1

On Knowledge and Practices

A Manifesto

Although the idea of emphasizing them is relatively new, and there is still some disunity concerning how to focus our analysis, mathematical practices are in the agenda of every practicing philosopher of mathematics today. Mathematical knowledge, on the other hand, has always figured prominently among the mysteries of philosophy.[1] Can we shed light on the latter by paying attention to the former? My answer is yes. I believe the time is ripe for an ambitious research project that targets mathematical knowledge in a novel way, operating from a practice-oriented standpoint.

Let us begin by placing this kind of enterprise within the context of the philosophy of mathematics. During the twentieth century, we have seen several different broad currents in this field, which, simplifying a great deal, can be reduced to three main types: *foundational* approaches (logicism, intuitionism, formalism, finitism, and predicativism), *analytic* approaches (focused on questions of ontology and epistemology), and the so-called "*maverick*" approaches (to use Kitcher's colorful terminology), which have typically been anti-foundational and focused on history, methodology, and patterns of change. Mixed approaches have, of course, been present throughout the century, although one can say that they remained relatively uninfluential; early examples are the work of Jean Cavaillès in France during the late 1930s and that of Paul Bernays in Germany and Switzerland from the 1930s on. But in the 1980s and

[1] A mystery that some twentieth-century philosophers (Wittgenstein prominent among them) tried to dispel like a simple fog, but quite unconvincingly. For reasons why mathematics is not a game of tautologies, nor a mere calculus of symbol transformations, see especially Chapters 4 and 6.

1990s the situation—at least judged from an Anglo-American perspective—seemed to be one of confrontation between the anti-foundational maverick camp and the system-oriented camp.

It seems to be the case that a new generation of philosophers of mathematics has arisen whose work is superseding those distinctions. They follow upon the footsteps of Cavaillès, Bernays, Manders, and others. Examples of this new phenomenon are provided in some recent anthologies, such as Mancosu (2008), Ferreirós and Gray (2006), and van Kerkhove, de Vuyst, and van Bendegem (2010). These philosophers engage in an analysis of mathematical practices that incorporates key concerns of the "mavericks," without adopting their anti-foundational, anti-logical orientation. They are no longer obsessed with all-encompassing formal systems (e.g., axiomatic set theory) and the associated metalogical results, directing their attention instead to different branches and forms of mathematics—geometry ancient and modern; different ways of practicing analysis, algebra, topology, and so on. But thereby they do not imply—not at all, in my case—that there's nothing to learn about mathematics and its methodology from the crisp results of foundational studies. They keep considering the traditional questions of ontology and epistemology, but within a broader palette of issues concerning the evaluation of mathematical results (see the different aspects treated in Mancosu 2008) and the place of mathematics within human knowledge—one of my central concerns here.

All of that is meant when I say that I aim at providing a novel analysis of mathematical knowledge from a practice-oriented standpoint.

Notice that the new orientation in the philosophy of mathematics is highly interdisciplinary. Some authors emphasize knowledge of mathematics itself and logic, coupled with careful scrutiny of epistemological issues; some put an emphasis on combining philosophical issues with historical insight; some others stress the role of cognitive science (Giaquinto 2007) or sociological approaches (van Bendegem and van Kerkhove 2007); and the list goes on, with mathematics education, anthropology, biology, etcetera. My own approach, as will become clear, has a strong interdisciplinary bent. But one has to be quite clear and careful about the ways in which the different disciplines could or should contribute to the enterprise. Instead of trying to provide a principled discussion at this point, we shall clarify the matter as we go along. However, let me give an example of what I mean by "careful": it is highly relevant to establish contact with cognitive science and with the biological underpinnings of human knowledge, but I believe the time is not ripe for simply taking some 'established' theories or models from cognitive science and "applying" them to mathematics. While paying attention to what goes on in cognitive science and neuroscience, and aiming at convergence with that kind of research, a philosopher of mathematics can and perhaps should remain independent from the concrete current theories in those fields (see Chapter 3).

