine the central principles governing theories, their evolution and their relationships. "Taking our inspiration from Mach, and seeking to apply to mathematics the critical-historical method that the eminent thinker developed in his study of the formation of the principles of mechanics, we shall sometimes seek to show how the idea evolved, and sometimes characterize, outside of all historical considerations, the system of elementary notions in their logical sequence. This method is legitimate, because a completely objective history of the sciences is

impossible: the only truly objective historical method would be the complete publication of everything that appears, a method whose absurdity is plain to see. Criticism will always intervene to discern the important works that must constitute the fundamental scientific theories." ([10], p. 495) This illustrates the historical-critical method used by Winter to achieve his aims of clarifying and organizing the principles and ideas of science. Aware of the accumulation of scientific knowledge, "scientific criticism may have yet another interest. By seeking to highlight the fundamental ideas found in the most recent theories, it can contribute to the work of *elementarization* of notions that is necessary for the progress of science. Simplification is an ongoing process needed in parallel to the growth of knowledge." ([10], p. 496).

On the other hand, "we know that a certain implicit philosophy guides the scholar, if not when he seeks to solve clearly stated problems, at least in the choices of problems he addresses, and it is the axioms of this implicit philosophy that it would be interesting to uncover and criticize" ([14], p. 276). This remark constitutes, as we said, the keystone of Winter's philosophical edifice: a quest to clarify and organize the often implicit principles of the sciences. Such a philosophy does not claim to determine the ultimate principles of thought, but the scientist must gain from awareness of this underlying philosophy, complementing his technical virtuosity. Winter systematically shows the fruits of such historical study, for example in his study of the history of concepts and methods in number theory, where "most of the great ideas that transformed algebra and analysis [...] were also fruitful in arithmetic" ([7], p. 323). It is in this way that critical philosophy revives the unity of the world, fragmented in its study by science, by drawing out constant generalizations, as he shows on the example of function theory ([13]), where the advent and evolution of functional calculus, seemingly disordered and advancing in various directions, follow unifying principles: the philosopher's task has been to bring them to light. The greater scope and penetration conferred by philosophical exploration has been echoed historically by mathematicians such as Hadamard, Leibniz and Poincaré, whose scientific impact has been huge. The philosophical and critical study of the evolution of ideas and methods, for example, reveals the profound links between analysis and arithmetic, yet so distinct at first and in the inner practice of the scientist, thus revealing a larger and more complete whole formed by them, whose parts are interdependent ([7],

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p. 344). Another example is provided by Weyl's work, which "is above all an effort to logically adjust the principles of differential and kinematic geometry to the theory of relativity" ([16], p. 28). More generally, "the philosopher's task is to justify the work of science by showing that the principles on which science rests are not an incoherent set of arbitrary symbols, but form a rational organization" ([2], p. 620), made explicit by the philosopher. This historical-critical approach thus appears fundamental, both to shed light on the evolution of mathematics and to contribute to the associated philosophical reflection, which "naturally emerges from the history of science itself" ([15], p. 666).

The usefulness of philosophy in science. Winter defends the role that philosophy can and should play in scientific research, and its usefulness within the sciences. He argues that "criticism will always intervene to discern the important work that must constitute fundamental scientific theories" ([10], p. 495) but "the work of criticism we are talking about is not only of purely philosophical interest, it can be of great use to science" ([4], p. 496), as we have already mentioned. Philosophy is thus placed at the service of science, a position legitimized by the observation that "the spontaneous development of philosophy in scientific circles is the best proof of its vitality and usefulness" ([8], p. 911). In fact, most of these philosophical works were written by leading scientists whose knowledge of their subject matter cannot be attributed to them.

Many examples of the usefulness of philosophy in science are highlighted by Winter. Dubois-Raymond and Cantor are the finest examples of the fruitfulness of philosophical method combined with science ([8], p. 918). Both drew on the lessons of philosophical reflection on the mathematical principles that guided them. "If there are discoveries in science that are due to skillful calculations, others to ingenious experimental devices, there are, on the contrary, some that have their origin in the meditative effort of a philosophical mind" ([7], p. 326). A close study of Leibniz's and Newton's work on the genesis of the infinitesimal calculus shows that "Leibniz's inferiority as a calculator was more than offset, in the creation of a method, by the breadth of his philosophical thought" ([7], p. 327), to the extent that some of the most essential advances in positive science have been made by philosophical thought ([7], p. 327). Another example is provided by the development of algebra described in [10]: although Lagrange failed to

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establish a general method for solving equations, based on those of Cardano and the Italian school in the case of small degrees, he nonetheless introduced the fundamental concept of a resolvent function and developed the notion of symmetries and permutations. These new ideas were key to Galois's later studies, as he succeeded in grasping the fundamental notion of a group associated with an equation and characterizing its resolubility. The demonstration of these structures led to Abel's proof of the non-solvability of general equations of degree greater than 5, motivating a reform of the notion of the solution of an equation. This also gave rise to the means, so fruitful in analysis, of representing solutions as limits of successive approximations, through transcendental functions, integral forms, development in series, etc. It is also Klein's philosophical reflections that take the unification of geometries a step further ([10], p. 523). The enlightening and organizing role of Winterian philosophy is richly illustrated here, and judged through the eyes of the usefulness within the sciences.

Finally, scientists and philosophers play a complementary role in the establishment of science and the development of ideas, and "while the scientist is exclusively interested [in] the new and precise fact that has been added to the body of knowledge, the attention of the philosopher, more romantic by temperament, is drawn above all by the disturbance in our ideas" ([14], p. 268). "Applied to the positive sciences, philosophy, far from being, as the ignorant suppose, a distraction for idle minds, constitutes on the contrary the very principle of fermentation that excites minds too inclined to contract machine-like habits" ([8], p. 920). This posture "prevents scientific thought from crystallizing" ([14], p. 269), clearing the way for new ideas. But "neither can it be seen as a kind of universal speciousness, in which mathematical thought would be completely annihilated" ([5], p. 214). As well as being a unifying and organizing principle, philosophy of science also serves a useful function for scientists. Conversely, "there are mathematical problems, properly speaking, that raise far-reaching philosophical questions" ([5], p. 214).

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§ 3. — Winter against metaphysics.

The rejection of metaphysics. Despite the rationalization of French and German schools of thought since the 18th century, notably with the philosophy of the Enlightenment, Kantian criticism and the advent of modern logic, metaphysics remains at the heart of philosophical developments. Metaphysical arguments are used in ways that are often implicit, sometimes unconscious, but always underhand. In this way, philosophies are often partially tinged with metaphysics, the rigor of reasoning sometimes giving way to the intervention of a transcendent evidence that withdraws its legitimacy, in the sense of a scientific epistemology, from the reasoning as a whole. Such is the position of Dunan, who posits at the outset of his study that "science is the starting point of metaphysics" ([1], p. 167) and who derives from science a metaphysics that governs it, subjecting one to the other through a philosophical postulate rather than an argument respecting explicit methodologies or rules.

Many other scientists and philosophers adhere to such a metaphysic stance. Such is the case with Kant, whose transcendental logic Winter recognizes as valuable, but whose metaphysical aspect unfortunately takes him beyond science ([3] and [11]). Poincaré also rightly defends himself against drift and utopianism, without taking care to avoid the final metaphysical argument in favor of the intuition he defends ([9]): Winter emphasizes at the outset his intention to avoid this caveat by "distinguishing[ing] in problem[s] the methodological point of view and the metaphysical point of view" ([9], p. 921).

From Winter's very first article, the young philosopher tackles the transcendent aspect characteristic of metaphysical postulates. He illustrates this with the example of Charles Dunan's vitalist theory ([1], p. 169), and despite agreeing on the existence of a living unity that goes beyond the mere union of body parts, the only way to justify it, according to Winter, should be through thought and science, whereas Dunan, conversely, justifies thought through a vitalist metaphysical postulate. Metaphysics is presented by Winter in its aspect outside science, beyond its object, and therefore also alien to it. But "metaphysics is not a science, nor can it provide scientifically indisputable formulas" ([3], p. 595), and only science and deductive thinking are sufficiently precise to prevent

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reflections from being biased by an arbitrary and unfounded initial postulate, such as those of metaphysics. Thus, at the risk of borrowing from logicism, Winter above all rejects metaphysics as a mind game that cannot contribute to philosophy, let alone science. This categorical rejection of metaphysics is expressed many times by Winter, and is summed up in these words: "Let us therefore leave symbolism to the poets, but let us not expect to find in it a philosophical method" ([1], p. 179).

Winter remains constantly suspicious of the unfounded drift of arguments and assertions, the stealthy drift of philosophy towards the arbitrary postulates of metaphysics. The origin of this arbitrariness is the indeterminacy and indeterminability of metaphysical postulates, which are all as valid as any other, and it is therefore the infinity of possible unifying principles for metaphysics that condemn it by its inability to decide rationally, as seen in the sterile speculations on different infinities ([12]). Winter thus believes that "extreme wisdom for a mathematician would be not to consider such problems, at least in their absolutely general form" ([12], p. 611), but to judge them in the light of their usefulness in the real world of science and its applications, a criterion of truth already raised.

Metaphysics: a fuzzy posture. The second notable argument against metaphysics is the lack of precision in its arguments and reasoning, which is equally condemnable, since vague arguments forbid to prove anything clearly, especially not to discuss anything precisely. Thus Winter rejects Kant's general logic as absolutely disconnected from reality and de facto empty of any content, and his metaphysical discourse on transcendental logic can only discredit from the outset a thought that could otherwise be worth considering ([3]). Generally speaking, metaphysics falls into all the anti-positive pitfalls that emerging logic highlights and helps to avoid: fuzzy or tautological propositions, through its ill-defined concepts, and the burden of arbitrariness that guides the choice of postulates for reflection, making metaphysics the abyss *par excellence* of "lawyer proofs" that Kant himself seeks to avoid. By banishing both mathematics and experience from its reasoning, metaphysics deprives itself of demonstration, "but, without demonstration, there is no valid intellectual theory, and it is this inability to provide proofs that constitutes the vice of all metaphysical philosophy" ([3], p. 604). The father of positivism, Comte himself,

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sinks into the vagueness of generalities with the ambition for positivism to grasp the reigning order in the fundamental notions of science, considered as above science and resulting in an "arbitrary reversal of method, in the realm [of] generalities [that] have been completely useless to science" ([7], p. 325). Generally speaking, "the regressive method, the metaphysical analysis of elementary notions, is a return to the vague forms of the vulgar discourse of common thought, from which science originated [...] and it is therefore sterile work" [...] ([8], p. 914) to tolerate a metaphysics that represents nothing more than a regression of thought towards the vagueness that modern positive science seems to have definitively conquered.

This ambient vagueness, firmly anchored in philosophical practices, has both a reassuring and a deceptive effect, affecting even the most rigorous scientists. Thus, like many of the great rationalists and scientists such as Kant and Poincaré, the vagueness of metaphysics perniciously intrudes into discourses that nevertheless claim to be of a different rigor: Poincaré's defense of intuition over logic, blandly immune to the attacks on any lack of scientific knowledge, witnesses the difficulty of the struggle. Winter's choice is to rescue philosophy from this obscure and empty metaphysics, and in fact to liberate science from it, because "for [philosophical] reflection [on the sciences] to be effective, it must be oriented in the very direction of science; it must strive to advance positive science with its own means, and not lose its way in vague theories of knowledge, which are generally no more than theories of ignorance ([7], p. 345). This is the case, for example, of "intuition [which is] a certain metaphysical principle whose scientific definition cannot be given, but which can, to a certain extent, be determined in the following way: intuition is transcendent with respect to purely logical forms" ([9], p. 60). Throughout his critical commentaries and historical studies, Winter meticulously strives to detect the metaphysical parts of the arguments. For example, he condemns Émile Picard's surreptitiously metaphysical argument, which rejects without concession the idea of hereditary mechanics, even though, in order to dismiss hereditary explanations a priori, "they would have to contain some kind of crippling logical flaw, from which other methods would be exempt. However, this does not appear clearly" ([14], p. 275), and the conclusion is more the expression of a vague and illusory conviction than of a valid argument.

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Uselessness of metaphysics for the philosophy of science. Despite the presence of metaphysics in philosophical reflections on science, Winter does not overly elaborate much on a topic that, dealing with the metaphysical, can only remain sterile. The scientific reality rem ains indeed little affected by these questions, since they will never, as Borel argues with regard to the axiom of choice ([12], p. 615), influence scientific practice and the search for solutions: these are far more important issues than the metaphysical illusion that has haunted philosophers since antiquity. It is through historical and critical study that Winter tackles this question, and notes the uselessness of metaphysics in scientific work.

The history of wave mechanics, traced by Winter in his brief study of Louis de Broglie's *Introduction aux principes de la mécanique ondulatoire* ([20]), is a perfect example legitimizing the philosopher's positions. Indeed, it was by identifying the principles underlying the analogies observed that geometrical mechanics evolved into wave mechanics, as a result of the evolution — this time purely scientific — of the notion of light and the principles of geometrical optics. The analogy between the two theories of mechanics and optics paid off, as the evolution of optics naturally led to a revision of the fundamental positions of mechanics. The new theory has distinguished itself by its efficiency and its agreement with the results of other fields of physics, therefore finding a far more legitimate position than its predecessor, both in terms of its logical status and its usefulness. The failure of metaphysics to deal in the most abstract way with the problems of physics is further reinforced, for "there is [in this new theory] a state of things which numerous experiments and calculations have imposed, and against which general reasoning is powerless" ([20], p. 131), while metaphysical discourse have instead given birth to monstrosities so difficult to destroy, for instance ether, humours, phlogiston, etc. This is a rational and critical philosophy, based on the analysis of the evolution of ideas and the logical genesis of the principles of science and its various branches, exploiting the fruitfulness of the analogy between mechanics and optics — which is a method internal to science — that imposes itself against a vague and abstract metaphysics: analogy is thus legitimized, becoming a valid epistemological tool.

The previous example is already an indication that science now has the tools to think out, clarify and organize its own principles, with clarity and fruit, fulfilling the aim of the Winterian philosophy

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of science. Metaphysics will naturally continue to wither away little by little with the advance of science, for "the boundaries [of metaphysics and science] are not determined for eternity, those of the former narrow, those of the latter widen with the development of scientific thought" ([3], p. 591). Indeed, the metaphysical philosophers who claimed to be the only legitimate ones to study the principles of science have lost their monopoly to modern logicians and scientists, and "it must be acknowledged today that metaphysical doctrines, elaborated with transcendent ulterior motives, have done little to illuminate and determine the foundations of science" ([3], p.. 589), in contrast to the undeniable success of analysts and geometers, who "not only extended the proper domain of science, but [...] controlled and fixed its fundamental principles" ([3], p. 589). If, as Winter proposes, the metaphysics of science aims "1) at finding a philosophical foundation for the principles of science, and 2) at unifying human knowledge" ([3], p. 598), history seems to have removed all *raison d'être* for this metaphysics. Logical developments provide a solution to the first problem — never definitive, but sufficient for the criterion, the only legitimate one in science, of utility and experimental verification ([2], p. 616) — and the critical history of science provides a solution to the second. All light can thus be shed by science alone, by means of definitions clarifying vulgar notions, but this demonstration of principles is eminently scientific and avoids all metaphysics ([3], p. 600). The millennia-old and stagnant enterprise of the metaphysicians finally seems to have found its fulfillment ironically in the hands of Boole and Russell, logicians and great opponents of metaphysics, and if "numbers form, as it were, the very armature of intelligence" ([7], p. 345), the goals of metaphysics could be entirely fulfilled by science.

