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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

(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 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

(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.

§ 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:

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(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.

> "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

> 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.

> "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.

§ 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,

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[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

> 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.

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).

"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.

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...

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).

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).

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)

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/`

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

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

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":

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[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.

> "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.*).

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.

§ 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

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).

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.

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).

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.

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.

> 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

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[(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 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.

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.

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.

§ 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.

> "*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).

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.

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:

> "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).

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.

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.

> "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

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.

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).

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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