To briefly describe the crucial traits of the approach I shall put forward, I can say that this is a cognitive, pragmatist, historical approach:

- *agent-based* and cognitive, for it emphasizes a view of mathematics as knowledge produced by human agents, on the basis of their biological and cognitive abilities, the latter being mediated by culture;
- *pragmatist* or practice-oriented, as it places emphasis on the practical roots of math, i.e., its roots in everyday practices, technical practices, mathematical practices themselves, and scientific practices;
- *historical* and hypothetical, because it emphasizes the need to analyze math's historical development, and to accept the presence of hypothetical elements in advanced math.[2]

Our perspective on mathematics will thus stress the provenance of mathematical knowledge from particular kinds of interplays between cognitive resources and cultural practices, with agents at the center, making such interactions and the development of new practices possible.

There is an aspect in which such an approach goes against well-established habits of philosophers of mathematics. It was customary to focus one's attention on a single mathematical theory assumed to be sufficiently broad to embrace all of current mathematics; the chosen system was commonly a form of axiomatic set theory based on classical logic.[3]

[2] On this topic, see Chapter 6 and also the introduction to Ferreirós and Gray (2006).

[3] However, other systems have been considered in foundational work, e.g., constructivist systems, and in recent decades there has been much discussion of category theory as an alternative foundation (see, e.g., issues of *Philosophia Mathematica* for 2004 and 2005).

This tradition emerged as a result of foundational developments in the early twentieth century, but its roots lie deeper, in the centuries-old vision of Math as an Ideal Theory, given in some Platonic realm (often conceived as God's mind), fixed and static, offering a unified foundation which one may bring to light by excavating the imperfect glimpses of the "true" math that our current theories provide.[4] Of course, this old tendency has been heavily criticized, both by the "mavericks" and by more recent proponents of a philosophy of mathematical practice. One of the reasons for such a critique is that set-theoretic reductionism blinded philosophers of mathematics to important phenomena, such as the hybridization of branches of mathematics, progress through the expansion of mathematical domains, or the specificity of methods characteristic of this or that branch of math. As a general rule, it is not only salutary, but necessary, to avoid the excessive systematicity and reductionism that was characteristic of much philosophy of science and epistemology. But there is another reason to go against that old habit, one that is crucial to my whole approach.

It is a key thesis of this work that several different levels of knowledge and practice are coexistent, and that their links and interplay are crucial to mathematical knowledge. What I mean will be spelled out in the next chapter, but for a first approximation consider Philip Kitcher's idea that given historical periods in mathematics are each under the spell of what he called a "mathematical practice," a system or aggregate of linguistic statements, methods, questions, and results (Kitcher 1984) that guide mathematical work. Intentionally or not, Kitcher's very interesting work promoted a rather Kuhnian image of a "normal period" in which a discipline, math in our case, is governed by a single paradigm or disciplinary matrix;[5] it also seems to have been influenced by the dogma of systematicity that we have discussed. Contrary to this, I maintain that in any given historical period one can find more than one framework for mathematical practice. Hence different levels of

[4] On this topic and the Quinean dogma of systematicity, see Ferreirós (2005).

[5] The reference is to Kuhn's classic (1970) and especially the 1969 Epilogue. Kitcher's analysis of a "mathematical practice" is very different from Kuhn's notion of a "disciplinary matrix"; yet I mean to say that there seemed to be a common underlying assumption. A more detailed presentation and critique of Kitcher's idea can be found below, in Chapter 2.

knowledge and practice coexist historically during the same period, and even within the same agent, an individual mathematician.

