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A Philosophical Task in Our Times

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My rather grandiose title will have no other excuse than that it has been extracted from an article by Ian Hacking on the idea of styles of scientific reasoning[1]. According to Hacking, his reflection on style was part of a general problem that went far beyond the philosophy of science and that he did not hesitate to call "a philosophical task in our times". This task consisted, first of all, in making room for two apparently incompatible orientations in our vision of scientific knowledge. The first has emerged, since the 1960s, with the development of *Science Studies* and related approaches. It gives prominence to micro-historical events, controversies, local contexts (sociological, political, institutional, etc.), and "minor" authors. On the contrary, the other more classical approach underlines the strongly cumulative aspect of scientific knowledge, in a regime of historicity that Hacking qualified as "Braudelian". It is interested above all in the persistence, notwithstanding endless local variations, of global conceptual structures. Attempting to reconcile both approaches seemed to require, in Hacking's eyes, a revival of nothing less than "metaphysics" — by which he meant the revival of interest in this discipline that occurred in so-called "analytic" philosophy after Quine, and in particular from the work of Putnam and Kripke. To put it more simply, it was a matter of bringing together important questions about the status of "truth" and "reality" with the apparently contradictory images of science that emerge from the two previous orientations. The difficulty, Hacking emphasized, is not that we have to choose between one orientation or the other, as some have believed and as many still believe, but rather that they both appear to be correct even though we do not have a theory of knowledge that can accommodate them. The reference to Putnam was then natural since it

[1] I. Hacking, "Statistical language, statistical truth and statistical reason", in E. McMullen (ed.), *Social Dimensions of Sciences*, University Notre-Dame Press, 1991, pp. 130–157.

was precisely one of the problems he had wanted to raise, under the heading of the question of the stability of reference — a question that emerges when we note both the cumulative character of scientific knowledge and the local variations to which its expression is subject.

Whatever one thinks of the fact that this is one of the philosophical tasks in our times, one can agree that there is a central problem that arises today in our image of the world. Moreover, it plays an important role in the difficult dialogue between history and philosophy of sciences[(2)]. However, this question has scarcely reached our vision of mathematics, where it is nevertheless particularly salient. In fact, if there is any knowledge that appears to us as strongly cumulative, it is indeed that of mathematicians. Whatever the legitimate mistrust we may have towards the idea of "progress", it seems difficult not to concede that we recognize some results found in Archimedes, Li Zhi (Li Ye) or Nilakantha Somayaji as "true" (perhaps at the cost of misunderstandings, it does not matter). We think, on the other hand, that we know "more" in mathematics than they did. At the same time, "local" approaches to mathematics have gained considerable momentum in recent decades. Even when they do not recognize themselves in the picture drawn by *Science studies*, even when they strongly claim to belong to "conceptual" history, most historical studies now set themselves the task of reconstructing the *specific way* in which mathematical rationality was able to unfold in such and such a context. This specificity is often expressed by difference, i.e. we see more and more clearly that while Euclid, Al-Haytham, Liu Hui, Baskhara I, Leibniz, Lagrange, Kronecker and Banach, put forward truths that we recognize as ours, they did not think them exactly as we do.

But there is more, and it is at this point that the question becomes more than simply methodological. Indeed, the variability of contexts brings with it the variability of descriptions and opens up a question for history as well as for philosophy: what does mathematics talk about in the variability of its historical and geographical forms? Can I be satisfied with the fact that Euclid and Grothendieck use comparable terms (translatable into each other) to designate natural numbers, and conclude that they are dealing with the "same" object? This is the question of reference and more precisely of its stability across historically and culturally situated expressions.

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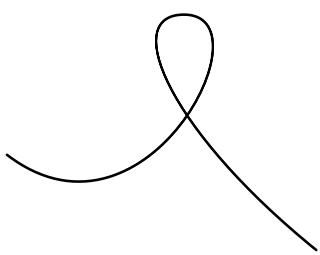
(2) See problems 7 and 8 ("Locality and globality") in Peter Galison "Ten Problems in History and Philosophy of Science", *Isis* 99 (2008), pp. 111–124.

Now, whereas the solution to this problem envisaged by Putnam and Kripke consisted in grounding reference independently of descriptions (always revisable and variable) and anchoring it in an "external" reality, with which we interact causally (what has been called "semantic externalism"), this solution seems to be forbidden — or, at least, very costly — in mathematics. Except in some extreme forms of Platonism where the intuition of ideal objects is compared to a form of "perception", philosophers tend to agree on the idea that mathematical objects are not with us in forms of causal interactions. My aim in this article will be to convince the reader that this is a central, though neglected, problem in the dialogue between philosophy and mathematics, and then to sketch out a few ways to answer it. I will limit myself, for lack of space, to an essentially programmatic approach.

§ 1. — Position of the problem.