The anti-metaphysical conclusion is reached not through a new metaphysics, but through the simple historical-critical observation that metaphysics has remained unable to solve its problems, and that modern science and logic are fruitful in their own right. This is Winter's constant approach in his research: to legitimize approaches through the concrete utility and historical development of ideas, because " we think with the current forms of thought and the question of whether they have always existed or will always exist is meaningless: we cannot answer the problem of origin" ([2], p. 618). So, like the problem of infinities and the opposition between logicians and intuitionists, we cannot settle the question,

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we can only show its various sides. He who wishes to formulate absolute conclusions exposes himself to inconsistency in the present, and to being contradicted by facts in the future " ([12], p. 616). This is a philosophy based on historical epistemology, judging by the quality of scientific achievements, according to what a conceptual Darwinism would be.

Winter, holding the torch for an entire positivist generation of philosophers and scientists, seems victorious against metaphysics, both in philosophical reasoning and in scientific thought, not least because metaphysics is out of its object. However, this general anti-metaphysical fervor conceals a tendency towards the systematic logicization of the world, and now that logic's great adversary seems defeated, a logicist imperialism is taking shape.

§ 4. — Winter against logicism.

The metaphysical arbitrariness of logicism. The dazzling logicization of the sciences at the dawn of the 20th century is presented by its followers as the pinnacle of the rationalization of thought. This revival of logic, with its all-encompassing formalism, is akin to the realization of Leibniz's dream of *calculus ratiocinator*, which reduces all philosophical or mathematical discussion to algorithmic calculation. Logicians have therefore "not only extended the proper domain of science, they have controlled and fixed its fundamental principles. [...] Until then, no philosopher had thought of testing the value of Euclid's postulatum and the principles on which his geometry is based" ([3], p. 589) This new logic is fundamental in that it claims to be the foundation of the other branches of science, becoming the language through which the sciences express themselves, the form of their discourse. It is the precise study of the laws of this discourse that Winter calls *logisitics* ([7], p. 321).

However, despite this powerful and fruitful logical revolution, unlike metaphysics, which theses have little influence on positive science — the foundations of logic being, by extension, the foundations of scientific reasoning — biased logic can only lead to a deviation in science itself. In opposition to the positivist and logicist tendencies of his time, Winter does not fail to identify shortcomings he already criticizes in metaphysics. It is therefore appropriate

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to raise very precisely the arguments behind Winter's metaphysics accusations against logicism.

Winter's first criticism of logicism, analogous to his criticism of metaphysics, is its arbitrariness. Indeed, the logical systems that claim to govern thought exist in an infinite variety: almost any variation of axioms remain an admissible logic system. This can be seen in the multiplication of consistent logical systems, such as the different geometries, which are nonetheless exclusive. This multitude of possibilities requires a choice that can only be based on metaphysical arguments, practical observations or cultural habits. For over two millennia, Euclidean geometry thus arbitrarily closed the door to other geometries. These arbitrary choices are all the more serious in that "given the tacit assumption that logical-mathematical laws have an absolute value, we apply to them the idealist principle [that] the laws of the mind are the laws of things", and the arbitrary formalization effected by logic is therefore impoverishing ([2]) for science, and binds it to principles from which it will find it difficult to free itself. These axioms, atoms of logical systems, are to be discussed in a particularly enlightened way: "from the moment that the progress of mathematical methods consists in giving logical rigor and precision an ever-increasing share, we must necessarily be led to scrutinize the very foundations of the reasoning of geometers, to analyze the fundamental types of demonstrations, and to fix the indefinable elements on which these demonstrations rest." ([5], p. 189).

Even more than the arbitrary choice of the axiomatic system, this formalization is abusive, and assumes without justification the possibility of translating an obviously fuzzy thought into a precise and perfectly delimited calculation. Thus, the general craze masks the impact on physical and mathematical models of our choice for classical logic, which has been so reluctant to introduce approximations in analysis or statistics in physics: "isn't there an obvious contradiction in wanting to give an absolute logical foundation to what is only psychological and human?" ([5], p. 198), and isn't it condemning science to do so? This confirms Winter's anti-logicist stance. He does, however, recognize the merits of logic, and hardly condemns science to the status of mere speculation. If reasoning is to be governed by rules, these rules must be clarified, evolve and be perpetually questioned, in the light of their usefulness, which alone guarantees the value of the choices made.

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Winter does not fail to raise these limitations in several of his historical studies. In particular, he notes that "Hilbert's axioms are purely formal in character and are given as a series of arbitrary decrees [and] this is the flaw in this doctrine, whose logical importance nevertheless remains great. The author develops a hypothetical-deductive system of axioms for the science of the moment, and it is not obvious that another system of axioms cannot be substituted for this one to some extent" ([19], p. 230). Hilbert himself would fall into the same trap by not doubting the axioms of classical logic, which are just as arbitrary a priori, although he does mention the legitimacy of the principles of equality and syllogism in [2] (p. 607). The infinity of possible postulates that so often characterizes the terms in which a metaphysical problem is posed is found in the fields of knowledge that logicism claims to subject to its uniformity. This limitation is particularly evident in the interpretation of legal texts, for which logic will never be able to determine a criterion for choosing among [the] postulates" ([4], p. 618). In the same vein, the great debates between logicians and philosophers are also all metaphysical in nature, unduly monopolizing attention and effort. So the relationship between logic and the reality of thought, such as the debate between purely logical reasoning and the existence of an intuition irreducible to logic ([5]), does not appear to be proven, and is at the very least not reducible to one another. Thus, he concludes that logistics, as a universal explanation, must be condemned in the same way as all metaphysics, because exercised outside its field of application, it remains a pure mind game with no scientific utility" ([5], p. 215). This rejection of logic's claim to universality heralds the failures of a future *Tractacus* to logicize the world.

The utility criterion for judging logicism. The logicist tendency to believe that, "by following the laws of logic, all truths can be found" ([9], p. 924) is refuted by Winter, since no logician has ever succeeded in reconstructing any part of science autonomously. The intuitionist, hardly in a better position, is confined to advancing the truism that "the inventor is not a calculating machine" and defending a certain metaphysical transcendence of intuition. The debate between these two positions, which appears to be insoluble, has no substance, and the two positions "are in fact opposing metaphysical theses whose very nature is that they can never be demonstrated scientifically and peremptorily" ([9], p. 925). The only reasonable,

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and never definitive, positions we can hope to advance on this issue are those based on *usefulness* alone.

This criterion of usefulness is enough to disqualify logicism, because the advent of logic and its systematic application has not solved the majority of problems facing the sciences. Indeed, if "formal logic cannot account for" ([5], p.. 207) certain mathematical phenomena, such as the sudden appearance of certain properties like the non-solubility of polynomial equations from degree 5 upwards, this is proof that logic cannot make up for the completeness of science, because "in the face of real mathematical difficulties [...] formal logic [is] of no use" ([5], p. 209). Or, on the other hand, it cannot independently resolve problems arising from science: The question of how to represent an integral raises a series of mathematical problems. For every function, there is an infinite number of expressions that can be used to represent it. The expression we choose should highlight the function's characteristic properties as accurately as possible, and in particular its singularities. However, among this infinite number of expressions to choose from, logistics offers no positive criterion for deciding which is the best. As we have already seen in number theory, the mathematician is faced with real mathematical difficulties, against which formal logic is of no use." ([5], p. 209)

At the end of the day, "to try, as has sometimes been attempted, to establish that [logic] is, absolutely speaking, superior [to the rest of science], seems to us a meaningless undertaking, because we are in the presence of two equally fundamental ways of seeing" ([13], p. 467), each of which complements the other. The asserted criterion of utility ([5], p. 202) also works against logic *per se*, because logic has never succeeded in constructing anything on its own ([9]), and the great scientific revolutions have never resulted from logic alone ([10]). Such revolutions are first and foremost the work of philosophers, not logicians. Logic must therefore remain — as Russell concedes, even though he himself was one of the instigators of the logicist ideal — a branch of mathematics, ignored in fact by most mathematicians, and not the indispensable instrument of all mathematical research ([5], p. 189).

Logic as a control tool. There's no denying the fruitfulness of logic's ideas and what this degree of formalization and abstraction has brought ([13], p. 473 ff., but also [5], [10] and [17]). This canon imposed on reasoning is, when used in its role as keeper of rigor

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and clarity, the mean of guarding against inaccuracies and general a priori that have no foundation. Indeed, logic, "the axiomatic analysis referred to in this note, is an extension of Descartes' method [...] It essentially consists in substituting intuitive and experimental notions, often confused, with clear and distinct ideas" ([16], p. 28), reflecting both the Cartesian ideal of clarity as one of the primary criteria of scientificity, and the Winterian philosophical objective of clarifying and ordering principles. The value of asserting the positive virtues of logic goes beyond mathematics, and Winter shows its applicability on a simple but generic example of legal controversy, where the ambiguity of laws confirms that "only the precise use of algebraic formulas can avoid similar confusions" ([4], p. 618) between logical principles. The impossibility of applying the methods of the exact sciences to law is the simple consequence of the absence of rigorous reasoning and a precise critical method. Thus, "confusion of opposites and contradictions, and ignorance of Morgan's laws" ([4], p. 623), led the highest legal authorities and the most experienced legal doctors end up deciding in favor of the least consistent solutions, as Winter's logical study of the legal controversy in [4] shows. The evolution of analysis has thus "[substituted for vague intuition] exact analytic definitions. We can therefore say, in keeping with historical truth, that the analysis of the 18th century was much less rigorous than that of the 19th century, and unless we maintain that the work of the 19th century marks a step backwards on that of the 18th century, we must recognize that the overall evolution of mathematical science indicates progress in the direction of greater logical precision" ([5], p. 188-189). The advent of modern logic therefore makes it possible to have a perfect *organon* governing proofs.

This new status for logic, central to modern science, raises questions of its own, and Winter recognizes that logicists have a necessary clarifying role to play here. "To teach the first elements of calculus, it was necessary to use sentences; indeed, it would be impossible to explain the beginnings of mathematics without a prior grammatical discourse. [...] It is these [grammatical] questions which must be answered, and which must be treated with the same precision as mathematics itself, if the starting point of arithmetic, analysis and the other branches of mathematics is not to be drowned in a fog. [...] As we cannot imagine what the mind of a man would be like, from which all grammatical-logical notions would be banished, the very principle of all positive philosophy

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obliges us to study these notions as given facts, to establish the laws of combinations that are peculiar to them, as well as the relationships that unite them to mathematical science proper. This is the positive field of application of Logistics, a field which constitutes the logical introduction to the theory of numbers and the theory of functions." ([5], p. 195-196). In other words, "the analysis of mathematical reasoning, i.e. the determination of the grammatical-logical types it contains, is the proper domain of logistics" (*ibid.*, p. 197). This is a central task for the mathematician and philosopher alike, but one that will never be completed: "the system of our logical ideas is a closed system, and [...] the universe of abstract beings is an open system. But, by this very fact, the study of the universe of beings will never be completed, and it will force us to modify the closed system of logical constants." ([5], p. 210). Logic, as a study of the grammatical discourse of science, becomes central to scientific reasoning, and is thus a subject of study in its own right.

Logic naturally forms the last stage of the "process of thought essential to the exposition of mathematics" ([7], p. 322). This final stage is extensively studied by Winter, in the case of Frege's logical foundations of number theory or Hilbert's axiomatization of physics. Logic thus enables a certain unification of mathematics ([5]), in its language component, and it is an abstraction which, by distancing itself from its sensible object, frees us from vulgar a priori and avoids the illusions of the real and the familiar, of examples and intuitions ([16]). Logic thus finds a place within science, as a particular branch with circumscribed aims rather than as a specious one that would be above it.

The importance of the link to reality. To adapt to the problems discovered by time and practice, logic needs to be enriched with new definitions. These new objects of thought are arbitrary, but legitimized by their usefulness and natural necessity ([5] and [15]). Indeed, if science were to seek only generality and abstraction for its own sake, it would lose its ties with the particular things it sets out to explain, a situation that is obviously unbearable, for by developing purely abstract theories for their own sake, "we would run the risk of straying into the realm of formal logic and scholasticism" ([13], p. 509), in effect abandoning the primary objective of science: understanding the real world. Thus Cantor's thoughts on the powers of infinities seem, in Winter's eyes, to belong more to the realm of contentlessness than to the heart of science, and "if

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transfinites cannot be rejected by virtue of an argument from formal logic [...] only the use that can be made of them in particular problems will decide their scientific value" ([12], p. 615). Similarly, "the logistical definition of the irrational and the considerations that logisticians have made about it, whether or not they have been put into formulas, shed no light on the determination of the distinctive characteristics of commensurable numbers and incommensurables, of algebraic incommensurables of different degrees, and finally of transcendental numbers. The results obtained in these areas were achieved by directly tackling the difficulties using classical mathematical methods." ([5], p. 202). Another example is provided by his study of the meaning of the deterministic principle in physics, "which takes place by examining how this principle comes into play in the solution of problems" ([13], p. 74), thus becoming deeply rooted in the very practice of science.

According to Poincaré, as for many other mathematicians and physicists, experiments thus become "not a control procedure, but a method of demonstration" ([3], p. 615, in stark contrast to the positions of both the *Revue* and the logicists) in its own right. The logicist tendency reduces the philosophical or mathematical problem to a single aspect, whereas "philosophy must also determine the constitutive and specific differences in the principles of the methods" ([2], p. 610), which alone will make it possible to embrace the entire problem. Attachments to reality therefore seem paramount, and "we thus recognize that scientific judgment needs a certain particular content; but in trying to establish that this content is external to its method, we preserve that same method's universal scope" ([1], p. 172). Science is founded on coherence between its parts and, like the mason who must build his edifice from bottom to top," the logic of science can only pride itself on being a demonstrative and objective doctrine the day it becomes an integral part of science" ([3], p. 603), and if it can and must distance itself from its object and abstract itself in order to retain a sufficiently broad scope to serve as a general scientific method, it cannot cut itself off from it entirely, for it is its *raison d'être*. Experience, and in particular its basis in calculation and experimental logic, thus appears as a central method that should not be rejected without foundation.