The Kuhnian element in such an analysis is very misguided, as it actually makes it impossible to address key epistemological issues. Analysis of a certain mathematical framework requires us at the very least, in all cases that I can think of, to consider its connections with another level of knowledge and practice. Different practices are linked in a systematic fashion, which is learnable and reproducible, and their interplay is crucial for math as the peculiar kind of knowledge it is: crucial, that is, not only to the way individuals learn mathematics and to mathematical understanding, but also to the lack of arbitrariness that is typically found in mathematical changes, to the emergence of new practices, and to the objectivity of new results. All of these ideas will be developed and discussed in the body of the present work.

As already announced, I shall argue that our knowledge of mathematics cannot be understood without emphasizing the practical roots of math, including its roots in scientific practices and technical practices.[6] The general scheme of my non-reductionist way of analyzing the *interplay* of practices will do a crucial job in making it possible to substantiate this claim. The idea is akin to that of the pragmatist tradition, in particular to the elucidation of "meaning" in terms of "use" (although I must resist the *reduction* of "meaning" to "use," see Chapter 4). Presented in a simple form, the claim is that a mathematician confronted with an axiomatic system for, say, the real numbers, can only obtain *knowledge* of it by (re)establishing its links with other mathematical practices (e.g., and importantly in this case, geometry), and ultimately with the basic technical practices of measuring and counting. There is a sense of understanding, to be explained below, in which one can claim that mathematical understanding is only gained by this kind of process, i.e., by linking back to more elementary practices and to the cognitive roots. This is not to be conceived as a process of reduction, but rather as a form of substantiation.

In the course of history, scientific practices have also been poles of reference for mathematical practices. The study of geometry was not

[6] By *technical* practices I mean something specific (section 2.4), examples being the practices of counting, measuring, and drawing geometrical shapes.

stimulated only by basic technical needs of drawing geometrical figures (such as for architectural construction); it was also, and rather constantly throughout history, motivated by the use of geometrical models in astronomy. Likewise, if we take the paradigmatic case of the function concept, we have an instance in which a central mathematical idea has emerged in response to needs arising in the context of scientific modeling of natural processes. This crucial case can be turned, I believe, into an argument for two points: that mathematical theories and practices are not totally independent or autonomous, and that they can be more fully understood when they are regarded as part and parcel of the theoretical work mobilized by the enterprise of science.

Our strategy will be to understand mathematical results and theories as *knowledge*, contiguous with (and not essentially different from) other forms of knowledge; but we shall insist on the specificity of mathematics, in contrast with holistic strategies such as those of Quine (1951) and Kitcher (1984). What kind of knowledge does mathematics provide? What are its relations with other forms of knowledge? These are difficult questions that lie at the heart of the theory of knowledge, and all major epistemological approaches have attempted to solve them. It suffices to mention Plato and his views about the links between mathematics and the realm of Ideas, mathematical discovery, and reminiscence; Leibniz and his views about mathematical truths being analytic "verités de raison," so close to twentieth-century logical positivism; Mill and his ideas about mathematical truths being obtained inductively from an extraordinarily broad basis of evidence; or Kant and his views about mathematical truths being synthetic a priori, but totally irrelevant to the world of "things in themselves," relevant only to the phenomenal world.

From the time of the ancient Greeks, with their definition of knowledge as "justified true belief,"[7] to the twentieth century under the influence of denotational semantics, it has been common to consider mathematics as a body of *truths* (otherwise, it is felt, there couldn't be

[7] My beliefs can only be called knowledge when they happen to be true (normally, of states of affairs in the world) and I can provide justification for them. If I believe there is a king of France, this is false and hence not knowledge; if I believe there is a king of Spain, and I can provide justification, then I know it.

knowledge of them). Math has been regarded as the discipline that provides justifications of the strongest kind, namely deductive proofs. But what are mathematical truths true of? What do they refer to? Some of the main types of answers are given by the authors just mentioned. Mathematical truths have been taken to be truths about objects of a peculiar non-physical kind (platonism), truths about or deriving from concepts or linguistic conventions (analytic truths), truths about the empirical world as the mind or subject knows it perceptively (empiricism), or truths about the forms of pure intuition (Kantianism).[8] Yet all of these answers run into difficulties—some of which will be mentioned later—and in my view none of them is satisfactory.