In order to make the problem from which I would like to start tangible, I will begin with an exchange between Claude Chevalley and Oscar Zariski reported by Serge Lang:

> Chevalley and Zariski (one of the luminaries, along with Weil, Serre and Grothendieck, in the revival of algebraic geometry) were having a discussion about curves, and neither seemed to understand the other. In desperation, Chevalley asked Zariski, "What do you call a curve?" They were in front of a blackboard, Zariski said, "Well! For me it is this", and he drew a curve:



$$f(x, y) = 0$$

A curve acording to Zariski *A curve acording to Chevalley*

> And he continued: "And for you, what is a curve?" Chevalley answered: "It is not at all that for me, for me it is $f(x, y) = 0$" ![3]

[3] Marcel Berger, *Cinq siècles de mathématiques en France*, Paris, apdf, 2005, p. 179 (my translation).

This dialogue will perhaps surprise an outside observer. Two mathematicians, who had a major influence on the development of their discipline, who used to read each other's papers and exchange ideas, finally realize that they do not understand each other. More intriguingly perhaps, it is not a discussion about a particularly complex object, some higher homotopy group residing at the outer reaches of their knowledge, but about the seemingly simplest and most common idea in geometry, that of a curve. Yet mathematicians know that this situation is common — even if their practices of communication tend to banish it from public exchanges. It is true that this is the kind of disagreement that one prefers not to bring to the forefront, especially in a science that wants to be marked by "certainty". This does not make the dialogue with philosophers and historians any easier: mathematicians sometimes seem to suffer from a curious schizophrenia when they turn to the outside world and suddenly forget all the misconceptions, differences of opinion, and incomprehension that they had in the corridors a few moments before. This situation is not new. If we open Euclid or Archimedes, we are struck by the fact that their treatises do not contain any elements of reflection on their objects and methods, let alone on polemics. However, we know from almost contemporary testimonies that mathematics was already full of controversies about the nature of objects, the strength of this or that proof, and the legitimacy of this or that approach[(4)]. This phenomenon is even more evident for periods and cultural contexts, such as the Renaissance and the European Early Modern period, where the regime of controversies and discussions was an integral part of scholarly sociability.

Note that the debates reported by Proclus and Eudemus are not quarrels instigated by philosophers ignorant of mathematics. They involve actors as important as Menelaus and Geminus, Euclid and Apollonius. The early modern age provides us, for the reasons I mentioned, with many other examples, from the quarrels between Fermat and Descartes over their respective methods of tangents to those of Pelletier, Viète and Clavius over the angle of contact, not to

[(4)] An interesting account of some of these controversies can be found in Proclus' commentary on the first book of Euclid's *Elements* (*Procli Diadochi in primum Euclidis elementorum librum commentarii ex recognitione Godofredi Friedlein*, Leipzig, Teubner, 1873 [henceforth *In Eucl.*]). Although Proclus lived in the 5th century CE, he claims to depend for part of his sources on Eudemus of Rhodes, a disciple of Aristotle who lived in the 4th century BCE.

mention the clash over the methods of "indivisibles", and then over infinitesimals. Closer to us, the controversy between Kronecker and Jordan on the reduction of bilinear forms, Weierstrass' mockery of Riemann's "geometrical fantasies", Poincaré's anathema of set topology, Severi's disputes with the younger generation of algebraic geometers, Arnold's invectives against the Bourbaki style, to say nothing of the various "foundational" quarrels and the ongoing debates on the validity of the "inter-universal Teichmüller theory", etc. , should remind us that these discussions are, in fact, constant.

This is a field that has been of particular interest to the recent history of mathematics because it shows, if indeed it were necessary, that mathematics is no less subject to misunderstandings and disagreements than other disciplines. The cases we have mentioned can be described, at first glance, by the fact that the actors belong to different contexts (choose here the category that suits you best: tradition, style, school, paradigm, epistemic configuration, etc.). The phenomenon that emerges is then, according to the first branch of the problem raised by Hacking, that knowledge is strongly context-dependent, to the point that actors may not mean the same thing by the same term. This is the pole of instability of reference that all history of knowledge seems to manifest and from which mathematics does not seem to be able to escape.

Note that for that reason, it would be wrong to reduce the phenomenon in question to that of controversies, which form only a very particular case. Between Chevalley and Zariski, for example, even if one can relate them both to different "styles", there is no controversy. More generally, it is not difficult to find an equivalent of this type of exchange marked by misunderstanding or disagreement among actors belonging to the same school/tradition/epistemic configuration. The quarrel between Leibniz and Bernoulli over the logarithms of negative numbers is a case in point — all the more interesting, as Euler pointed out, because both had perfectly fair arguments. I will mention another, taken from the same context. When the quarrel between the supporters of differential calculus and the supporters of the "old style" (as Varignon called them) broke out in the Royal Academy of Sciences, the defenders of Leibnizian formalism naturally turned to their master for support. Here is the way the latter reports this episode:

> When they were disputing in France with the Abbé Gallois, Father Gouye and others, I told them that I did

> not believe that there were truly infinite or truly infinitesimal magnitudes; that they were only fictions, but useful fictions, in order to abbreviate or to speak universally, like the imaginary roots of algebra $(\sqrt{-1})$... But as the Marquis de l'Hôpital thought that by this I was betraying the cause, they begged me to say nothing about it, except what I already had said in the Leipzig *Acta*, and it was easy for me to comply with their request. (à Pierre Dangicourt 11 Septembre, 1716, Dutens III, 500–501)