If logic becomes the organon of scientific discourse, its critical and reflexive study remains subject to confrontation with the objects and problems internal to science. So, "we see the possibility

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of an axiomatic analysis of physics. We say analysis, not deduction of principles. It would be an analysis *a posteriori* of scientific notions from a given science in fact, and not a construction *a priori* of science. In the case of physics, the compatibility of axioms results above all from experimental verification." ([13], p. 101). "The system of physics axioms must be conceived as an experimental-deductive system. The expression "hypothetico-deductive system" coined by logicians does not seem to us to express clearly enough that the sole purpose of postulates and hypotheses is to introduce experimental data into calculations. From the point of view of formal logic, we can forget for a moment, in the manner of Hilbert, the intimate links that unite fundamental principles with experience; we can't do this from either a physical or a philosophical point of view and, in the end, we have to conceive of the axiomatics of physics that we have just sketched as the logical analysis of experience." ([13], p. 102). All this philosophy looking through experience is summed up by Winter in these words: "the theory was born on a land of experiences, and must remain there to develop further" ([18], p. 323).

Jacques Hadamard is presented by Winter as another perfect example of a theoretical and rigorous mathematician who does not neglect this essential aspect that gives meaning to the mathematical theories and ideas he expounds in his courses at the École Polytechnique, "in a constant concern to link analysis to its true source: Physics" ([21], p. 608). Only this marriage between the abstraction of methods and the reality of the object can guarantee the fruitfulness of science, a lesson from history defended by Winter and well-illustrated by Hadamard. As Winter points out, while mathematical analysis has contributed much to the development and formalization of physical models, physics has been no less a driving force behind the development of analysis, and to forget this perpetual mutual enrichment would be to deny the very origin of ideas. It is these numerous applications [that] bear witness to the fruitfulness of new ideas; they are, in a way, the guarantee of their objectivity, and "criticism [must keep] the content in its experimental form to prevent metaphysical principles, mere conditions of thought, from being transformed into dialectical ideas" ([2], p. 619).

Science is thus always nested in its object, at the confluence of many other fields ([25]), and to forget this would be to deny a part of science, perhaps the only part that links it to reality and differentiates

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it from purely speculative philosophy, from metaphysics. So analysis, as Winter develops in his historical-critical study of the evolution of function theory, must not forget its object — physics — and it is by remaining linked to its object that it will retain its fruitfulness and continue to evolve ([5] but also [17].). Winter thus circumscribes "logistics [which] must have boundaries in the sense of metaphysical abstraction (an upper limit, if you like), which means that it must, through an explicitly formulated postulate, cut short any digression into the sterile realm of scholasticism. It must also have boundaries in the sense of its applications (lower limits) [...] to avoid any duplication with already existing methods." ([5], p. 190).

Although logic, in its positive and non-metaphysical role, appears to be the best canon for presenting science and a rich guardian of the rigor of reasoning, it is not enough to solve scientific problems, and its use must be that of a conscientious, powerful but epistemologically limited tool. In contrast to the logicist tendency of his time, Winter emphasizes the philosopher's role in guaranteeing the balance between logical freedom and the reality of science, synthesizing the forms of laws and the reality of objects.

§ 5. — Conclusion: A historical-critical epistemology rooted in the sciences.

An original mathematical philosophy. Neither philosopher nor mathematician, Maximilien Winter eschewed the major trends of his time in both metaphysics and logicism, in favor of a rational, critical philosophy rooted in the sciences and their history. In this respect, he set himself apart from his comrades from the *Revue*, whose aim was to rehabilitate metaphysics and morale, and from the vast logicist school developing in Europe. Discreetly and unpretentiously, he opts for a relatively new philosophical ideal, denying philosophy its primacy over thought and putting it back on a par with mathematics, science and facts, without reducing it to them — avoiding a positivist stance — so that each can shed light on the other. Philosophy of science thus becomes a scientific philosophy, where problems are no longer purely abstract, but where thinking revolves around science. This approach makes it possible to identify fundamental and fruitful principles, never

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definitive, but whose value is guaranteed both internally, through their usefulness within science and their relevance to its objects; and externally, through the historical-critical confirmation of their a posteriori fruitfulness. The coexistence of science and philosophy is thus made possible by a dialectical organization between the two, a position giving superiority neither of philosophy on science (as would be the case with metaphysics), nor of science on philosophy (as would be the case with logicism or positivism), but a mutually profitable exchange — science becoming aware of its methods and principles, philosophy being enriched by scientific technique and issues.

A timeless choice. With his pious wish to limit pretensions and balance the roles of both philosophy and science, Winter remained philosophically isolated throughout his life, his studies and ideas finding few echoes and quickly forgotten after his death. His reflections were swept up in a torrent of enthusiasm about the foundations and principles of science, founding the logicist wave that would sustain both the Vienna Circle in the philosophical world and the Bourbaki group in mathematical spheres. The man who advocated impartial judgment of science in the hope that it would continue to progress, in the light of rational philosophy and critical history, found himself overshadowed by the positions he opposed. More than half a century later, efforts and hopes to base science and the world on a definitive, closed system of logic collapsed. Gödel's results intersected with Winter's departure, and although he found strong scientific support for his rejection of logicism, logic continued its quest to standardize mathematics, and the philosophy of science essentially continued to detach itself from science, borrowing only its most arbitrary part: its new logical formalism.

The judgment of history. A century of evolution has seen the gradual collapse of logicist enterprises. If logic has provided a powerful and fruitful tool for science and philosophy, as Winter defended, the desire to condense facts and sciences has impoverished theories and ideas, standardizing them in an artificial and arbitrary way, which the philosopher already decried as the usual metaphysical reversal of reasoning: the image we have of the world has become a postulate from which we seek to explain a world that is no longer the same.

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The incredible growth of applied mathematics has brought as much to the other sciences as these applications have brought to mathematics, with Kantorovich's economics fruitfully resurrecting Monge's twice-century-old transport problem; Yves Meyer's image processing bringing a revolution in the theory of function representations from Fourier series developments thanks to wavelets ; biological and financial modelling, creating the conditions for the development of random processes, which had not seen major advances since the amusements of the Chevalier de Méré; computer security, giving that simple game of the mind that was arithmetic a new lease of life with ubiquitous applications; and no one will forget the contributions of the development of aerodynamics and design to geometry. Philosophy also contributes to this enrichment of mathematics, Carlo Rovelli's philosophical conceptions of time and space having been the very essence of Alain Connes' developments in non-commutative geometry. Never complete, the task of science thus always advances in the right direction, "slowly but surely moving towards an ever more perfect order" ([3], p. 606), epitomizing Winter's optimism. The evolution of metaphysical systems gives way to the progress of science, which is based on, and illuminated by, logic and critical philosophy.

Maximilien Winter, that illustrious unknown who believed in a living mathematics and philosophy and who defended as the only legitimate judgment that based on critical history, that jurist ignored by those he hoped to enlighten and who preferred to embrace the logicism and metaphysics he rejected, will have departed in the shadow in which he lived. Today, his philosophy has shown itself capable of guiding and enriching the sciences, his own historical-critical method having been taken up by some of the greatest mathematicians trained within the Bourbachian opposition.

Another jurist ahead of his time had once illuminated mathematics and paved the way for future generations, and it may be a century later that we realize what Maximilien Winter contributed to the French mathematical epistemology.

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Mathematics an Imagined Tool for Rational Cognition Part I

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*"...numbers are free creations of the human mind;
they serve as a means of apprehending more easily and more sharply the difference of things."*

Richard Dedekind [Dedekind, 1888, p. vii]

Abstract. By analysing several characteristic mathematical models: natural and real numbers, Euclidean geometry, group theory, and set theory, I argue that a mathematical model in its final form is a junction of a set of axioms and an internal partial interpretation of the corresponding language. It follows from the analysis that (i) mathematical objects do not exist in the external world: they are imagined objects, some of which, at least approximately, exist in our internal world of activities or we can realize or represent them there; (ii) mathematical truths are not truths about the external world but specifications (formulations) of mathematical conceptions; (iii) mathematics is first and foremost our imagined tool by which, with certain assumptions about its applicability, we explore nature and synthesize our rational cognition of it.

Keywords. Mathematical intuition; mathematical models; mathematical objects; mathematical truths; applicability of mathematics

The basic problem of the philosophy of mathematics (not mathematics itself) is to answer the following intertwined questions:

- Whether mathematical objects exist, and if so, in what way do they exist?
- What is the mathematical truth and how do we establish it?
- How is mathematics applied?

This article and its sequel present a solution that can be considered an elaboration of Dedekind's quotation cited at the beginning of the article. The basic theses that I intend to argue are the following:

- Mathematical objects do not exist in the external world. They are imagined objects, some of which, at least approximately, exist in our internal world of activities or we can realize or represent them there.
- Mathematical truths are not truths about the external world but specifications (formulations) of mathematical conceptions.
- Mathematics is first and foremost our imagined tool by which, with certain assumptions about its applicability, we explore nature and synthesize our rational cognition of it.

I will try to make clear what is absolutely clear to the famous physicist Percy W. Bridgman: "It is the merest truism, evident at once to unsophisticated observation, that mathematics is a human invention." [Bridgman, 1927, p. 60]. Having practised mathematics all my life, by vocation and by profession, just as breathing was natural to me, so it was natural for me to consider mathematics as a human invention and a free creation of the human mind whose purpose is to be a tool for our rational cognition and rational activities in general.[(1)] When I decided to clarify to myself what human invention mathematics was, it proved to me, I believe, a far more difficult task than explaining what breathing is. In this article and its sequel, I have outlined what I came up with along the way.

In this article I will consider as illustrative examples the classical mathematical models which are still the most important ones: the natural number system, the real number system, and Euclidean

[(1)] Of course, many mathematicians do not share my opinion that mathematics is a free creation of the human mind.

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geometry; as well as contemporary standard mathematical models: group theory, and set theory (sections 1 to 6). To quote the famous mathematician Saunders Mac Lane: "...a philosophy of Mathematics is not convincing unless it is founded on an examination of Mathematics itself." [Mac Lane, 1986, p. 60]. In the last section (Section 7) I will set out the basic characteristics of the view of mathematics as an imagined tool for rational cognition. In the next article, I will elaborate in more detail how mathematical objects possibly exist and how mathematics is applied. I will also describe how the whole structure of mathematics can be understood as an imagined tool for rational cognition. Also, there I will expose this view of mathematics to various tests as well as determine its place on the map of different views of mathematics. In [Čulina, 2022a] there is a complete exposition in the form of a preprint.

The terms "intuition", "idea", "conception", "model" and "theory" will denote more and more precise stages in modelling certain thoughts. The terms "model" and "theory" will eventually acquire a precise meaning that deviates from the standard meaning in mathematical logic. They will be synonymous here and will denote a set of axioms in a language, together with a partial interpretation of the language. In doing so, I will give preference to the term "model" when the emphasis is on the interpretation and to the term "theory" when the emphasis is on axioms. Thereby, the term "interpretation" generally refers to the meanings of linguistic forms, and more specifically to the interpretations of first-order languages. The term "specification" generally refers to the refinement of ideas, and more specifically to the description using a set of sentences (axioms) of a first-order language that an interpretation of the language must satisfy. By "internal world of activities" I mean the world that consists of activities over which we have a strong control, and which we organize and design by our human measure. For example, these include movements in a safe space, grouping, arranging and connecting small objects, spatial constructions and deconstructions with small objects, talking, writing and drawing on paper, shaping and transforming manipulative material, combining and repeating actions, making choices, dynamics of actions and changes in the environment subordinated to us, painting, singing, etc. This does not include activities with objects over which we have no strong control, or our activities are significantly limited by the environment, such as e.g. climbing steep rocks, building a house, etc. Although each

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person has her own specific way of organizing and designing internal activities, which depends on her as well as on the environment to which she belongs, due to the generality of further considerations, it does not matter whether we mean the internal world of activities of an individual person or the internal world of activities of the entire human species. The internal world of activities will be discussed in more detail in Section 7. I would also note here that all imagined objects are considered as concrete and not abstract objects. In Section 7 it will be argued that language is the bearer of abstraction and not objects.

§ 1. — **Natural numbers.**

Natural numbers are the result of modelling our intuition about the size of a collection of objects. We measure the collection through the process of counting, and natural numbers are objects for counting. To start counting we must have the first number, to associate it with the first chosen object in the collection. To continue counting, after each number we must have the next new number in order to associate it with the next chosen object in the collection. There is no special reason to sort out certain particular objects as natural numbers. Merely for the needs of performing a calculation we sort out a particular realization. In the past those were collections of marbles on an abacus, and today we use sequences of decimal digits on paper and of bits in a computer. It means that for counting it is not important how numbers are realized, but only the structure of the set of natural numbers which enables us to count is important: that there is a first number and that each number has its successor. It seems that natural numbers exist in the same way as chess figures, in the sense that we can always realize them in some way. However, the structure of natural numbers, as opposed to the structure of chess, brings in itself an idealization. So that the counting could always continue, each natural number must be followed by the next natural number. Therefore, there are infinitely many natural numbers. Thus, although we can say for small natural numbers that they exist in the standard sense of that word, the existence of big natural numbers is at best a kind of idealized potential existence.

In order to precisely formulate the conception of natural numbers, we need a corresponding sufficiently precise language, a

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mathematical language, or even better, a first-order language. Among other symbols, the language should contain the symbol for the first number, the standard is "1", and the function symbol, for example an "S", for the immediate successor operation ($n \mapsto \mathrm{S}(n)$). This allows us to name each number. What the names denote is not so important. It is only essential that the named objects have the structural role of natural numbers (the names can even denote themselves). We specify the conception by declaring certain sentences of the language to be true about natural numbers. It is not necessary to precisely specify these claims here, nor the language in which they were made.[2] In that language, the claims must express, inter alia, (i) that 1 is the first number, (ii) that each number n has its successor $\mathrm{S}(n)$ which is a new natural number in relation to all previous natural numbers, and (iii) that any natural number can thus be obtained. I will hereinafter call these claims the axioms of natural numbers. In my view, the axioms of natural numbers are neither true nor false, just as the axioms that would describe the game of chess would be neither true nor false. They are simply a means of specifying our ideas about the objects we use for counting.[3] However, Gödel's incompleteness theorems [Gödel, 1931] and Lowenheim-Skolem theorems [Löwenheim, 1915; Skolem, 1920] tell us that we cannot have a complete (at least up to isomorphism) specification in a first-order language, even if we include sets in the specification.[4] Therefore, in addition to the axiomatic specification, it is necessary to have an interpretation of the language, as well. In the case of natural numbers, it is a partial internal interpretation — a partial interpretation in our internal world of activities. The interpretation is partial due to the idealization of the existence of extremely big numbers. The interpretation belongs to our internal world of activities

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[2] The standard formulation consists of Peano's axioms and recursive conditions for addition and multiplication in the first-order language of arithmetic [van Dalen, 2013, p. 82].