Though I cannot attempt to provide here anything close to a careful analysis of this broad topic, I feel it will be helpful to offer readers some remarks that may help them locate my proposals within a general scheme. But it should be taken into account that my argument in the bulk of the book does not depend on the considerations offered here. It is in this spirit that the following paragraphs ought to be read; the numbering is intended to underscore their *non*-systematic character; I do not in the least aim at completeness.

1. Common to all those attempts at answering the question of referents is the assumption that the semantics for mathematics must be similar to the semantics of common language. When I say "Venus is the morning star," the word "Venus" is the proper name of an object, a planet, and the word "star" is the common name for objects in a class to which the sun and other brilliant celestial masses belong. Medieval logicians knew already that there are many terms of common language to which this kind of semantics does not apply, namely what they called "syncategorematic terms" (particles like "all," "and," and "is"). Yet, the idea is that whenever we find proper names in mathematics (1, *e*, the Riemann theta function), they should be taken to refer in the same way

[8] The empiricist view, or a view more or less akin to it, admits rather different versions. When mathematics was regarded as the "science of magnitudes," many understood this to mean that it referred to physical magnitudes (but notice that a Kantian interpretation was also possible). In a strong move of refinement, Gauss defended mathematics as the "science of relations," and this position can be given a (broadly speaking) empiricist reading.

as "Venus" does; and whenever we find common names (real number, analytic function, closed set), they should be taken to refer like "star" does. If we resist this idea, then we shall be providing a semantics for math and science that will deviate from our semantic conceptions in common life. Perhaps the reader will not be surprised if I say that this would be a salutary move.

Without entering into complexities at this point, we can suggest how the reorientation would proceed. The common ingredient in the previous attempts was to adopt a referential semantics as the basis for a correspondence theory of truth: there had to be a specific domain of objects, which provide the reference for proper and common names (or constant terms, variables, predicates, and relations). To avoid this, one must avoid the correspondence theory, adopting some kind of deflationary theory of truth, an account of how we can arrive at truths that does not invoke a domain of previously given objects. This would actually be the starting point for another way of presenting the project of this book. I shall try to provide a strong account of how we arrive at—and share—mathematical truths, without invoking mathematical objects in any strong ontological sense. Of course, the word "deflationary" by itself does not make any miracle; what is required is a detailed account, a robust theoretical explanation of the basis on which mathematical truths can be found and shared.

2. My use of the word 'truth' at this point must be relativized by implicit or explicit reference to a mathematical theory. This agrees with the practice of most mathematicians; hence it should not be perceived as a shortcoming. That use is such that one can say, for instance, that Theorem 29 of the *Elements*, Book I ("the angles in a triangle add up to two right angles," 180°), is a truth of Euclidean geometry and the theorem that "the angle sum in a triangle is less than 180°" is a truth of the geometry of Bolyai-Lobatchevskii—while everybody knows that they cannot both be true of physical space, since they contradict each other. Similarly, the Well-ordering theorem is a truth of the Zermelo-Fraenkel set theory ZFC.

For that reason, it is actually not very enlightening to speak of "mathematical truths," and in this book I shall normally not employ that phrase. What I aim to provide, more than anything else, is an account of our *knowledge* of mathematical results; and the core of the matter is accounting for the *objectivity* of mathematical results. The motto here, following Kreisel and Putnam, could be "objectivity without objects".[9] The phenomenon that we aim to explain is the following: mathematical results force themselves upon us in such a way that we cannot help accepting them (except perhaps by some strong revision of the basic axioms or the logical principles employed).[10] Let me give an example that Einstein praised for the beauty of its statement and its method of proof, namely, Cantor's theorem of the nondenumerability of the set of real numbers, proved by the diagonal method. This is a mathematical truth, i.e., an objective result, and our task is to explain why and how this is so. My answer will be found in Chapter 9. (A follower of Brouwer can, of course, reject Cantor's theorem, because he or she rejects the basic axioms of modern mathematics, and some of its logical principles. The simplest way of avoiding Cantor's result is by rejecting the concept of an infinite set altogether—but once we accept the sets of natural and real numbers, we are forced to accept the result; see Chapter 9.)