This situation is not at all exceptional. To take up examples we have already seen, Proclus, while arguing that Euclid had formulated his demonstrations in the best possible way, points out that some proofs would better express the reasons for the fact under consideration if they were expressed by means of motion, an approach that is however avoided in his master's treatise(5). Van Schooten, the great promoter and editor of Descartes' mathematics presented his master's famous method of tangents on the same footing as that of... Fermat!(6) Closer to us, Hermann Weyl, while claiming a Riemannian heritage in complex analysis, had no difficulty in integrating Weierstrass' approach, which Felix Klein had nevertheless presented as belonging to an irreducibly different "style"(7). What is obviously troubling in these exchanges is that the actors sometimes seem to disagree not only on major epistemic values that could always be claimed to be external to their practice, but on the very characterization of the objects and the methods of reaching them(8).

(5) *In Euclidem* 69. See Orna Harari (2008), "Proclus' account of explanatory demonstrations in mathematics and its context", *Archiv für Geschichte der Philosophie* 90 (2), pp. 137–164.

(6) *Geometria à Renato Descartes anno 1637 Gallice edita* [...]. *Operâ atque Studio Francisci a Schooten*, Amsterdam, Elsevier, 1659, pp. 252–253. On the method of normals in Descartes and his quarrel with Fermat, see V. Jullien, *Descartes. La 'Géométrie' de 1637*, Paris, PUF, 1996, pp. 101 sq.

(7) See H. Weyl, *Die Idee der riemannschen Fläche*, Berlin, Teubner, 1913 and *Oeuvres mathematiques de Riemann* trans. by L. Laugel; with a pref. by M. Hermite; and a discourse by M. F. Klein, Paris, Gauthier-Villars, 1898.

(8) With a few exceptions to which I will return, it is very significant that the so-called "philosophy of mathematical practice" has massively focused on the question of values: purity, fecundity, explanatory and aesthetic character of proofs, see Paolo Mancosu (ed.), *The Philosophy of Mathematical Practice*, Oxford, OUP, 2008.

§ 2. — The question of reference.

This problem is particularly important because it gives us a synchronic view of a phenomenon that unfolds diachronically as *the very fact of the history of science*. That science can evolve is indeed marked by the fact that the descriptions that one can give of the same object can change, that it advances, as Cavaillès said, "by deepening and erasing"[9]. When it is a question of proposing a more precise or better suited description of the objects, this does not really pose a problem. But the same cannot be said for correcting a description in a way that is not a simple improvement. In this case, in fact, a property that we thought belonged to an object will be rejected (or there will be no "correction") and the two descriptions, the old and the new, will therefore be incompatible.

It is this phenomenon that Putnam pointed out in order to contest the idea (of Russellian origin) according to which we access objects through definite descriptions, i.e. a set of statements that can be formulated with the help of a list of predicates and that, when the description is complete, would suffice to determine "what we are talking about"[10]. Among the arguments that he mobilized were famous cases of evolution of our scientific concepts, such as the different theories of the electron that followed one another from Rutherford to Schrödinger. We know, in fact, that the "planetary" model sees the electron as a particle endowed with a position and a velocity determined at each instant, while the "cloud" model sees only orbitals (regions determined by a probability of presence of the particle) and reject the idea that one can determine both the velocity and the position. These two descriptions are incompatible: either the electron has a determined position or it does not. The second interpretation is not simply a way of specifying the first. But supposing we accept the latter, how can we explain that we were "already" targeting the same object with a description *that did not correspond to it*?[11]

(9) *Sur la logique et la théorie de la science*, Paris, Vrin, 1997, p. 78.

(10) Putnam's immediate target is of course Quine and his slogan that "to be is to be the value of a variable". For Quine, in fact, the philosopher has to evaluate ontological commitments on the basis of scientific discourses, all of which are assumed to be reformulated in first-order logic, and whose variable values constitute the entities we are dealing with in this or that domain.

(11) See, for example, H. Putnam, "Language and Reality" in *Philosophical Papers, Vol. 2: Mind, Language and Reality*, Cambridge, Cambridge University Press, pp. 272–290.

A natural solution, Putnam noted, is to accept that our ways of referring to objects are in fact *independent* of the descriptions we give them. This is ultimately rooted in interactions with real exemplars or paradigms (which can be grounded on direct or indirect perceptions, like observations on the screen of a cathode ray tube), whose extension has no reason to be perfectly fixed (our description can lead us to finally reject an exemplar as not being of the same type as the others). But this does not mean that we necessarily have to interact with scientific observations in order to be able to speak of an electron. The other crucial mechanism that Putnam was pointing to is deference to experts: we trust experts in the way they relate to different objects with which we have no interaction (except through them). In both cases, the reference is thus not fixed by the meaning of the terms alone, but by mechanisms external to the descriptions.