[3] In [Ferreirós, 2016, Chapter 7] Ferreirós argues that Peano's axioms are self-evident truths about our practice of counting. Our practice of counting is not a natural process but a voluntary activity. That is why, in my opinion, the truth of Peano's axioms does not derive from that practice, but from the fact that they norm that practice, especially since they introduce an element of idealization into the practice.

[4] Although in the classical set theory all structures of natural numbers are mutually isomorphic, according to the Lowenheim-Skolem theorems, the classical set theory itself has non-isomorphic interpretations [van Dalen, 2013, p. 105-106].

because numbers are our imagined constructions that we can partially, to the point of isomorphism, realize in the world available to us: using marbles on an abacus, tallies on paper, etc. On the other hand, even in the idealization of the total interpretation of the language, it is necessary to additionally have an axiomatic specification, because the recursively defined truth value of sentences in the interpretation is not a computable function [Gödel, 1931; Tarski, 1933]. Since we can never construct all numbers, the overall structure of natural numbers does not exist in the literal sense of the word. There is only the conception of natural numbers, specified by axioms, and partially realized in our internal world of activities. This is the final result — mathematical model — of modelling our intuition about natural numbers as objects for counting. It carries with it incompleteness and the ever-present tension in mathematics between basic intuition and the constructed model, as well as between the axiomatic specification and the constructive content of mathematical concepts. This tension is a positive source of accepting new axioms as well as improving intuition.[5]

§ 2. — Real numbers.

Through real numbers we organize and make precise our intuition about the process of measuring.[6] While natural numbers are imagined as objects for counting, real numbers are imagined as the results of the process of measuring. However, we can imagine (idealized) situations in which the process of measuring never stops — we generate a potentially infinite list of digits, with no consecutive repetition of the same group of digits after an individual step. If we want to have the results of such processes of measuring, we must introduce, in addition to rational numbers, new results of measuring — irrational numbers. As opposed to natural numbers whose existence

[5] This tension took a more dramatic form in the historical conflict between modern and classical mathematics, primarily in the confrontation between Dedekind's modern conceptualist approach and Kronecker's classical constructivist approach, as well as in the crisis in the foundations of mathematics — in the conflict between Hilbert's program in the foundations of mathematics and Brouwer's intuitionism (see, for example, [Ferreirós, 2008]). How much of this tension is present in the Hilbert's program itself can be read in [Sieg, 1999].

[6] In my opinion, this is the main role of real numbers, as a tool for rational cognition. Various practices with real numbers and their central role in mathematics are nicely described in [Ferreirós, 2016, Chapter 8].

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we can understand at least as some kind of an idealized potential existence, we cannot explain the existence of irrational numbers in this way. Although we can approximate irrational numbers by rational numbers with arbitrary precision, their existence is outside our means of construction — we have just imagined irrational numbers.

As is the case with natural numbers, the final mathematical model of real numbers is a junction of axiomatic specification[7] and partial internal interpretation of the corresponding language. For example, in the mathematical model we can identify Euler number e, the irrational number to whom the sequence $\left(1+\frac{1}{n}\right)^n$ is closer, as we increase the natural number n. Although we can approximate the number e with arbitrary precision by constructions in our internal world of activities, it certainly does not exist in the same way as my dog exists. It exists in the same way as an idealized material point in classical mechanics, as a non-existing phlogiston in a wrong theory about chemical reactions, and as Snow White in the classical fairy tale *Snow White and the Seven Dwarfs*. However, although our language usually has only a partial interpretation, the classical logic of using the language assumes that it is a semantically complete language — that it has a complete interpretation: each name names an object, each predicate symbol refers to a binary predicate, and each function symbol refers to a function.[8] Because of this assumption, in thinking itself there is no difference whether we think of objects that really exist or we think of objects that do not really exist. That difference can be registered only in a "meeting" with reality. And for mathematics there is no such meeting: a mathematical model creates its own reality in our internal world of imagination. However, unlike erroneous physical models, the mathematical model of real numbers can be realized approximately in our internal world of activities in the same way that correct physical models are realized approximately in the external world or children's fairy tales in real theatrical performances.

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[7] The axiom of completeness ensures that any idealized measurement process has a result. A variant of the axiom corresponding to this approach postulates that every decimal expansion $a_0, a_0.a_1, a_0.a_1a_2, \ldots$ (where $a_1, a_2, \ldots$ are decimal digits, whereas a_0 is an integer) has a limit and that every real number is a limit of such an expansion. All other variants of the completeness axiom postulate in other ways these imagined objects. See, for example, [Deveau and Teismann, 2014] — the variant with decimal expansions has the label CA18.

[8] For complete interpretations of the first-order languages see, for example, [van Dalen, 2013, Section 3.4]

§ 3. — Euclidean geometry.

With the appearance of non-Euclidean geometries in the 19th century, Euclidean geometry lost the status of an a priori mathematical theory. It became only one of the possible models for physical space, distinguished only by the fact that it is a good approximation of the space in which practical science takes place.[(9)] Contrary to such a view, according to which Euclidean geometry is a part of physics, I will argue here that just as number systems are idealized conceptions derived from intuition about our internal activities of counting and measuring, so too is Euclidean geometry an idealized conception derived from intuition about our internal spatial activities. Since the view of Euclidean geometry presented here is not standard, the argumentation will be more detailed than in the sections above.

Internal spatial activities should be distinguished from external spatial activities. The former are conditioned by our human nature, the latter additionally by the world around us. At first glance, it seems difficult, almost impossible, to draw a clear line between these two types of activities. However, for some activities we can clearly determine that they belong to external spatial activities. For example, mountain climbing is an external spatial activity because it involves orientation in a given landscape and taking care of the configuration of the terrain over which we move. When we are on the different parts of a mountain road, different spatial situations will require different responses. However, as far as our ability to react is concerned, it is the same in all places and in all directions. Of course, there is also the ubiquitous gravitational force that makes one direction in space prominent, which we especially have to take care of. Likewise, if there is a strong wind blowing from a certain direction, that direction also becomes prominent to us. However, we always attribute the appearance of differences in a certain direction to the influence of an external factor, which shows that a priori all directions are the same to us. If we have to light a fire by placing twigs so that they form a cone, our approach to geometric construction will be the same, whether we are making a small or a large cone. This shows that our ideas of spatial constructions are independent of the units we use in their construction. If for a certain selection of units a change in construction occurs, we ascribe

[(9)] In [Torretti, 1978], the impact of the appearance of non-Euclidean geometries on the philosophy of geometry is analyzed in detail.

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it to an external factor. If in this way we try to identify the nature of our internal spatial activities, as invariants to the different spatial situations in which we find ourselves, then we are very close to Delboeuf's analysis [Delboeuf, 1860]. He considers what remains when we ignore all differences of things caused by their movements and mutual interactions. According to Delboeuf, in the ultimate abstraction from all diversities of real things we gain the homogeneous (all places are the same), isotropic (all directions are the same), and scale invariant (geometric constructions are independent of size) space — the true geometric space which is different from the real space. However, for Delboeuf this geometry is the background geometry of real space, while for me it is the geometry of our internal activities in space.

We can come to the same conclusion if instead of an external argument, seeking common ground in all our external spatial activities, we use an internal argument, analysing our internal spatial activities directly and independently of the external world. A simple introspection shows that we do not distinguish different places, different directions and different units for spatial constructions until the outside world forces us to distinguish them. To eliminate the presence of gravity on the Earth's surface we must look for examples where it is negligible. In addition to the extravagant situation of a free fall, these can be examples of activities that take place approximately in the horizontal plane or three-dimensional examples in which gravity is not important. For example, a child will build his imaginary monster using Lego bricks regardless of the location where he built it, the way he oriented it in space and the dimensions of basic Lego bricks used in its construction. The same indifference to location, direction, and size is present when we rearrange the Rubik's cube.

I believe that our most basic approach to space, the approach inherent to us, is an *a priori ignorant approach to space*: all places are the same to us (the homogeneity of space), all directions are the same to us (the isotropy of space) and all units of length we use for constructions in space are the same to us (the scale invariance of space). These three principles of symmetry express our basic intuition about our internal spatial activities. Any deviation from these symmetries we attribute to the external world. Thus,

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it is precisely these principles of symmetry that determine a clear boundary between our internal and external spatial activities.[10]

Just as internal and external spatial activities can be distinguished, so can the geometry resulting from these activities. In my view of mathematics as our, in a certain sense, a priori tool of rational cognition,[11] geometry arising from internal activities is a part of mathematics, while geometry arising from external spatial activities is a part of physics. For example, caring about the direction of gravity belongs to external spatial activities, so pointing out that direction belongs to physical geometry and not to geometry arising from our internal spatial activities. Of course, in this physical geometry, as in other physical theories, appropriate mathematics is incorporated, but it is a different tool of rational cognition (see Section 5) than the geometry that arises from our internal spatial activities. In the book [Schemmel, 2016], it is nicely described how appropriate physical geometries emerge from our external spatial activities, which are very important for our survival.

In [Čulina, 2018], an elementary system of the axioms of Euclidean geometry is developed. On the one hand, the system is directly founded on the three principles of symmetry described above, while on the other hand, through the process of algebraic simplification, it gives an equivalent Weyl's system of axioms of Euclidean geometry (the axioms of Euclidean affine space) [Weyl, 1918, Chapter 1]. In this way, Euclidean geometry is characterized by these three principles of symmetry without any additional assumptions (except the idea of continuity). Thus, I gave the argument that Euclidean geometry is an idealized mathematical model derived from intuition about our internal spatial activities.

I consider this interpretation in space of our human internal activities the primary interpretation of Euclidean geometry. However, we can preserve the sentence part of the theory but

[10] The importance of these principles has been recognized a long time ago in the works of Delboeuf [Delboeuf, 1860], Helmholtz [Helmholtz, 1868], Clifford [Clifford, 1873, 1885] and Poincaré [Poincaré, 1902], but in a different interpretation than the one described here. In the 17th century John Wallis proved, assuming other Euclid's postulates, that the scale invariance principle saying that "For every figure there exists a similar figure of arbitrary magnitude." is equivalent to the Euclid's fifth postulate [Wallis, 1695-1699]. Also, it is well known that among all Riemannian manifolds Euclidean geometry is characterized by these three symmetry principles (see, for example, [Clifford, 1873]).

[11] I will explain my views below in Section 7.

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change the interpretation. Then it does not need to be a mathematical conception anymore. It depends on a new interpretation, be it an external or an internal one. If we ask ourselves whether the physical space obeys the axioms of Euclidean geometry, we must extract from space what we consider as points (maybe enough localized parts of space), as directions (maybe directions of light rays), and the distance between two points (maybe the time needed for light to pass from one point to another). If in such an interpretation the physical space satisfies the axioms of Euclidean geometry then we have an experimentally verifiable theory. Its sentence part is the same as in our mathematical theory of the space of our human activities, so we can transfer all results to the structure of physical space. Only the interpreted part is different. It does not belong to mathematics anymore, but it is a base for an experimental verification of the theory about the external world. However, we can change an interpreted part of the originally imagined Euclidean geometry in a way that it will be still a mathematical theory. And it happens in mathematics often. Namely, when we investigate complex mathematical objects which we cannot perceive so easily, for example a set of functions of some kind, it is useful to find Euclidean structure in it. Then we can transfer our geometric intuition to that set — think of functions as points, measure how distant two functions are, etc. In that way we can visualize them and succeed in thinking about them more easily and effectively.

The example of Euclidean geometry purports that only the axiomatic part of a theory can belong to mathematics, while the interpretation does not have to. Also, mathematical interpretation does not have to be an idealized direct interpretation in our internal world of activities, but it can also be an interpretation in another mathematical model (theory).

§ 4. — **Group theory.**

Group theory, in addition to being an elementary part of more complex mathematical theories, models above all our intuition about symmetry as invariance to certain transformations. Since different situations have different symmetries, unlike previous mathematical models which have an intended interpretation, this theory does not have an intended interpretation, but has intended

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non isomorphic interpretations. Thus, some mathematical models are simply sets of axioms without a specific interpretation. However, if they are modelling some important inner intuition about our approach to the world, as group theory does, then they are usually very important. Probably, the most famous example is the Riemann's conception of geometry as a manifold with a metric [Riemann, 1883]. These models found their application half a century after their invention with the appearance of Einstein's general theory of relativity. Today, manifolds are an essential component of mathematics and physics. Although the application was realized so late, it had to happen, because manifolds model successfully the basic mathematical idea about the coordinatization of investigated objects, an idea that generalize such an efficient idea of measuring. Although, due to their generality, the theory of groups and the theory of Riemannian manifolds have no interpretation in our world of internal activities, they grew out of intuition about the world of internal activities, the former on the ideas of transforming objects and combining transformations, the latter on the idea of coordinatization of objects.

In the language of group theory, due to the existence of non-isomorphic interpretations, we sometimes think of a definite, and sometimes of an indefinite ("any") interpretation. However, the very use of language and its logic requires that when we think in a language, we necessarily assume that it is a semantically complete language, no matter how we imagine the interpretation. The situation is the same as when we use variables in our thinking. Whether we attach a certain value to a variable or not, in thinking within a classical mathematical language we necessarily assume that it has a certain value. However, if we think of groups in the language of set theory, then the groups themselves are the values of variables, not interpretations of the whole language as described above, and we think of them differently. The language of set theory allows us to connect and compare groups with each other, without having to know the true nature of individual groups, but possibly their isomorphic copies in the world of sets without urelements. Thus, the process of modelling the initial intuition and the way of working with the constructed models depends on the language in which we model the intuition.

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§ 5. — Mathematical models from other disciplines.

The source of mathematical models does not have to be an intuition about our internal world of activities. They can also be "borrowed" from other disciplines. The nature of our thought and use of language, as well as the way in which we manage the vast complexity of the world, leads to the extraction of a certain structure from such a domain. We extract from the domain certain objects, relations and operations and we describe their properties. If we have thus obtained an important model from that domain then its sentence (axiomatic) part is a mathematical model important for examination. We can use classical mechanics to illustrate this point. Although particles, motion and forces do not belong to mathematics, mathematics can take the structural properties of phenomena (usually described as a set of sentences in an appropriate language) and formally investigate them: the consequences (for example, in the problem of three bodies), the equivalent formulations (for example, Lagrangian and Hamiltonian formulations of Newtonian mechanics emerged in this way), etc.

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§ 6. — Set theory.