3. Since physics and other sciences (natural and social) employ mathematics, the account we give of mathematical truth—or the objectivity of mathematical results—will affect accounts of scientific truth. The kind of analysis that I shall propose here calls for a critical understanding of the mathematical aspects of scientific models of natural phenomena. From the standpoint we shall adopt, it makes very little sense to assume that the mathematical basis of an empirically adequate model should automatically be interpreted in a realistic fashion. Notice that this is often done, and sometimes even proposed explicitly, e.g., in the context of interpreting the conception of space-time that underlies relativistic models.

[9] The word "object" should be understood here in the metaphysical sense. There is no problem in speaking of objects within the context of a given mathematical framework, on the understanding that their mathematical existence (modulo a given framework) is not metaphysical existence. See Chapter 6.

[10] The phraseology is Gödel's (1947); he wrote that the axioms of set theory "force themselves upon us as being true" (this is found in the 'Supplement to the second edition', 1964, p. 271).

However, the approach I shall propose here cannot simply be called non-realist, fictionalist, or idealist. The question of realism cannot be posed in a "flat" way, so to speak, as if one could give a yes-or-no answer that applies uniformly to all mathematical theories and all mathematical practices. (Similarly, there is more to say about the relation between "math" and "truth" than found in the preceding paragraphs.) This is a consequence of the ideas about mathematical knowledge that I shall be presenting: in particular the ideas concerning the interplay of practices presented in Chapter 2 and developed subsequently, my views on arithmetic certainty (Chapter 7), and the hypothetical conception of advanced math (Chapter 6). Once again, it seems to me that the over-systematic, reductionistic attitudes that were prevalent in the past have impeded a nuanced approach to questions like this, which require it. We shall return to this issue in the last chapters.

4. Recently, Giaquinto (2007, 5) called attention to the fact that twentieth-century philosophy of mathematics has abandoned the traditional topic of how knowledge is gained by the individual. This was a result of the concentration on the problem of rigor that was characteristic of foundational studies, plus the focus on theories and their justification as the sole question for philosophy. It was also aided by a psychologistic-subjectivistic understanding of the question of how knowledge is gained by individuals, which in my view has been superseded by more recent conceptions in cognitive science. Those philosophical and foundational orientations overshadowed the traditional concern for the epistemology of discovery by individuals that we find in Plato, Kant, and Mill. Giaquinto has proposed reviving this concern, which is obviously of great interest in connection with cognitive science, and also of practical interest within the field of math education. As has been said before, the approach I am proposing is centered on the agents, hence cognitive, and thus resonates with Giaquinto's proposal.

The subjectivistic understanding of individual knowledge, which I have mentioned as superseded, was a close ally of another influential idea of the twentieth century: the call for an epistemology without a subject. The shortcomings of traditional conceptions in epistemology—based on an attempt to reconstruct human knowledge as if it were obtained by a single perceiving mind, solely on the basis of its observations of phenomena—were more than clear. It was probably because of those subjectivistic connotations that a need was felt to free epistemology within the general scheme of contrapositions between subjective/objective and descriptive/normative. In France, the call was to replace the "*philosophie du sujet*"; and Cavaillès proposed a "*philosophie du concept*" that was later turned into philosophical structuralism. In England, Popper raised the call for an epistemology without a subject. In my opinion, however, these were wrong solutions based on a correct perception of symptomatology; the right surgery should have proceeded differently. Nothing is gained by trying to study epistemology without a subject; in fact, without a community of subjects. But the notion of a "subject" has to be seriously reconceived.