Before proposing some examples indicating that mathematics has no way of departing from this observation, I would like to make two more general remarks. First of all, even though the initial difficulties here come from a tradition which considers that the study of language is the gateway to philosophy, one should not believe that they depend on such a framework. Insofar as it is a question of refusing that signification can fix the reference, these difficulties immediately extend to all the traditions, of idealist or phenomenological origin, which would support comparable theses but by placing themselves at a level which would be that of thought (or of "concepts") or of consciousness. This is a point that Putnam himself has stressed, hammering home the point that meanings cannot be "in the head". Moreover, the mechanism of deference to experts is not the same as that of intersubjective agreement on which phenomenology has placed great emphasis. It is not a question of saying that my conceptions must agree with those of other subjects, but rather that I defer the explicitation of the reference of certain terms.

A second remark is that we will not be able to get away with a moderate relativism that would quickly concede that our actors do not speak of the same object when they provide incompatible descriptions of it. Contrary to the cases that have been put forward by authors such as Bachelard, Kuhn, Foucault, Lakatos, Kitcher, etc., we are not saying that the sciences evolve, from time to time, by changing major conceptual frameworks (which is *also* true). It is a question of pointing out the ubiquity of the mechanism of conceptual variability which can be located on a very small time-scale and sometimes in a single individual. We have already seen an example of this with Bohr, who was an important actor in the elaboration of the two incompatible conceptions of the electron that we have mentioned. There is therefore no need for several actors, the very fact of "better understanding" an object and that this better understanding leads us to revise our own judgments (and thus possibly to deny certain "essential" properties) is enough to open the question: how do we then relate to the "same" object? This is why the problem raised by Putnam is, in a sense, much more significant than the one raised by Kuhn and his successors: it is not a question of saying that different "paradigms" or "styles of thought" generate distinct objects and that incomprehension prevails between them. The phenomenon of conceptual change prevails within any framework of thought. To hold that each conception produces its own objects would be to support a regime of generalized misunderstanding, including with ourselves. There would be, in the end, only a dusting of meanings and objects, whose apparent relations of identity would never be more than illusory (a bit like the way the Humean self is constructed as a simple name for a bundle of experiences). One will certainly find some forms of Wittgensteinism ready to happily grant this pulverization of meaning into a myriad of language games, more or less well joined to each other (this is the slope towards which *Science Studies* naturally leads according to David Bloor)[(12)]. But, as Putnam points out, one wonders who can then put forward this description of meaning (which is true of *all* contexts)?[(13)] Relativism is here, as it often is, merely a hollow reproduction of the "divine point of view" that it intended to contest by arrogating to itself, it is not clear how, a point of view on *all meanings*.

But let us leave these metaphysical quarrels there and turn to mathematics which, as I have said, has remained relatively distant from these questions. The phenomena we have just encountered are however just as present in mathematics, and even more problematic, since descriptions are our privileged mode of access to mathematical objects and it does not seem that we can interact causally with an "exemplar" of a mathematical object. Let us look, for example, at the first propositions of Simon Stevin's *Arithmetique*,

[(12)] David Bloor, "Wittgenstein and Mannheim on the sociology of mathematics", *Studies in the History and Philosophy of Science*, vol. 4, n 2, 1973, pp. 173–191.

[(13)] See H. Putnam, *Realism with a Human face*, ed. By J. Conant, Cambridge, MA: Harvard University Press, 1990, chap. 1.

one of the places where symbolic algebra was invented in the Renaissance[14]. Stevin reproduces a dialogue (perhaps fictitious) that he had with scholars of his time about what a number is, and takes issue with the fact that their definitions do not include the "unit" (in the tradition stemming from Euclid, the unit is the "principle" of numbers defined as "multiplicities of units"). Defining number by measure, he then points out that there is no need to even conceive of a number as a discrete entity (a unit being a number, there is no problem in dividing it and calling numbers rational or "broken", as well as irrational or "deaf"). More generally, he introduces a notion of "geometric number" and does not hesitate to advance, in a passage that has remained famous, "that there are no absurd, irrational, irregular, inexplicable or deaf numbers" (p. 33).

One could say that this is only a matter of *extending* the Euclidean notion in a conservative way, but this is not the case: Stevin's natural numbers are not Euclid's natural numbers, if only because the former must include *zero* and *one*. But the consequences of this first, apparently innocuous, extension are considerable. As Stevin forcefully points out, it authorizes a "community" of numbers and magnitudes that was *forbidden* for an ancient Greek mathematician (notably by making the geometric point correspond to the arithmetic zero). In fact, for Euclid, the world of numbers and the world of magnitudes behave in a closed way, and this is manifested by several surprising features in the *Elements*, such as the fact that no demonstration passes from the geometric books to the arithmetic books, or the fact that he finds himself having to formulate two theories of proportions (one for numbers and the other for magnitudes). The question is therefore not whether or not to add two elements to a set, it mobilizes different understandings of what a "number" is in depth. But how can Stevin and Euclid be said to relate to the "same" object "number"? Assuming that this is defended, a second, no less awkward question must be answered: how are they — and how are we ourselves — supposed to have access to this object *independently* of the (partly incompatible) descriptions that have just been given?