In the consideration of any objects, the consideration of the sets (collection) of those objects naturally occurs. In mathematics, this step has a deeper meaning. Namely, the foundational mathematical modelling must model the very intuition about mathematical and thought modelling itself. In the process of thought modelling, we extract a structure from a set of objects, that is to say, we extract some distinguished objects, and some relations and functions over the set of objects. Therefore, the subject of the foundational modelling must be the structures themselves and their parts. We can reduce the description of the structures to the description of their parts. It is the standard result of mathematics that we can describe distinguished objects by functions, functions by relations, and relations by sets. In this way we can reduce the foundational modelling to the analysis of sets. From sets we can build all structures. Also, we can compare such structures using the functions between them. The language of sets provides simple means for describing structures and constructing new structures from the old ones. In this way, sets give us an universal language for mathematics. Furthermore, sets are often necessary for specification. For

example, the specification of natural numbers requires the axiom of induction, which, in its most general formulation, needs the notion of a set. Likewise, the specification of real numbers requires the axiom of continuity, and the specification of Euclidean geometry requires Hilbert's axiom of maximality, and they both need the notion of a set. However, in what way exist the set of natural numbers, the set of real numbers, and the set of space points, when they are infinite? Moreover, when we think of sets, we also consider sets of sets. If we want to have an elegant, rounded and universal set theory, infinite sets are naturally imposed on us, truly the whole infinite hierarchy of infinite sets, together with infeasible operations on them.[12] How can we understand the existence of such sets and operations? Should we reject this theory, which has proven to be very successful, because its objects can be realized only when they are finite? Hilbert, who certainly knew what good mathematics is, said the following on the Cantor's set theory of infinite sets:[13] "This appears to me to be the most admirable flower of the mathematical intellect and in general one of the highest achievements of purely rational human activity," [Hilbert, 1926, p. 167].[14]

The language of set theory presupposes an intended interpretation. In addition to the fact that we can only partially realize it, the interpretation itself is not clear to us in many ways. It is clear that the idea of a set derives from our activities of grouping, arranging and connecting objects and that set theory is an idealized mathematical model for these activities. However, in the very finite part, when we talk, for example, about the set containing three concrete objects o_1, o_2 and o_3, it is not clear what kind of object the set itself is, let us call it $s = \{o_1, o_2, o_3\}$. Formally, we can describe the situation by saying that we have added a new object to the objects, which we then call the set of these objects and which has the unique property that only the objects o_1, o_2 and o_3 and no other belong to it. Such a description would correspond to a combinatorial approach and obviously has a structuralist overtone — the very nature of sets is not important but their relationship to other objects is important. However, another description $s = \{x | x = o_1 \text{ or } x = o_2 \text{ or } x = o_3\}$ of the same set has a different connotation. Now the set is given by

[12] An elegant exposition of the standard ZFC set theory can be seen, for example, in [Enderton, 1977].

[13] Cantor's exposition of his set theory can be seen in [Cantor, 1883].

[14] The complicated and at times dramatic historical development of set theory is described in detail in [Ferreirós, 2007].

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a predicate, so it is a kind of extensional abstraction of an one-place predicate. In this view of sets, as extensions of one-place predicates, they have no structural role but have their own individual nature in our world of meaningful linguistic forms, in the same way that points and directions have their own individual nature in our world of internal spatial activities. Let us note that both the structural and individual view of sets change the initial intuition about the impossibility of realizing infinite sets. In the structural view, the set of all natural numbers is simply a new object. It differs from a finite set only in that there are infinitely many objects in the membership relation with it. Natural numbers can be defined without the concept of infinity.[15] Thus, from a structural point of view, the formation of the set of all natural numbers is not burdened by the concept of infinity, although it has infinite members. In the individual view, the predicate "to be a natural number", as commented above, can be defined without the concept of infinity. Let's compare, for example, that predicate with the predicate "to be an electron". The letter predicate can be defined by a certain experimental procedure. It is well defined regardless of whether there are finitely or infinitely many electrons in the world. It is the same with the predicate "to be a natural number": it is well defined regardless of whether there are finitely or infinitely many natural numbers. In the individual point of view, that predicate, as well as any other that has the same extension, can be considered the set of natural numbers. So, we can conclude that the formation of the set of natural numbers is not burdened by the concept of infinity, although it has infinite members. Thus, both in the structural and in the individual view, the set of natural numbers is a finitely well-formed object. The idea that a set must be "made" of its elements is not present here at all, an idea according to which we can never make an infinite set. Despite all the doubts related to the notion of a set, the constructed mathematical model is very successful. Today, ZFC axioms form its axiomatic part and the model has an intended interpretation, although there are doubts as to what the interpretation is, what we can realize and how we can realize it. Here we only have a more pronounced tension between the basic intuition and the final model, which can ultimately lead to model refinement, model change, or even separation into multiple models.[16]

[15] see, for example, [van Orman Quine, 1963, p. 75]

[16] In addition to other models of set theory that complement, weaken or change the ZFC axioms (see [Fraenkel and Bar-Hillel, 1958] and, for newer alternatives,

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§ 7. — What mathematics is.

The conceptions described above possess all of the essential characteristics of mathematical conceptions. First of all, mathematical conceptions have a clearly defined purpose — to be successful tools in the process of rational cognition, and in rational activities in general. This purpose significantly influences their design and determines their value. We can use mathematical conceptions directly, like the use of numbers, through an ideal model of interaction with the world. We can use them indirectly: (i) like the use of the Euclidean geometry — by changing the interpreted part of the theory into an external interpretation, (ii) like the use of the group theory — by giving just the axioms and their consequences regardless of interpretation, which could be the external one, or (iii) like the use of the set theory — by organizing effectively other mathematical tools. Also, we use mathematical conceptions indirectly, (iv) as constituent parts of more complex mathematical conceptions — as it is, for example, the case with Euclidean space as a tangent space on a Riemannian manifold, or (v) we use them indirectly in the way described above with Euclidean geometry — to interpret them in collections of complex mathematical objects for the purpose of making them more intuitive and more manageable. A multitude of specific mathematical models that are used to model specific problems should also be mentioned here. Such a model is applied directly, its purpose is concrete, and its design and evaluation largely depend on the problem it models.[17]

The conceptions described above purport that mathematics is an inner organization of rational cognition and knowledge, a thoughtful shaping of the part of the cognition that belongs to us. For example, we organize the possible results of measurement into an appropriate number system. The inner organization needs to be distinguished from (but not opposed to) the outer organization of rational cognition, a real shaping of an environment that comprises construction of a physical means for cognition (for example, an instrument for measuring temperature). Mathematics is a process

[Apostoli et al., 2009]), the multiverse view in set theory has recently been developed, according to which there is no "true" mathematical model for the concept of a set, but there are various equally acceptable models [Hamkins, 2012].

[17] In the book [Carrier and Lenhard, 2017], mathematics is analysed as a means of building specific mathematical models, while in the book [Fenstad, 2018] a general model of such use of mathematics is given.

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and a result of shaping our intuitions and ideas about the reality of our internal activities, into thoughtful models which enable us to understand and better control the whole reality. For example, we shape our sense for quantity into a system of measuring quantities by numbers. Thoughtful modelling of other intuitions about our internal human world of activities, for example intuitions about symmetry, flatness, closeness, comparison, etc., leads to other mathematical models. This claim will gain its full meaning only when I explain what the terms "internal world of activities", "intuition" and "mathematical model" mean. That is the content of the rest of this section.

In the introductory part, I briefly described our internal world of activities as the world that consists of activities over which we have a strong control, and which we organize and design by our human measure. The examples I listed there are activities that we can easily recognize in free children's play.[18] These activities form the basis of the adult world of internal activities. These activities develop as we grow up, but this is primarily the development of their design and conceptualization. Their presence in early development stages indicates their biological basis.[19] They are a unique characteristic of the human species, an essential part of our evolutionarily developed abilities by means of which, unlike other species that adapt to the environment, we adapt the environment to ourselves.[20] Of course, cultural evolution and social context play a decisive role in designing and conceptualizing these activities.[21]

There are many examples of highlighting the importance of these activities. Dedekind talks about "the ability of the mind to relate things to things, to let a thing correspond to a thing, or to represent

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[18] In [Čulina, 2022b], it is argued that, contrary to the narrow standards of mathematics education, we best help children in their mathematical development by providing them with an environment in which they will, in free play, and with our unobtrusive help, develop their internal world of activities, design it, conceptualize it and apply it to problem-solving.

[19] The book [Lakoff and Núñez, 2000, p. 28] discusses: "ordinary cognitive mechanisms as those used for the following ordinary ideas: basic spatial relations, groupings, small quantities, motion, distributions of things in space, changes, bodily orientations, basic manipulations of objects (e.g., rotating and stretching), iterated actions, and so on."

[20] "Man is a singular creature. He has a set of gifts which make him unique among the animals: so that, unlike them, he is not a figure in the landscape — he is a shaper of the landscape." [Bronowski, 1974, p. 19].

[21] See, for example, [Kitcher, 1983] for the influence of cultural evolution and [Ernest, 1997; Hersh, 1997] for the influence of human society.

a thing by a thing, an ability without which no thinking is possible" [Dedekind, 1888, p. viii]. Hilbert sees the source of his finitist mathematics in "extralogical concrete objects that are intuitively present as immediate experience prior to all thought", and these objects are "the concrete signs themselves, whose shape ...is immediately clear and recognizable" [Hilbert, 1926, p. 171]. Feferman writes that the source of mathematical conceptions "lies in everyday experience in manifold ways, in the processes of counting, ordering, matching, combining, separating, and locating in space and time" [Feferman, 2014, p. 75]. Hersh writes: "To have the idea of counting, one needs the experience of handling coins or blocks or pebbles. To have the idea of an angle, one needs the experience of drawing straight lines that cross, on paper or in a sandbox" [Hersh, 1979, p. 46]. And so on.

Internal activities are concrete activities with concrete objects: they take place in space and time, and in a given environment — on the table with a pencil and paper, in a ballroom, or on a sandy beach. They are experiential activities, but it is not an experience of the external world, but an experience of our actions in the world available to us and subordinated to us. We experiment not with the objects of the external world but with the possibilities of our actions in the world adapted to us. In this world, Piaget distinguishes two types of knowledge [Piaget, 1970, p. 16-17]. An example of the first type of knowledge is when we pick up two objects and determine that one of them is heavier. This knowledge arose from our action performed on the objects and its source is in these objects: it is knowledge about these objects — knowledge about the world. Piaget calls such knowledge "physical knowledge". But when we order objects, for example by weight, this order was created by our actions and its source is in our activities and not in the objects. The knowledge that has its source in such activities, for example that there is always the same number of objects no matter how they are ordered, is called by Piaget "logical mathematical knowledge". Mathematical knowledge has its source in the world of internal activities precisely in this way: it springs from these activities themselves and not from the objects of these activities. Feferman writes: "Theoretical mathematics has its source in the recognition that these processes are independent of the materials or objects to which they are applied and that they are potentially endlessly repeatable." [Feferman, 2014, p. 75]. However, we cannot

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completely separate activities from the objects of the activities: generally speaking, they depend on the objects. For internal activities, it is important that this connection is weak. Our control over these activities is the dominant force, rather than the influence exerted over them by the objects.

Likewise, it is not possible to draw a clear line where the world of internal activities ends, and activities become external. An example can be made out of constructing and deconstructing objects. When a child does this with Lego blocks, it is surely an internal activity. Although the structure of Lego blocks affects the possibilities of construction, these are activities over which we have a strong control and the possibility of designing them according to our own measure. Constructing and deconstructing stone walls without mortar is certainly not an internal activity, because it requires experience working with weights, centres of gravity and forces in contact. But both activities are the source of the same mathematical idea, the idea of analysis and synthesis of what we are researching: let us examine the phenomenon by breaking it into parts, study those parts and synthesize the knowledge thus acquired into knowledge about that phenomenon. This idea is a mathematical idea because it has its source in our approach to the world, not in the world itself. Although it is present in both mentioned activities, in the internal activity it takes a clear and separate form, while in the other activity it is connected with the physical content. Because of the freedom we have in the world subordinated to us, I believe that we can always internalize external activities, represent them with internal activities in which the mathematical idea will come to its full expression.

To conclude, although it is about concrete activities in a real environment, due to the subordination of that environment to our activities and due to the strong control over these activities and the strong possibilities of their shaping, we can talk about these activities as our internal world of activities, and even about a certain kind of a priority of that world.

The concept of intuition is not an intuitive concept. In [Kitcher, 1983, p. 49] Kitcher writes: "'Intuition' is one of the most overworked terms in the philosophy of mathematics, Frege's caustic remark frequently goes unheeded: 'We are all too ready to invoke inner intuition, whenever we cannot produce any other ground of

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knowledge.'"[22] In [Hersh, 1997, p. 61-62], Hersh gives a whole list of different and unclear meanings of this term, but also emphasizes its central role in the philosophy of mathematics: "1. All the standard philosophical viewpoints rely on some notion of intuition., 2. None of them explain the nature of the intuition that they postulate., 3. Consideration of intuition as actually experienced leads to a notion that is difficult and complex, but not inexplicable., 4. A realistic analysis of mathematical intuition should be a central goal of the philosophy of mathematics.". However, intuition about our internal world of activity is completely clear, it is intuition in the original sense of that word — "immediate awareness" — devoid of any ambiguities and misunderstandings. In my opinion, mathematical intuition is precisely this intuition.[23]

Major mathematical models, like the models described above, arise from intuition about our internal activities and organization. It is from these concrete activities that the idea of an idealized world emerges, the world that expands and supplements the internal world of activities.[24] In his book [Mac Lane, 1986], Saunders Mac Lane describes this process on a multitude of examples. The table on page 35 of the book shows a whole list of examples of activities from which certain ideas are born, and from these ideas mathematical concepts and models arise. For example, movements contribute to the idea of change, whose formulation contributes to the concepts of rigid motion, transformation group and rate of change; estimating contributes to the ideas of approximation and closeness, and their formulation contributes to the concepts of continuity, limit and topological space. However, what is the nature of idealized mathematical models? What is the nature of the irrational numbers that Dedekind "creates" to fill the "gaps" in the linearly ordered field of rational numbers [Dedekind, 1872]? Hilbert writes

[22] For example, Brouwer in his First act of intuitionism [Brouwer and Heyting, 1975, p. 509]writes: "...intuitionist mathematics is an essentially languageless activity of the mind having its origin in the perception of *move in time*, i.e. of the falling apart of a life moment into two distinct things, one of which gives way to the other, but is retained by memory. If the two-ity thus born is divested of all quality, there remains the *empty form of the common substratum of all two-ities*. It is this common substratum, this empty form, which is the *basic intuition of mathematics*."

[23] This understanding of mathematical intuition is fundamentally different from Kant's or Gödel's [Gödel, 1964] understanding of intuition.

[24] The very creation of ideas is largely an unconscious process: "Most thought is unconscious ...inaccessible to direct conscious introspection. We cannot look directly at our conceptual systems and at our low-level thought processes." [Lakoff and Núñez, 2000, p. 5].

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about infinity as a paradigmatic example of an ideal mathematical element: "...nowhere is the infinite realized; it is neither present in nature nor admissible as a foundation in our rational thinking ...The role that remains to the infinite is, rather, merely that of an idea — if, in accordance with Kant's words, we understand by an idea a concept of reason that transcends all experience and through which the concrete is completed so as to form a totality ..." [Hilbert, 1926, p. 190]. In [Feferman, 2014, p. 4] Feferman writes: "The basic conceptions of mathematics are of certain kinds of relatively simple ideal world pictures that are not of objects in isolation but of structures" and "The basic objects of mathematical thought exist only as mental conceptions". For Hersh, mathematical objects are "mental objects with reproducible properties" [Hersh, 1997, p. 66].