5. The general direction of "pragmatists" (interpreting this label in a broad sense that may include the later Wittgenstein) was, in my opinion, much better oriented than the moves of Popper or Cavaillès. To be brief, we should employ the notion of an *agent*, rather than a subject, and specifically of a community of agents. As Dewey liked to emphasize, the tradition in epistemology has been to rely on a "spectator theory" of knowledge as merely based on perception (the ego portrayed as a passive spectator sitting in the theater, its perceptions as what happens in a scene, out of its reach and control). I dare say that the *oblivion of action* was the worst defect of traditional epistemology, its main shortcoming that, in my view, has made impossible a well-grounded account of human knowledge.[11] Even perception, which modern philosophers from Descartes and Locke onward understood as a passive reception of impressions, is in the light of neurobiology and cognitive science a complex system that results from the interplay of sensory input and motor output (not anything like a primitive faculty of the mind, as philosophers tended to think). Input and output, sensation and action—without this feedback, one cannot even make sense of perception, let alone the further complexities of knowledge production.

[11] This was the clearest lesson that I learned from studying epistemology, back in my undergraduate days, when I knew almost nothing about pragmatism—but was aware of the work of Piaget and several other scientists in biology and psychology.

A brief excursion into etymology may help hammer the point. The word "subject" comes from Latin *subjicere* (formed from *sub,* "under" + *jacere,* "to throw," also related to *subjectare*), and has a strong passive meaning, which accords very well with the traditional oblivion of action. Originally, it meant a person is subject to another (dependent on, submitted to, another, like a king's subjects in the ancien régime);[12] it seems that only in the seventeenth century did it come to be used for the thinking subject. Possibly this was meant to suggest the mind or soul that "lay beneath" the world as perceived. By contrast, the word "agent" comes from Latin *agere,* "to act," and is related to *actio,* "action"; and if that were not enough, it is formed from *agens,* the active participle of *agere.* When we stop talking about subjects in favor of agents, we should be constantly reminded of the role of action in all facets of human life. *Im Anfang war die Tat*: in the beginning was the deed (Goethe).

6. One last clarification concerning the general topic of knowledge is in order. Parallel to the centuries-old debate about the role of language in thinking (recall the Sapir-Whorf hypothesis, heavily criticized by some cognitive scientists, or the debate between Chomsky and Piaget) is the corresponding debate about knowledge. What is the role of linguistic knowledge, versus implicit or tacit forms of knowledge? In recent decades, the debate has found new forms of expression within the field of AI, in the contrast between traditional systems based on symbolic processing and neural network systems; the former are enormously successful in tasks such as calculation and chess, but reveal well-known shortcomings in pattern recognition, where neural networks have presented significant advantages.[13] In the twentieth century, both analytic and hermeneutic philosophers made a strong bet on language with the celebrated linguistic turn, but this far from ended the debate, and there

[12] It could also mean a subject matter, what lies beneath (subjicere) a certain theme.

[13] Notice that pattern recognition tasks bear relations with some important topics of twentieth-century philosophy, in particular with Wittgenstein's reflections on language (family resemblance) and Kuhn's views on the cognitive organization of scientific knowledge (his insistence on the role of exemplars).

has recently been a re-turn.[14] Meanwhile, Polanyi and Kuhn and others were arguing for the importance of tacit knowledge in scientific practices, which agrees with the views of cognitive linguists such as Lakoff. Although I do not intend to contribute to the debate here, I feel the need to clarify my position.