At this point, the example is so elementary that one might say that it is resolvable by some recourse to a supposedly universal structure of our mind that would give its "real" basis to all these descriptions, say "the human activity of counting" or "the pure intuition of time as succession". This hypothesis is not easy to

[14] S. Stevin, *L'Arithmétique*, Leyde, Plantin, 1585.

defend, given the considerable historical and cultural variability that we know today in the different numbering systems. But we do not even need to engage in such discussions. For the phenomenon in question obviously also applies to a multitude of mathematical concepts created from scratch in more recent times. Let us take the example of the concept of "group" invented by Galois. Historical studies have first taught us that his original concept is not exactly the same as ours, so that the question arises as to what we mean when we say that Galois created "the" concept of group. And there is more. This concept was very quickly taken up in different meanings and practices by authors such as Cayley and Dedekind(15). This multiplicity of views has deeply nourished "our" concept of group, which can be presented either in an "abstract" way (à la Dedekind) or by "generators and relations" (à la Cayley). One might then be tempted to say that such terms function as proper names that are transmitted in the manner of Kripkean "rigid designators". But besides the fact that this view leaves the problem unresolved (what makes us believe that it is a matter of rigidly designating "the same thing" if we have no other access to mathematical objects than descriptions?), it is not supported by historical studies. Galois does not use the word "group" to designate what we isolate in his text as "group". Moreover, it is not uncommon, in fact, for something that appears to us at one time as the same object to have received several names ("differential"/"fluxion"; "table"/"matrix", etc.). Symmetrically, the same name can be used by different actors to designate different things ("structure" for the young Bourbakists/for Glivenko and Ore; "sheaf" in the sense of projective geometry/in the sense of Leray, etc.). More generally, the solution that would consist in basing semantic externalism on the mechanisms of historical transmissions (this was a path that Kripke favored for the transmission of proper names and that has been generalized by some for theoretical terms(16)) will come up against all the cases, now well documented, where no transmission is historically attested. Let us think of the presence of the "arithmetic triangle" or of the way in which systems of linear equations

(15) See Caroline Ehrhardt : "Un concept mathématique, trois notions : Les groupes au XIXe^{e} siècle chez Galois, Cayley, Dedekind", *Images des Mathématiques*, CNRS, 2010, `http://images.math.cnrs.fr/Un-conceptmathematique-trois.html`.

(16) See P. Kitcher, "Theories, Theorists and Theoretical Change", *The Philosophical Review*, Vol. 87, No. 4, 1978, p. 519-547.

are already solved "by the Gauss pivot method" in Chinese mathematics, of the techniques of calculation on "series" developed in the Kerala school, of the "resolution of quadratic equations" on such and such a Babylonian tablet, etc. Finally, how can we not evoke more generally the cases where a mathematical fact is rediscovered or those of independent discoveries?

§ 3. — Intermezzo on Symbolic knowledge.

As is often the case, those in contemporary philosophy who think that the study of its history is only of interest to antiquarians often end up merely echoing a problem that has arisen before, albeit in a different guise. In fact, the question of whether a description can fix the reference of a concept was already at the heart of the quarrel of "ideas" that came about in the 17th century. Descartes, as we remember, proposed a criterion of truth very different from adequacy to reality, which had dominated until then in the Aristotelian tradition: "certainty" (not supposed to rid us of, but to guarantee "adequacy"). This certainty, of which mathematics was an example, was to be evaluated by the fact that it was based on "clear and distinct" ideas, characterized by their indubitability. But this opened up an obvious question that was to tear his successors apart: should we consider that an idea that is not "clear and distinct" refers to "nothing" or that it refers to something "improperly"? For Malebranche, inspired by Augustinian Platonism, the word "idea" should be reserved for what true knowledge targets. For Arnauld, one could and should speak of "idea" in all cases, under penalty of not being able to speak of a "false" idea. Leibniz intervened in this debate, in a famous text, by pointing out that the positions were vitiated by never producing a criterion of "clear and distinct"(17). For the rest, the question seemed purely terminological. It was enough, for example, to use the term "notion" for the different conceptions that we have and reserve the term "idea" for the case where there would be adequacy (but this is only one choice among others).

(17) *Meditationes de cognitione, veritate, et ideis*, *Acta Eruditorum*, November 1684, transl. Ariew and Garber, *G.W. Leibniz: Philosophical Essays*, Indianapolis, Hackett, 1989, p. 25.

In his argument in connection with mathematics, Leibniz made a remark of capital importance. If we characterize "clear and distinct" notions by the fact that we are able to provide a list of necessary and sufficient characteristics to acknowledge them (a path that was proposed by some Cartesians and that clearly anticipates complete definite descriptions), then this criterion will fail to guarantee the truth of the discourse in the sense of adequacy or correspondence to a "reality". In fact, I can give a list of criteria to "clearly and distinctly" identify an object like the regular decahedron: it is a convex polyhedron with ten identical faces, whose vertices have the same number of edges. But there is no such thing as a regular decahedron, as was demonstrated by Euclid, in a still unsatisfactory way, and then by Descartes by means of algebra (a demonstration that Leibniz knew because he had read it in Paris). Thus the philosopher underlines the fact that our knowledge can be "clear and distinct", but still contain hidden incompatibilities.