I consider all the above descriptions of mathematical objects and concepts to be insufficiently clear because they refer to insufficiently clear psychological terms. The same applies to other descriptions of mathematics as a human invention, which I found in the literature. My view of the nature of mathematical models stems from my view of the essential role of language in thinking. My view of the essential role of language in thinking has its inspiration primarily in the works of von Humboldt [Humboldt, 1836] and Whorf [Carroll, 1956], and is explained in detail in [Čulina, 2021]. According to this view, language is not only a medium for expressing and communicating thoughts, but a medium in which thoughts are realized, a medium in which thoughts take their completed form. In the words of von Humboldt: "Language is the formative organ of thought. Intellectual activity, entirely mental, entirely internal, and to some extent passing without trace, becomes, through sound, externalized in speech and perceptible to the senses. Thought and language are therefore one and inseparable from each other. But the former is also intrinsically bound to the necessity of entering into a union with the verbal sound; thought cannot otherwise achieve clarity, nor the idea become a concept. The inseparable bonding of thought, vocal apparatus and hearing to language is unalterably rooted in the original constitution of human nature, which cannot be further explained ...without this transformation, occurring constantly with the help of language even in silence, into an objectivity that returns to the subject, the act of concept formation, and with it all true thinking, is impossible." [Humboldt, 1836, p. 50].

It is also important to point out here that, concerning thinking, the abstractions are the language abstractions, and not thinking

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about the so-called abstract objects. We talk about concrete objects (which can be real or imagined), and abstraction is achieved by extracting certain predicate and function symbols with which we talk about objects. For example, we count using concrete objects. Thus, the language of arithmetic talks about concrete objects (whose nature is not important to us), and with the language itself we achieve the appropriate abstraction. The language of arithmetic separates what is important to us when we use objects as numbers (first number, successor, predecessor, comparison, ...) from what is not important (e.g., size of marbles if we use collections of marbles for numbers, or font of decimal digits if we use sequences of decimal digits for numbers).

In this way of looking at the nature of thinking, which in its final effect is the creation and use of language, we can only realize mathematical models by building an appropriate mathematical language. By choosing names, function symbols and predicate symbols, we shape the initial intuition into a structured conception. Since the conception usually goes beyond our constructive capabilities, the constructed language has only partial interpretation, and that interpretation is internal — the interpretation in our internal world of activities. Since the interpretation is only partial, and because the imagined domain of interpretation is usually infinite, we cannot determine the truth values of all sentences of the language. Therefore, we must further specify the conception by using an appropriate set of axioms. Thus, the final mathematical model (theory) is a junction of axioms and partial internal interpretation of an adequate language. Sometimes, as we have seen on the example of the group theory, a mathematical model can be reduced to a set of axioms without an internal interpretation, although it arose from a corresponding intuition about the world of internal activities. Sometimes, the internal interpretation can be a total interpretation in another mathematical theory, as we have seen on the example of Euclidean geometry.

This way of looking at mathematical models can be acceptable to those who believe that mathematical concepts exist even without language. Given the concrete nature of language, unlike the vague nature of mental phenomena, they can accept mathematical language as a convenient representative of mental conceptions. I would also like to point out here that viewing abstract thinking as

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the creation and use of language does not mean denying the complex thought processes that take place behind it. This is a functional view: only the final effect of thinking is considered.

Furthermore, we must not forget that although a mathematical model is the final product of modelling an intuition about our internal world of activities, in real mathematical practice it is never isolated from the source from which it originated. This is especially important because a mathematical model, generally speaking, is not complete — there are multiple interpretations that are extensions of partial interpretation and that satisfy axioms; and intuition always leaves room for completion. In addition to testing a mathematical model as a means of rational cognition, it is also tested by how well it models the initial intuition, i.e. how well its consequences correspond to the intuition from which it emerged. In [Lakatos, 1976], Lakatos clearly demonstrated the importance for mathematics of this internal testing and revision of mathematical models (Lakatos calls this activity quasi-empirical mathematics). Feferman [Feferman, 2014] distinguishes mathematical conceptions by how close they are to everyday practice. The more distant they are, the less clear he considers them. For those conceptions that are completely clear, such as the conception of natural numbers, he believes that every statement within such a conception has a definite meaning, and thus has a truth value, regardless of whether we are able to determine its truth value or not, so the corresponding logic is the classical first-order logic. For those conceptions at the other end of clarity, such as the set theory, which lack some aspect of definiteness, he believes that the concept of truth may be partial and that the appropriate logic is semi-intuitionistic. Similarly, Ferreirós [Ferreirós, 2016] distinguishes mathematical conceptions according to whether their truth is based in our cognition and direct practice, such as the conception of natural numbers (Ferreirós then speaks of elementary mathematics characterized by certainty) or require additional hypotheses, such as the conception of the continuum (Ferreirós then talks about advanced mathematics, which is characterized by the presence of hypothetical statements — statements that gain or lose their objectivity through mathematical practice). The newer philosophy of mathematics, the so-called philosophy of mathematical practice, devotes special importance to these processes of interaction of mathematical models and basic intuitions, as one of

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the main sources of mathematics (see, for example, [Ferreirós, 2016; Mancosu, 2008]).

Although I consider it important how strongly mathematical models are connected to the internal world of activities, my view of mathematical models is more uniform. In my opinion, the basic criterion for evaluating mathematical models is their success as a tool of rational cognition. Seen in this way, there is no difference, for example, between the arithmetic and Hilbert spaces, although their connection to the internal world of activities is different. Also, in my opinion, there is no difference in the meaning of the truth of different mathematical models. To me, all mathematical statements are specifications, whether they specify what we will do with natural numbers in our internal world of activities, or whether they specify the truth of the continuum hypothesis. From such an understanding of the truth of mathematical concepts, it follows that the logic of mathematical models is classical logic, although perhaps in some situations, with regard to the problem that is being solved, it makes sense to look at mathematical models with non-classical logics.

Just as mathematical models are not isolated from the sources from which they arose, they are not isolated from each other either. Set theory is a natural environment for formulating and comparing mathematical models. In such an approach, axioms become the definition of a certain type of structure. However, set theory analyses the described structures in a uniform way, without going into their nature, whether they are extracted from the external world or from the internal world of our activities. Thus, although it gives an elegant mathematical description, set theory can also hide the true nature of mathematical models.

Mathematics is largely an elaborated language. The "magic" of mathematics is largely the "magic" of language. Inferring logical consequences from axioms, we establish what is true in a mathematical model. This can be very creative and exciting work and it seems that we discover truths about an existing exotic world, but we only unfold the specification. The key difference from scientific theories is that the interpretation here is in our internal world of activities and not in the external world. The external interpretation of a scientific theory enables us to test the theory, whether it has a power of rational cognition of nature. If the theory has such power then at least some objects of the theory exist in the primary sense of the word and at least some sentences of the theory are truths about

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nature. If the language does not have such a part, and that is the case with mathematics, then the objects we are talking about exist only within the conception (story), although they do not exist in the external world. Equally, if the language does not have an experimentally verifiable part then sentences we consider true within the conception are not true in the external world. We cannot experimentally verify that $|| + || = ||||$ $(2 + 2 = 4)$, not because it is an eternal truth of numbers, but because it is the way we add tallies. Likewise, we cannot experimentally verify that $(x^2)' = 2x$ because it is the consequence of how we imagined real numbers and functions. Mathematical objects are, possibly, through a partial internal interpretation, objects extracted from our internal world of activities, and mathematical truths are, possibly, through a partial internal interpretation, truths about our internal activities.[(25)] We are free to imagine any mathematical world. The real external existence of such a world is not important at all; all that matters is to be a successful thought tool in the process of rational cognition. In Cantor's words, "the essence of mathematics lies precisely in its freedom" [Cantor, 1883, p. 66]. The only constraint is, inside classical logic, that conceptions must not be contradictory. For Hilbert, in mathematics to exist means to be free of contradictions. In Hilbert's words: "the proof of the consistency of the axioms is at the same time the proof of the mathematical existence", [Hilbert, 1900, p. 265]. In Dedekind's words, "numbers are free creations of the human mind" [Dedekind, 1888, p. vii].

These views are in sharp contrast with historical views that mathematical truths exist really in some way and that we discover them and not create them. Historically, this change of view occurred in the 19th century with the appearance of non-Euclidean geometries. The new philosophical view of mathematics has freed the human mathematical powers and it has caused the blossoming of modern mathematics. It is a nice example of how philosophical views can influence science in a positive way. According to the old views mathematical truths are a particular kind of truths about the world. An exemplar is the Euclidean geometry — according to the

[(25)] Some statements get their truth through the partial interpretation of language, for example that 2+2 = 4, while for some statements the partial interpretation is not enough to determine their truth (e.g. the axiom of the completeness of real numbers or the continuum hypothesis). Then the assignment of truth to these statements is, at least in part (the axiom of the completeness of real numbers) or completely (the continuum hypothesis), a specification of an idea that transcends the internal world of activity.

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old views, it discovers the truths about space. The appearance of non-Euclidean geometries, which are incompatible with Euclidean geometries but are equally logical in thinking and equally good candidates for the "true" geometry of the world, has definitely separated mathematics from the truths about nature. It has become clear that mathematics does not discover the truths about the world. If it discovers the truths at all they are at best the truths about our own activities in that world. From my personal teaching experience, I know that looking at mathematics as a free and creative human activity is a far better basis for learning mathematics than looking at it as an eternal truth about an elusive world. Claims that mathematical objects exist in the external world, real or special, and that mathematical truths are truths about such a world, are unfounded and lead to religion. Such a belief can of course be very inspiring and can produce very powerful mathematics, but mathematics itself is a human creation, in its final form a junction of axioms and a partial internal interpretation of an adequate language.

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A triptych on empirical philosophy of mathematics. Part II: Who?

Deborah Kant & Benedikt Löwe

Abstract. This is the second part of a tripartite paper discussing the research focus of the CIPSH Chair *Diversity of Mathematical Research Cultures & Practices* (DMRCP). We discuss a philosophical approach called "empirical philosophy of mathematics" and say why it should be done, who should do it, and how it should be done. This second part deals with the question "who?".

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§ 1. — Introduction.

The *International Humanities Council* (*Conseil International de Philosophie et des Sciences Humaines*; CIPSH) has established a *CIPSH Chair* entitled *Diversity of Mathematical Research Cultures & Practices* (DMRCP) at the Universität Hamburg. This institution aims to study mathematics as a human discipline using methods from the empirical social sciences and the humanities with a particular focus on the effect of differences in research cultures and practices on mathematics.

While the term "philosophy" is not part of the name of the CIPSH Chair, it stands in the tradition of a wider research community that was or is involved in the conference series *Perspectives on Mathematical Practices* (PMP) (Brussels; 2002 & 2007), the network *Philosophy of Mathematics: Sociological Aspects & Mathematical Practice* (PhiMSAMP) funded by the *Deutsche Forschungsgemeinschaft* (DFG; 2006–2010), as well as the closely related *Association for the Philosophy of Mathematical Practice* (APMP). Like the CIPSH chair, this research community is motivated and driven by questions of philosophical nature about mathematics and studies the discipline of mathematics on the basis of empirical data.[(1)] For the purpose of this paper, we shall introduce the loosely defined term

empirical philosophy of mathematics

for this particular approach: by *empirical philosophy of mathematics*, we refer to philosophy of mathematics whose philosophical arguments make use of empirical data about mathematical practices, i.e., the way mathematicians speak, act, and collaborate, their institutions and incentive structures, their implicit and explicit value judgments etc.

In our *Triptych* consisting of three closely connected parts (cf. also [25, 26]), we shall argue why it is reasonable to assume that empirical data about mathematical practices is relevant for philosophy of mathematics, discuss whose task it is to provide such empirical data, and give a description of empirical philosophy of mathematics in practice.

[(1)] Cf. [30] for a discussion of the peculiar relationship between this research community and the field of philosophy.

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The type of empirical data. We emphasise that we stipulate that *empirical philosophy of mathematics* specifically uses *empirical data about mathematical practices*, not just any type of empirical data.

This distinction is considerably more salient for other disciplinary philosophies of science than for philosophy of mathematics. For an empirical science, e.g., physics, chemistry, or biology, there are two very different types of empirical data that might be relevant for the philosopher: empirical data about the physical, chemical, or biological world and empirical data about scientific practices in physics, chemistry, or biology.

Only very extreme philosophical positions (e.g., positions that in principle deny the existence of physical reality or our ability to gain empirical information about that physical reality) would reject the view that empirical data about the physical, chemical, or biological world has some bearing on philosophy of physics, chemistry, or biology. Use of such empirical data in philosophical arguments is common in philosophy of science and would not constitute *empirical philosophy of physics, chemistry, or biology* according to our terminology. The use of empirical data about the practices of physicists, chemists, or biologists, on the other hand, would not be accepted by all or possibly even most philosophers of science in philosophical arguments. It is the latter type of empirical philosophy of science that we are focusing on.

Note that this distinction is less straightforward for a non-empirical discipline (such as mathematics): the question whether empirical data has any relevant impact on mathematics at all is a debated philosophical position within philosophy of mathematics. *Empiricism* is a philosophical position about mathematics, taking a particular view of how mathematical knowledge is obtained: an empiricist philosopher of mathematics would claim that empirical knowledge plays an important role in mathematical epistemology.[(2)] Depending on her philosophical views, an empirical philosopher of mathematics could argue either for or against empiricist philosophy of mathematics on the basis of empirical data about how mathematicians corroborate their knowledge in their research practices. Most importantly, being an empirical philosopher of mathematics does not imply any position on the question of empiricism in mathematics.

[(2)] For an overview of the early empiricism of John Stuart Mill (1806–1873), cf. [45, § 4.3]; modern empiricist positions usually relate to the Quine-Putnam indispensability argument [6, 42].

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CIPSH Chairs. It is reasonable to assume that very few readers know what a CIPSH Chair is, as this is a relatively new programme instituted by the International Humanities Council (*Conseil International de Philosophie et des Sciences Humaines*, CIPSH), modelled after the better known programmes of UNESCO Chairs, in 2018. In spite of their names, neither UNESCO Chairs nor CIPSH Chairs are are professorships, but rather research networks. We quote from the UNESCO documentation:

> A UNESCO Chair is a project and a team at a university or a higher education or research institution that partners with UNESCO in order to advance knowledge and practice in an area that is a priority for both the institution and UNESCO. [46, p. 5]

Similarly, a CIPSH Chair is a network of several research institutions, centred around one institution that created the CIPSH Chair and appointed its chairholder. In addition to the network that constitutes each individual CIPSH Chair, the CIPSH Chair programme creates another higher-level network where the chairholders can exchange experiences and best practices. The CIPSH Chairs programme was proposed and discussed by the 33rd General Assembly of CIPSH in Liège on 6 August 2017 and created by the CIPSH Executive Committee on 16 April 2018 during its meeting in Xiamen. Until early 2024, eight CIPSH Chairs were created, covering topics such as ethnolinguistics, digital humanities, and spiritualism.