This defines a type of knowledge that Leibniz called "blind" or "symbolic". It is based on the fact that we use "symbols" in our reasoning, in the most general sense of the term (including words in everyday language, or diagrams), which we assume to have already been fully analyzed. "We use them", he says, "in algebra and arithmetic, and, indeed, almost everywhere"[18]. Let us note in passing that the mechanism of deference is therefore in no way limited to the deference to experts. We also apply it to ourselves insofar as we think that we have elucidated certain concepts at such and such a moment of our learning (but no less frequently we realize later that "we had not understood anything" about this concept). The key point of the argument is that, except in the extremely rare cases where we have an "intuitive" knowledge of the objects concerned, that is to say where we could extend the analysis until we reach notions known by themselves, the descriptions which would allow us to delimit clear and distinct ideas cannot by themselves guarantee an access to truth. They must therefore be accompanied by procedures attesting to the possibility of the objects. Leibniz then isolates three types of attestation to which I will return later: conceptual analysis (mostly imperfect), the construction of the object by means of a "genetic" or "causal" characterization, and experience.

[18] *art. cit.*, p. 19.

§ 4. — The relationship to "*practice*".

If we return to the problem raised by Putnam, we see immediately that the road to semantic externalism seems difficult to follow in mathematics. Among the three strategies mentioned by Leibniz, this path would correspond to the one that allows us to stabilize the reference by resorting to a form of experience (in the form of a causal interaction with exemplars). However, there is a widely shared consensus that mathematics is not empirical in the sense that we do not develop an *a posteriori* knowledge of its objects and that we do not have causal interactions with them. Even authors who grant a primitive role to experience in mathematics, such as Brouwer and his followers, generally understand it as an experience *in thought*. There are certainly forms of "Platonism" (such as the one Gödel seems to have defended), which posits that we interact with mathematical objects that are "out there", independently of us, and that we "perceive" in a certain way. But this position is no less difficult to hold for reasons that Putnam immediately encountered in the case of sensible objects themselves. In fact, even for the latter, the idea that they can be "there, outside, independently of us" is full of mystery. This is the meaning of a long series of arguments he then deployed against "metaphysical realism".

Yet the road to semantic externalism is not entirely barred to us if we turn to another meaning of experience, one that is linked to our practices — what Putnam has called a "pragmatic" or "internal" realism. It is then a matter of pointing out that the stabilization of reference does not take place independently of certain practices of justification, including at the level of perception, and that it is in this way that the idea of things with which we interact independently of the descriptions we give of them is maintained(19). Now, even if such a path will not bear on perception *stricto sensu* in the case of mathematics, it can at least be based on the set of the non-verbal practices that feed it. This is the path that Reviel Netz proposed to follow in order to overcome the fact that Greek mathematicians were, as we have already underlined, in a state of permanent disagreement as to the nature of the objects and the methods they were using:

(19) Putnam's argument, then, is that just because we only access "reality" through conceptual frameworks does not mean that we cannot accommodate, *from within such and such a framework*, the idea of entities independent of our descriptions.

> what unites a scientific community need not be a set of *beliefs*. Shared beliefs are much less common than shared practices. This will tend to be the case in general, because shared beliefs require shared practices, but not vice versa. And this must be the case in cultural settings such as the Greek, where polemic is the rule, and consensus is the exception. Whatever is an object of belief, whatever is verbalisable, will become visible to the practitioners. What you believe, you will sooner or later discuss; and what you discuss, especially in a cultural setting similar to the Greek, you will sooner or later debate. But the real undebated, and in a sense undebatable, aspect of any scientific enterprise is its non-verbal practices[(20)].

This kind of strategy is at the heart of the program of a philosophy of mathematical practice and reproduces, with a certain delay, a "practical turn" which had already imposed itself, for the same reasons, in the study of experimental sciences. But it should be noted that, from a philosophical point of view, this solution will be of little value if it is not able to specify how one can delimit a "practice". If the analysis of the practice leads to saying that Rutherford and Bohr do not share the same practice, the problem of knowing how they could relate to the same objects will remain unresolved. If, on the other hand, we hold that they share the same practice, the problem will arise of knowing where the boundaries of such a "practice" pass if it encompasses incompatible views[(21)].

The problem is that "practice" here is a category introduced by the observer for the needs of the study and which, for this reason, has all the appearance of a pure and simple convention. One will thus find a number of historical studies showing the divergence of practices between actors relating to the "same" objects (in Putnam's sense). This will include all the cases already mentioned where conceptual disagreement is explained by a difference in practical

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[(20)] R. Netz, *The shaping of deduction in Greek mathematics*, Cambridge, CUP, 1999, p. 2.

[(21)] This difficulty is underlined by Ferreiros when he studies Kitcher's proposal that a "practice" goes with a "language" seen as a conceptual framework (P. Kitcher, *The Nature of Mathematical Knowledge*, Oxford, OUP, 1983). Thus Kitcher, because he includes Newton and Leibniz in the same practice (that of the nascent differential calculus), must attribute to them a common "conceptual framework," which seems questionable, to say the least (see J. Ferreiros, *Mathematical knowledge and the interplay of practices*, Princeton University Press, 2015, p. 35).

context. To return to the example mentioned above, it certainly makes sense to say that Euclid and Archimedes share the same practice (the one that Netz undertook to describe), but it is no less true to say, as Leibniz maintained, that there are here two great practices in geometry: one that, via Apollonius, leads to the Cartesian geometry of curves, and the other that leads to infinitesimal geometry in its various forms. Moreover, we can easily find different "practices" in the same author: thus, the reasoning by exhaustion that supports Archimedes' "infinitesimal" practice can be found on occasion in Euclid himself[(22)].

But "pragmatic realism" is more specific here than a mere incantatory reference to "practice". The point Putnam was highlighting was that the practices in question are characterized by forms of *justification*. If we take the example of ancient Greek geometry, the question is not so much to describe a certain way of reasoning with objects as to isolate a particular type of *justification*. This is what Ken Manders undertook to do in a famous study in which he succeeded in isolating a type of inference attached to diagrams (as opposed to that carried by the text) in the context of the plane geometry of Books I to IV of the *Elements*[(23)]. We are then in a position to explain both in what sense Euclid and Archimedes belong to the same practice — but also why the reasoning by exhaustion that plays a central role in Archimedean geometry can open up a *different* way of relating to objects. In both cases, the important thing is not the delimitation of a "practice", but our ability to identify stable types of inference.

The temptation is great then to say that we can completely dispense with semantic considerations and characterize objects only by their roles in inferences. This is the path that Manders clearly follows in relation to diagrams[(24)]. But this poses two problems that will prevent us from closing our initial questioning too quickly. On the one hand, it is not at all obvious that we can do without descriptions to *control* inferences, and in particular to block the way to certain illegitimate inferences. In this case, the practice of definitions/descriptions interacts with inferential practices in a

[(22)] The idea that mathematicians work at the crossroads of multiple "practices" is a point that Ferreiros insists on (*op. cit.*).

[(23)] K. Manders, "The Euclidean diagram", in P. Mancosu (ed.), *op. cit.*, pp. 80–133.

[(24)] See Manders, *art. cit.*, section 4.1.2 and Ferreiros, *op. cit.*, p. 9.

complementary way to ensure a form of stabilization[25]. On the other hand, confining objects to their inferential role only makes the problem of transtheoretical identity all the more acute, i.e. the fact that certain objects, say the Euclidean circle, can be reinterpreted in *other* inferential practices (starting, in the Euclidean case, with Archimedes', but just as well with Cartesian algebraic geometry, complex analysis or modern algebraic topology). If the identity of objects is limited to their inferential role in a given practice, such phenomena of reinterpretation will simply not make sense. Finally, we should note that the stabilization by the "material" practice of the various symbolic systems which Manders highlights will be of no help in cases where these systems are different (typically in the case of different notational systems). And this was precisely the case in the two examples we came across (Stevin/Euclid and Cayley/Dedekind).

§ 5. — By way of conclusion: towards a study of the modes of referential stabilization.

I hope to have indicated in the above that the general problem of the stabilization of reference would benefit from being posed within the framework of mathematics — if only because it presents specific difficulties which a general theory of knowledge should be able to account for. It seems to me that the most promising way out of the difficulties raised is to recognize that there can be several modalities of stabilization. From this point of view, Leibniz's suggestions seem particularly interesting, both because they rescue a form of scientific realism in a general context of epistemic opacity (for Leibniz, as we have seen, the vast majority of our knowledge is "blind"), but also because they propose a pluralist approach to the problem of fixing references. They thus open up a research program for the philosophy of mathematical practice, the main lines of which I would like to sketch in order to conclude this paper by returning to the difficulties raised.

So let's go back to the example of Euclid and Stevin. As I have pointed out, we will not be able to base a possible trans-theoretical

(25) For examples, I refer to D. Rabouin, "Proclus' conception of geometric space and its actuality", in V. De Risi (ed.), *Mathematizing Space. The Objects of Geometry from Antiquity to the Early Modern Period* (Basel: Birkhäuser, 2015), pp. 105–142.

identity in this case either on a definition or on a common symbolic practice. If we associate "conceptual analysis" with the first option and a certain relation to "experience" with the second (I will come back to this point shortly), two of the Leibnizian strategies are thus immediately barred. But what about the other way, based on a "genetic" characterization of objects? Here the divergence is greatly reduced. While Stevin explicitly departs from Euclid on the definition of "number", he does agree with him on the fact that a number is characterized by certain operations and, in particular, that it makes sense to look for the greatest common divisor of numbers, to study their decomposition into prime factors, etc. These constructions are at the heart of the extension he proposes in order to generalize the concept of number to what he calls "algebraic numbers" (in a different meaning from the one we give to this term today) and their "arithmetic" (which we would call an arithmetic of polynomials). More interestingly perhaps, the "genetic" approach allows us to isolate a practice over the long term, from the "arithmetizing" tradition of Arabic algebra to the contemporary notion of a polynomial ring (in this case a "Euclidean" ring). The key point is that the disagreement on the descriptions is resolved here in an agreement on another form of characterization of objects *by the way they are generated and manipulated*. This allows us to argue that in one sense our two authors are talking about the same object and in another sense they are not — and yet this relativity never lapses into relativism precisely because we are able to *specify* the modes of justification (construction vs. definition) that allow for such judgments — this is the basis of "internal" realism, as Putnam defines it.