DMRCP. *Diversity of Mathematical Research Cultures & Practices* (DMRCP) is one of these eight CIPSH Chairs. It studies cultural phenomena in mathematical research, with a particular emphasis on phenomena where different research cultures diverge on their practices, to obtain a multi-dimensional description of the nature, epistemology, and methodology of mathematical research, contrast it with other disciplines, and describe and understand the cultural variance across various boundaries (e.g., national, disciplinary, or sectorial boundaries). This research must necessarily involve a plethora of disciplines, including (in alphabetic order) anthropology, history of science, mathematics education, philosophy of science, psychology, and sociology, and actively involve research mathematicians.

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The goal of the network is to catalyse research cooperation between the network nodes, to coordinate the global research activities in this area, and to provide infrastructure (both for research support and research dissemination) to those researchers working in the area.

The authors of this paper are the scientific coordinator and the chairholder of DMRCP, respectively. DMRCP has both Senior Fellows and Fellows: the Senior Fellows together with the chairholder form the DMRCP Council, the main deliberative body of the network that makes strategic decisions and elects the Fellowship.

The inaugural Senior Fellows were Hanne Andersen, Carolin Antos-Kuby, Helen De Cruz, Karen François, Christian Greiffenhagen, Gerhard Heinzmann, Matthew Inglis, Catherine Jami, Brendan Larvor, Ursula Martin, Helena Mihaljević, Eva Müller-Hill, R. Ramanujam, Dirk Schlimm, and Bart Van Kerkhove.[(3)] DMRCP Fellowship is open to researchers interested in the aims and scope of DMRCP: readers interested in the network are invited to contact the first author to discuss a nomination for election.

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The triptych & this paper. This paper forms the second part of a triptych consisting of three papers in which we shall discuss and partially answer three fundamental questions about the approach of *empirical philosophy of mathematics*: "**Why** should we do empirical philosophy of mathematics?", "**Who** should provide the empirical data for empirical philosophy of mathematics?" and "**How** should we do empirical philosophy of mathematics?" The three parts of the triptych elaborate on and continue the discussion started in the papers [29, 30, 31], respectively.

In Part I of the Triptych [25], we discussed the fact that philosophical concepts have non-philosophical counterpart concepts that are used in ordinary language; in particular, concepts from philosophy of mathematics have counterparts used in the technical language of mathematicians. Statements about these non-philosophical concepts are empirical statements. We argued that the meta-philosophical position that these empirical statements have no bearing whatsoever on our philosophical theory is tantamount to severing the link between the philosophical concepts and

[(3)] Hanne Andersen resigned from her Senior Fellowship on 1 July 2023 and was replaced by Henrik Kragh Sørensen on the DMRCP Council.

their natural language counterparts entirely. In line with naturalistic positions in philosophy, we consider this to be an undesirable meta-philosophical position.

For this paper, we shall assume that we have convinced the reader that empirical facts about mathematical practices are philosophically relevant and discuss how to obtain these empirical facts: in particular, we wonder **who** is responsible for providing the empirical data for this endeavour. We shall argue that the philosophers of mathematics will have to play an active part in the empirical research themselves.

The answer to the question **Who?** means that empirical philosophy of mathematics is a major effort that requires learning the methods of other disciplines and training our junior researchers in non-philosophical research skills. Part III of the triptych will provide a description of **How** to do empirical philosophy of mathematics and what the active role of the philosopher is.

§ 2. — Empirical data about the natural world vs. empirical data about human practices.

We remind the reader that *empirical philosophy of mathematics* is philosophy of mathematics whose philosophical arguments make use of empirical data about mathematical practices. In § 1, we highlighted the difference between using empirical data about the physical world and using empirical data about human institutions, concepts, and activities.

When philosophers use empirical data about the physical world, they are usually not involved in the empirical work themselves; rather, they rely on empirical research done by scientists independently of the philosophical debates.

To give a number of examples: Firstly, the rich philosophical literature connecting the question of the existence free will to findings in the neurosciences (cf. [38]) relies on the work by many social and cognitive psychologists such as Libet's seminal work on *Bereitschaftspotential* [28] that was done without active involvement of professional philosophers. In the philosophy of mind, discussions about the unity of consciousness, e.g., Parfit's thought experiments on split brains [39], use empirical facts about neurosurgical split-brain procedures which were originally developed in the 1930s, long before philosophers started to think about these

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problems.[4] Finally, in the philosophy of physics, an extensive literature on philosophical issues concerning quantum mechanics exists [36, 47], most of which is relying on experimental findings of physicists that were done independently of philosophers' work.

We should emphasise that we do not wish to interpret the term "independently" too strictly in this description: notions such as free will, consciousness, personal identity, and the nature of physical reality are philosophical concepts of wider interest to a community extending well beyond the world of academic philosophy, including the scientists who did the empirical work in our examples. As a consequence, it is only natural that the scientists were influenced by their own philosophical views as well as the history of philosophical discussions about the notions. Given our academic practices, it is not unlikely that these scientists interacted with philosophers on funding panels, university committees, in academies, at interdisciplinary conferences, or within the collegiate university, and therefore had the chance to exchange thoughts with philosophers about the philosophical consequences of their work.

However, the causal and temporal relationship in these cases is clear: the scientists did the empirical work without the immediate or primary intention to contribute to the philosophical debate and they did it prior to the active debate in the philosophical literature. To consider the example of Parfit's split brain thought experiment: the brain procedures of *commissurotomy* and *corpus callosotomy* were first used in the 1930s and 1940s with purely therapeutic intentions (avoiding the spread of an epileptic seizure to spread from one brain hemisphere to the other), long before Parfit came up with his thought experiment in the 1970s.

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§ 3. — Operationalisation.

In the following, we shall use a particular empirical statement about mathematical practice as a running example that will inform our discussion.

> Given a sufficient amount of time and effort, any two mathematicians will be able to agree on whether a proof in their joint area of expertise is correct and complete. (Φ_1)

[4] Cf. [34, Footnote 18] for a discussion of "medically informed [philosophical] publications".

This statement is commonly used in philosophical debates about the special epistemic status of mathematics[5] and it has been the focus of empirical testing in a series of papers.[6]

This empirical sentence contains several terms that need to be specified before it can be empirically tested, corroborated, or refuted. The concepts "mathematician", "agreement", "area of expertise", and "sufficient amount of time" require *operationalisation* to determine whether a particular data point is a positive or negative case for (Φ_1). E.g., we need a clear empirical condition for whether a test subject is a mathematician, whether a particular proof falls into their area of expertise, and whether the observed behaviour of two mathematicians constitutes agreement.

Note that the necessity of operationalisation is not a special feature of empirical sentences in the social sciences; it is also required for sentences in the natural sciences. Consider the following natural science sentence structurally analogous to (Φ_1).

> Given a sufficiently high increase in temperature, any substance in liquid state will go into the gaseous state. (Φ_2)

The sentence (Φ_2) contains natural language terms such as "liquid state", "gaseous state", and "sufficiently high temperature" that need to be operationalised in order for any experiment to serve as a corroboration or refutation of (Φ_2). While the natural language terms in (Φ_2) are not precise, there are natural interpretations for them that are uniformly accepted meanings for the concepts, except in cases specifically designed to test the limits of our definitions.

In contrast, for concepts dealing with human institutions and practices, there is very little intersubjective standardisation. E.g., there are many different operationalisations of the term "mathematician" that are all eminently reasonable. To give a few examples:

M1 A person who holds a higher education degree in mathematics.

M2 A person who has had research experience in mathematics in the past, as witnessed by at least one research publication.

M3 A person who is currently employed to do mathematical research.

Clearly, **M1**, **M2**, and **M3** do not describe the same group of people; in fact, for any two of them, it is easy to find a person who belongs

[5] For a general introduction into the discussion of the special epistemic status of mathematics, cf. [40, 41] and [32, § 1.1, in particular Footnote 4].

[6] Cf. [24, 49, 50] and the discussion in [31, pp. 28–29].

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to the former, but not the latter and vice versa. The differences are large enough that the status of (Φ_1) is likely to depend heavily on which of the operationalisations we choose.

At the same time, for each of the three operationalisations, it is easy to think of contexts in which it is a reasonable operationalisation of the term "mathematician". So, how do we pick the right one?

We shall tackle this question in § 4; before we do so, we should like to make two highly relevant methodological remarks:

First we observe that the level of specificity of our operationalisations provided is not sufficient for actual decisions: taking **M1** as an example, someone who holds a degree called *Bachelor of Science in Mathematics* clearly falls under **M1**, but what about a degree in "Business Mathematics" or "Mathematical Physics" of "Statistics"? In the North American university system, do only graduates who majored in mathematics count, or also those who minored? If the degree is in a language other than English, should we look for degrees that have the dictionary translation of "mathematics" in their name?[7] Details like these have haunted empirical projects doing analyses of mathematics graduates and their career trajectories such as the *Pipeline Project* of the *International Mathematical Union* as it is not clear whether national statistics of people falling under **M1** from different countries measure the same thing [3, 9].

Furthermore, the choice of operationalisation does not just affect the interpretation of the data, but can influence the data itself. Suppose that we are collecting data to test (Φ_1) and conduct a study with test subjects. For each data point, we need to decide whether (according to our chosen operationalisation) the two individuals involved were mathematicians, the result fell into their area of expertise, and whether the they agreed on correctness and completeness after an appropriately long period of discussion. Typically, some of these decisions will be based on asking the test subjects a number of relevant questions. But questions about their status as researcher and area of expertise may very well have

[7] To illustrate the issue underlying the last question, note that "science" is the dictionary translation of the German word "Wissenschaft" (or the Dutch word "wetenschap"), but the disciplines that fall under the heading "Wissenschaft" are not the same as the disciplines that fall under the heading "science" and English does not even have a concise term that would describe the scope of the German term "Wissenschaft".

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an effect on their later behaviour in the experiment and therefore change the actual data.

§ 4. — Adequacy of operationalisations.

We have now seen that an empirical statement about mathematical practices such as (Φ_1) is not testable per se, but requires operationalisation; furthermore, the operationalisation is not unique and the choice of the operationalisation affects the result of the test. How does the empirical researcher decide which operationalisation to use?

In the empirical social sciences, we need to return to the original *research intention*: the research project that prompted us to ask the empirical question (Φ_1) had some goal; each of the operationalisations will produce false positives and false negatives with respect to our expectation.[8] Whether these false positives and false negatives created by our choice of operationalisation jeopardise our study depends, among other things, on whether the existence of these discrepancies is in conflict with the original intention of the research project. Similarly, at the end of § 3, we mentioned that in practice, getting the information needed for interpreting the data may affect the data; again, whether we are worried by these effects in the experimental set-up can only be determined with the original research intention in mind.

To illustrate this with a medical example: suppose we have a viral test and a test population in which some test subjects are infected with the virus and others are not. We try to determine how good our test is; whether we are more worried about false negatives or false positives depends on the context. If the virus is deadly and there is a cheap drug with no side effects, we should like to have no false negatives and do not mind administering the drug to some people who are not infected; in contrast, if the virus is not particularly dangerous and the drug is outrageously expensive or has major side effects, we should aim to avoid false positives.

[8] E.g., a mathematics professor who has a degree in English literature and therefore does not meet the requirements of **M1**, a very prolific and eccentric mathematical researcher who has never published and therefore does not meet the requirements of **M2**, a historian who was hired by a mathematics department and whose contract by administrative default mentions the phrase "mathematical research" placing them within the scope of **M3**, even though they are not expected to do mathematical research.

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The design decisions made as part of the experimental methodology must and will reflect this.

In the natural sciences, the difference between acceptable operationalisations tends to be much lower than in the social sciences, and we often find that methodologies are largely independent from the research intentions. This higher degree of intersubjective stability means that empirical results can be used as evidence for different research projects; this is the situation found in the examples of philosophy using empirical data from the natural sciences in §2.

That is not equally true in the social sciences: an experiment done within a particular framework has incorporated research design decisions based on the research intention of the original empirical researcher which may be incompatible with the research intention of a different researcher who wishes to use these results later. An example of this issue is highlighted in the literature by the meta-discussion about psychological experiments in economics and psychology: experiments in economics routinely identify payoffs in decision situations with financial incentives; that identification is not done in experiments performed by psychologists, even if dealing with very similar research questions.[9]

We make no claims about a general structural difference between the natural and the social sciences: in fact, variations in operationalisation and design decisions based on research intentions exist in the natural sciences as well as in the social sciences. However, we claim that the degrees of variance and dependency are considerably higher in the social sciences than they are in the natural science.[10]

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[9] Cf. [2, 5, 48]. Hertwig and Ortmann explicitly discuss this: "Empirical tests of theories depend crucially on the methodological decisions researchers make in designing and implementing the test. (p. 383) ... [E]xperimental economics and corresponding areas in psychology (in particular, behavioral decision making) have very different conceptions of good experimentation. (p. 384) ... [T]he most important argument motivating financial incentives, is that most economics experiments test economic theory, which provides a comparatively unified framework built on maximization assumptions (of utility, profit, revenue, etc.) and defines standards of optimal behavior. Thus, economic theory lends itself to straightforward translations into experiments employing financial incentives. ... Psychologists typically do not rely on a similarly unified theoretical framework that can be easily translated into experimental design. (p. 390)" [18]

[10] We cannot back our claim up with numerical data: doing so would be an interesting research question for a methodologist of science or a philosopher of social sciences.

This phenomenon will play an important role in our argument in § 5.

§ 5. — The sartorial metaphor.

In [30], the second author introduced a *sartorial metaphor* for the uses of empirical data in philosophy. The empirical work in philosophy as mentioned in § 2 is described as

> using empirical data like ready-to-wear or off-the-rack clothes, provided on the shelves and racks of a store with no direct input by the customer expressing his or her preferences of clothing style or fit; on the other end of the spectrum, one would have the bespoke experience where the customer can determine every detail of the garment and the garment is then tailored exactly to the specifications of the customer and made to fit his or her body perfectly. Those who have the appropriate skills could even become the tailors themselves, getting rid of any need to communicate wishes and desires. Bespoke empirical philosophy would be a project in which the philosopher works very closely with the empirical scientist and designs an experiment or other observational activity jointly with her or him; the extreme case of bespoke would be do-it-yourself where the philosopher becomes an empirical scientist and does the empirical work herself or himself. Of course, the more extravagant and non-standard your desires and wishes with respect to your clothes are, the less likely it is that you will find these off the rack and you might have to move towards bespoke tailoring. [30, p. 36]

As was argued in [30], the easiest approach for the empirical philosopher interested in an empirical statement such as (Φ_1) is to search the library for empirical studies that provide answers to (Φ_1) that have already been provided by empirical researchers, i.e., *off-the-rack* empirical philosophy.[(11)] If these studies do not exist

[(11)] This approach avoids a situation where the philosopher has to do empirical work themselves, reducing both the involved effort and also the risk of inadequate empirical data, as pointed out in [30, p. 37]: "[I]f you find a high-quality garment off the rack that fits you very well, it may be considerable better than a bespoke

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(yet), they can either approach empirical researchers and ask them whether they would like to do these studies for the philosophers ("*bespoke*") or do the studies themselves ("*do-it-yourself*").

For empirical philosophy of mathematics, we first observe that empirical data about mathematical research practices is rare: while the area of *Science and Technology Studies* developed in the 1970s and 1980s provided empirical data about research practices in the natural and technological sciences, mathematics has been considered by this community only rarely. In the main monograph on the sociology of mathematics [17], Heintz comments "[d]ie Soziologie [begegnet] der Mathematik mit einer eigentümlichen Mischung aus Devotion und Desinteresse" (p. 9); after Heintz's seminal book came out, a number of papers have been published,[(12)] but compared to the sociology of other sciences, the literature is still very sparse. The literature of cognitive science and psychology on number cognition is very rich,[(13)] but relatively few cognitive scientists deal with research practices in mathematics.[(14)]

However, the discussion in §4 shows that the problem is not merely the dearth of sociological and cognitive science studies dealing with questions such as (Φ_1): even if they existed, the sociologists' and cognitive scientists' choice of operationalisation (and therefore methodology of collecting the data) would have been based on their research intentions and consequently unlikely to give the empirical philosopher precisely what they need for their philosophical argument. This combination of the general lack of empirical research about mathematical research practices and the close relationship of the small number of existing results with the research intention of the original author means that any successful use of off-the-rack empirical data in empirical philosophy of mathematics would be a serendipitous exception to the rule.

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garment from a mediocre tailor or (in the case of most of us with no expertise in tailoring) a garment that you made yourself. Similarly, in empirical philosophy, there are advantages and disadvantages to both ready-to-wear and bespoke approaches. In the first instance, philosophers are not empirical scientists, so do-it-yourself empirical philosophy requires that the philosopher acquire the skills and learn the techniques of another discipline, wasting time and energy that could be spent on something that they are more qualified for (such as doing philosophy) and possibly even leading to sub-standard or flawed empirical work."

(12) Cf., e.g., [11, 12, 13, 14, 15, 16, 33, 43].

(13) Cf., e.g., [1, 4, 7, 8, 10, 27, 37].

(14) A notable exception is the work by Matthew Inglis and his collaborators, including a number of empirical philosophers of mathematics; cf. [19, 20, 21, 22, 23, 35, 44] and also the references in Footnote (6).

Without overstretching the metaphor, we can say that in tailoring as well as in empirical philosophy, there is a spectrum between ready-to-wear and bespoke. Not every problem with style or fit or every desirable detail that cannot be found in off-the-rack clothes requires a fully bespoke order. A more affordable option is *alterations*, either done by an alterations tailor or the customer themselves. Letting out or taking in, changing a button, or adding simple design features can be done with a substantially lower degree of tailoring expertise than required for full do-it-yourself tailoring. Similarly, not every study used in empirical philosophy of mathematics needs to be designed from scratch; instead, the literature can provide studies that would be useful after alterations: e.g., a sociological study of a different discipline can be modified and replicated for the case of mathematics. We believe that the future of empirical philosophy of mathematics lies in collaborations between philosophers and empirical researchers as well as the acquisition of enough empirical research skills by the philosophers to do the necessary alterations required to adapt and perform existing studies to the philosophical context.

§ 6. — Conclusion.

We have argued that in the particular case of empirical philosophy of mathematics, off-the-rack empirical data is hard to find. This means that empirical philosophers of mathematics will either need to request bespoke studies from empirical scientists or do their own empirical work.

Contractual research is common in the applied sciences; it is the model closest to bespoke tailoring in our sartorial metaphor: a research team that lacks expertise for doing some particular task hires another research team to perform that task for them. However, empirical studies cost time and money and it is unlikely that philosophers, in particular philosophers of mathematics, have the financial means to afford paying empirical scientists to do specific research tasks for them.

As a consequence, bespoke empirical philosophy of mathematics is more likely to consist of collaborations where philosophers and empirical scientists work together on a project and become coauthors of the resulting publications. This also comes at a cost: in order to convince the empirical researcher to join the project,

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the research questions have to be of interest to them and therefore might require some modification in the form of compromises. Furthermore, the philosophers have to become familiar enough with the methods of empirical research to contribute to the methodological decisions and whether they are in line with the original research intention.

If the empirical philosopher cannot find empirical scientists to collaborate with or their expectations of compromise are not acceptable to the philosopher, the philosopher will have to become an empirical researcher herself, either by altering studies from the social science literature or by designing new studies. As a consequence, the answer to our question "**Who** should provide the empirical data for empirical philosophy of mathematics?" is that the philosophers have to be actively involved in the collection of empirical data.

By and large, philosophers are not trained for this and training a new generation of empirical philosophers of mathematics to have the expertise to either contribute to or even lead empirical research is a major additional effort that is necessary to do empirical philosophy of mathematics successfully. The CIPSH Chair DMRCP aims to play its role in this effort by coordinating training activities for junior researchers in the field.

Embarking in the project of training a new generation of philosophers in additional skills is a major commitment; in Part III of our Triptych [26], we follow a paradigmatic example to highlight how empirical philosophy of mathematics is done.

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The Meaning of the Formula
A tribute to Gilles Châtelet

Bernard Teissier

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An offensive philosophy cannot content itself with endlessly rambling on about the 'status' of scientific objects. It must stand resolutely in the vanguard of the obscure, not seeing the irrational as 'diabolical' and resistant to articulation, but as the means by which new dimensions can emerge.

Gilles Châtelet, *Les enjeux du mobile*

Gilles Châtelet (1945-1999) was a mathematician, physicist and philosopher whose philosophy of science I greatly appreciate, as well as the passion with which he developed it, particularly in [C].

René Thom (1923-2002) was an extraordinary mathematician and philosopher who used mathematics to construct qualitative models of changes in the state of systems as a function of changes in the parameters on which these systems depend. These models are universal in the sense that they do not depend on particular systems but organise their evolution. Some of his ideas have become famous under the name of *Catastrophe Theory*. His fundamental work on the subject is [Th1].

Abstract. A reflection on the relation between rationality and the meaning of the tools it uses, centered on the approaches of Gilles Châtelet and René Thom.

§ 1. — Introduction.

At last! more than two centuries after Kant, our culture is beginning to seriously question the dominance of what is commonly called *rationality* and its limits. One symptom of the relevance of this questioning is the resurgence of anti-scientific schools of thought, sometimes of religious origin. It seems to me that part of our society's disaffection with the sciences stems from a misunderstanding of the nature of rational thought, which is perceived as having a strong inhuman component. Along with René Thom, Gilles Châtelet is one of those who have had the courage, in modern times, to emphasise the extent to which the most rational sciences, and in particular physics and mathematics, are also human sciences, i.e., dependent on human nature.

As far as Thom was concerned, in fact, it was more a question of creating a rational language, of mathematical origin, sufficiently flexible and rich to be able to talk about phenomena, such as the creation of language or the stages of embryogenesis, which seemed to escape classical rational thought. And this led him to a vision of the meaning of forms and their dynamics that was so innovative that it is still poorly understood except by a few experts. Gilles Châtelet, seemingly in the opposite direction, was relentless in his search for the forks in the road of thought, those mysterious, misty places where new ideas and great questions are born. And he sought them in our humanity, in our gestures rather than in formalism, while recognising the heuristic value of the latter.

Both, apparently looking for different things, were in fact two of the great thinkers of an *adult* rationality, if we accept that rationality must be understood as a *dialogue following the rules of rational thought* between man and his environment. There are other modes of dialogue, such as art and poetry.

From this point of view, reductionism is the infantile disease of rationality that consists in asserting that the dialogue must end one day, and that man will have the last word. This is contrary to all plausibility, even if man has, in areas where his perception of the world is on the right scale and even in certain areas where it is not, said many things that he finds very interesting, and that nature does not contradict.

This desire to have the last word obviously greatly diminishes the quality of the dialogue, and it has another consequence: it

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encourages man to place himself in the role of God[1] , and thus to deny his humanity in order to be quite certain of always being right (this is also known as the quest for objectivity, in the name of which some people shoot at anything that seems to come under the heading of introspection or psychology). An infantile disease, I tell you!

Thom in [Th2] and Châtelet in [C] had clearly understood that the real problem of modern rationality, which has tools for founding truth, is the problem of the foundations of meaning. It is these foundations which give the dialogue its quality and encourage us to pursue it. The important thing is that they are useful for the dialogue, which implies on the one hand that we can use them for reliable (verifiable) intellectual constructs and on the other that they have meaning for us. The objects and concepts of science must be compatible with these two requirements.

In fact, this compatibility is rather analogous to that which exists between the administrative life of a citizen and his real life. In this case, the citizen, *alias* the mathematical or physical object or concept, *leads a double life* in our scientific imagination: one life as an axiomatically defined object, and another far more exciting life as an object endowed with meaning. It is above all in this real life that it is used in dialogue with the world.

And Gilles Châtelet tirelessly tracked down the occurences of extensions to the world of ideas of our experience of space and movement, which he calls "gestures" for short. Such extensions, for example by an "allusive stratagem" or a concentration of ambiguity, provoke the simultaneous creation, often by a partly unconscious process, of a mathematical or physical object and its meaning. He understood that the objects and concepts we create are there first and foremost to carry a meaning, to perform a "gesture" in the mathematical or physical domain or to resolve an ambiguity, and only very distantly afterwards to have a "status" that enables them to be bearers of truth.

Once you have seen things in this way, you re-read the history of scientific ideas with fascination and, like Châtelet, you follow Oresme in his attempt to define speed rigorously in a framework where the division of magnitudes of different natures does not exist, and Grassmann in his capture of space in the nets of the algebra he creates for this purpose. One of the intuitive foundations of this

[1] As René Char wrote: It seems that God always has the last word, but he says it in such a low voice that no one ever hears him.

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algebra is that a rectangle *must* be a product, while Oresme is constrained by the idea that a product *must* be an area. Both refer to the Greek view that we "see" a product of two lengths as the area of the rectangle that has them as sides. But for Grassmann, the product of vectors *generates* space; the Greek idea has been transfigured. We are really rediscovering rationality as a dialogue in which concepts and objects are created by the effect of human impulses very strongly linked to our perception of the world. To question the objectivity of these concepts and objects is quite simply irrelevant compared to what drives them. It is the infantile attitude mentioned above.

Conceiving of space as a product is one of the strongest ideas in geometry, one that drives the Cartesian vision as well as Riemann's local definition and, in its infinitesimal form, the definition of differential forms, which also contains the orientation of space necessary for the theory of integration. And nowhere have I found more forcefully expressed than in Châtelet's work the idea that it is really in this type of conception that the real driving forces of scientific construction lie.

§ 2. — Where are the foundations?

In my opinion, the only criticism that can be made of Châtelet's presentation is that he does not specify the origin of the mental operations or gestures that he evokes so well to explain the "true" meaning of the constructions of physics. He does place the origin in our "being in the world" but stops there.

I have suggested elsewhere ([T1], [T2]) that it is useful to look for the source of meaning in the complex structure of our perceptual system, in the links between our different perceptions, particularly visual, motor and vestibular, which integrate them into a single perception of the world around us. To this structure must be added unconscious judgements and impulses such as those to determine causes or origins, to make analogies, to complete what is incomplete.

Certain proto-mathematical objects are very probably accessible to primates, and I like to say that a mathematician understands a demonstration when he or she has succeeded in explaining the situation to the primate inside him or her. For me, the "primate" represents the *unconscious* sedimentation of our ancestors'

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experiences of the world into the structure of our thinking. For example, rational mechanics associates with any dynamical system (for example a collection of spinning tops spinning on each other or a sun/planets system), a space in which the temporal evolution of the system is represented by a curve, a trajectory, and this has a strong meaning for our primate, who has been throwing stones for hundreds of thousands of years. During their apprenticeship, and indeed throughout their lives, mathematicians learn to make sense of increasingly elaborate objects using such examples. And understanding a proof literally means that it makes sense to the person looking at it. This is very different from logical verification, and much more complicated to explain!

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§ 3. — Afterwords: mathematicians need a sense of the formula.

Along with René Thom, Gilles Châtelet embodied the slogan I like to repeat:

"The mathematician must have a sense of the formula."[(2)]

This sentence, which is intended to be creatively ambiguous in the sense of William Empson in [E], suggests that the mathematician must not only know how to concentrate a lot of information in a few concepts and signs or formulas, but also know the origin of the meaning of each concept and each term in a formula. Those who reflect on their discipline must first try to determine, as mathematicians, the meaning of each concept or formula, before trying to make it the subject of a philosophical discourse.

The formulas relate to quantities that condense a very large amount of mathematical or physical information, such as volume, curvature, the number and nature of solutions to an equation, or energy, mass, temperature and many other concepts.

A good example (which remains to be treated from the point of view proposed here) is the theorem of Hopf (see the excellent book [M])

$$\chi(X) = \sum_{x_i \in \mathrm{Zer}(v)} \mathrm{Ind}_{x_i}(v)$$

[(2)] "Avoir le sens de la formule" translates in English as "To have a way with words" so that the sentence "Le mathématicien doit avoir le sens de la formule" presents the same ambiguity in a more amusing way.

which asserts, for a compact differential variety without boundary X, the equality of its Euler-Poincaré characteristic and of the sum of the indices at its singular points (the points where it vanishes) of a differentiable and sufficiently general field v of vectors tangent to the variety.

The term on the left is topological, the one on the right is differential. In particular, the result implies that there is no vector field without zero on even-dimensional spheres. So, it is quite an elaborate result, which has had a prodigious mathematical legacy.

But how is this meaning to be determined? It is to this question that Châtelet provides some very original and valuable answers, of which I have tried to give an idea above: a concept may be the abstraction of a "gesture", i.e., an atavistic experience of our primate, such as throwing a stone, or it may resolve an apparent contradiction. In all cases, it has a genesis. It seems a long time ago (in the 1930s) that Bourbaki could write "Le traité prend les Mathématiques à leur début..." (The treatise takes Mathematics at their beginning) when writing Chapter 1 of the first Book of "Élements de Mathématique", which deals with the theory of sets, then barely forty years old.

Mathematics has no more beginnings than the human species; it has a history and a dynamic that are studied by specialists. But if, like Châtelet, we are also interested in their "metaphysics", in what they reveal about our being in the world – in short, in what a true philosophy of mathematics should be concerned with – then we must not only revisit history, but also realise that he is describing the coastline of a continent where almost everything remains to be discovered.

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