Let us take another example. It is known that Cantor and Dedekind, while sharing many of the conceptions that gave shape to the emerging set theory, disagreed about the nature of the continuum. In *Grundlagen einer allgemeinen Mannigfaltigkeitslehre* (1883), Cantor criticized Dedekind for having characterized continuity by a property that applies to any perfect set. He then gives the well-known example of a set that satisfies this criterion, but that we would like to call "discontinuous" (the famous "triadic" set). This set is continuous in the sense of cardinality (it has the "power of the continuum"), it is also perfect, in the sense that it coincides with the set of its accumulation points, but all its points are nevertheless disconnected from each other (its connected components are reduced to singletons). In the latter sense, we would therefore like to call it "discontinuous" and not "continuous". We are here in a case where the actors are situated in a homogeneous symbolic practice (let us say, for the sake of brevity, the nascent "set theoretic" practice) and agree on the operations to be carried out with their objects. But they clearly separate themselves at a purely conceptual/descriptive level. It is the conceptual analysis, the first path traced by Leibniz, which allows then the reference to be fixed by separating the different meanings of the term "continuous". Thus, after Cantor, we can fix the reference of the term "continuous" not in a single object or a single property, but by distinguishing *different meanings* attached in the past to the same word "continuous" (and which we may designate by other terms today: completeness, connectedness). The fact that we are not necessarily dealing with the same entities under the same terms in earlier periods is then no longer particularly troublesome since we are able to distinguish meanings that were wrongly mobilized in an equivalent way. It is a simple (and relatively trivial) case of equivocity: what the ancient authors designated with a single term corresponds in fact to different, but well delimited meanings(26).

The last point I would like to emphasize is the question of recourse to "experience". As I have indicated, it does not seem to be possible to rely on experience in mathematics if one has to accept *a posteriori* modes of justification (which does not mean that there is no "experimental" dimension in mathematics). From this point of view, the third Leibnizian strategy seems forbidden. But Leibniz himself stressed several times that mathematics nevertheless rested on a singular regime of experience, that of the symbolic systems we work with(27). We have seen, moreover, that this avenue has been explored in a very fruitful way by authors such as Netz and Manders concerning Euclidean diagrams (or notations). That these systems have a truly "material" component is a point which Manders particularly underlines in his study of Euclidean diagrams. Yet the anthropologist Ed Hutchins has rightly pointed

(26)This phenomenon has been excellently analyzed by Kitcher (*art. cit.*), in response to Putnam's externalism, to argue that descriptions can fix the reference in certain situations (at least if we allow for the possibility of the coexistence of several descriptions that appear to us to be retrospectively incompatible in one and the same actor). This is what he called a "reference potential".

(27)"It is therefore necessary to notice that the proofs or experiences that one makes in mathematics to guarantee oneself from a false reasoning (...) are not made on the thing itself, but on the characters that we have substituted in the place of the thing" (A VI, 4, 5). The same text qualifies this reasoning explicitly as *a posteriori* (A VI, 4, 4).

out that the study of human reasoning reveals two major strategies for stabilizing our reasoning: one that relies on meaning (this was the case for the first two paths proposed by Leibniz), and the other on experience, and more precisely on what he called the "material anchoring" of reasoning[(28)]. In this case, we stabilize the reference by delegating part of our inferences to material devices that reason in a way "for us". This idea has also been put forward by some logicians as a way of accounting for "surrogative reasoning", sometimes in explicit reference to Leibniz, for whom it is believed — quite incorrectly — that this is the only "blind" mode of knowledge[(29)]. As Shimojima has argued, this makes it possible to characterize "diagrammatic" reasoning as ultimately relying on the interaction with material objects whose structures we exploit in order to make our inferences[(30)] — a description that is remarkably consistent with the analyses of both Netz and Manders. This is not the place to go into the details of this fascinating research, but I wanted to mention it in order to indicate both the fruitfulness of the Leibnizian diagnosis and the way in which it could be used as a basis for certain adapted forms of semantic externalism compatible with mathematical knowledge. It seems to me that there are all the necessary tools here to take up the challenge launched by Hacking for philosophy in our times.

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(28) E. Hutchins, "Material anchors for conceptual blends", *Journal of Pragmatics*, 37, 2005, pp. 1555–1577.

(29) C. Swoyer, "Structural Representation and Surrogative Reasoning", *Synthese*, Vol. 87, No. 3, 1991, p. 449-508.

(30) A. Shimojima, "Reasoning with diagrams and geometrical constraints", *Logic, Language and Computation*, 1, 1996, pp. 527–540.