In my opinion, the roots of knowledge and thought lie deeper than language; thus I tend to take sides with Piaget, Kuhn, and Lakoff. Steven Pinker [1994], in discussing how people think, argues that language is not adequate to account for all of our thoughts, and concludes that, if there can be two thoughts corresponding to one word [indeed, one sentence], thoughts can't be words.[15] This is obviously not to say that language and symbolic systems do not play a crucial role in human knowledge generally, and mathematics in particular. I believe they do, and I am even tempted to think that the use of written symbols (ciphers, diagrams, formulas) is a defining trait of mathematical practices. The idea that language is the very source of thought appears to me quite obviously wrong, but this still leaves room for the more nuanced view that language does contribute to human thinking abilities in a crucial way—and, by the way, we should distinguish between the modalities of oral and written language. In fact, I do hope that the study of mathematical cognition can play the role of a privileged arena for sorting out this vexing question. But this is a task for future studies.

7. The idea that there is no methodological difference between math and science serves, no doubt, the purpose of making things simpler for the philosopher. But it lacks power of conviction; in fact it makes things too simple (see Chapter 6). On the other hand, the opposite move of making math and natural science essentially different, most often carried through by means of the distinction between formal and empirical sciences, is again simplistic and misleading. Carnap, e.g., considered mathematics as a formal science based on linguistic convention, in contrast with the empirical sciences, which were supposed to be based

[14] This includes authors such as Van Fraassen.
[15] He indicates problems of ambiguity, lack of logical explicitness, co-reference, deixis, and synonymy as examples.

on mere observation and induction.[16] This simple dichotomy has to be abandoned, but to conclude from this that there exists no difference in terms of methodology would be to throw the baby out with the bathwater. Although these are issues to which we shall be returning, let me advance two ideas at this point.

In an analysis of the natural sciences from a practice-oriented standpoint, it becomes necessary to distinguish between several kinds of practices: experimental practices, theoretical practices, practices of writing and communication, etcetera.[17] Several authors have made the point that all of these kinds of practices—and, most interestingly, experimental practices—can indeed be found within the field of mathematics (consider the role of empirical methods in number theory and many other branches of math). Conjectures and hypotheses, too, are no less present in this field than they are in the natural sciences. Nevertheless, it remains true that empirical methods in mathematics are of a different kind than those in the natural sciences: experiment and observation in the sciences depend on physical interactions with objects or phenomena under study, while mathematical experimentation is done only with symbols and diagrams (either on paper or on the computer). A way of clarifying this situation—and one that I favor—is by arguing that *mathematics is an essential part and parcel of the theoretical workings of science*: essential because it is crucial to the processes of concept-formation, model-formulation, and theory-formation, but also distinctive and peculiar, because here the theoretical work becomes autonomous. That is to say, the activity of mathematicians (at least since Archimedes) has typically involved work on problems emerging from the theoretical framework itself, from a concentration on conceptual and theoretical issues *independently* of their potential role as models for physical phenomena.

[16] As everyone knows, Quine devoted much of his work to criticizing this dichotomy, which in my view is untenable. See Quine (1951), Putnam (1965), and also Mancosu (2005).

[17] There is a large body of literature on these topics, as the philosophy of scientific practices has also flowered in recent times. I have contributed myself in joint work with J. Ordóñez and in Ferreirós (2009), where I emphasize the three kinds of practices mentioned above, in connection with Newton's optical work.

8. We have to resist the temptation of picturing math as *totally* independent from other activities. This has been common in recent times, partly due to the influence of the ideology of "pure mathematics" in the twentieth century, partly because of older philosophical trends represented by Leibniz and Kant, received and transformed by mathematicians like Gauss, Dedekind, Frege, Brouwer, and others. A typical example—quite representative because of the extreme form of his claims—is the French philosopher Jean Cavaillès, for whom mathematics is a fully autonomous kind of human activity, to the point that it can only be elucidated by doing mathematics; hence the philosophy of mathematics must grow "*a l'intérieur des mathématiques*."[18] Many works in the philosophy of mathematics follow this orientation, often without the least explicit reflection, which of course helps that trend to reproduce itself dogmatically.

Notice how the proposed approach, by emphasizing the interplay between math and other kinds of practice, has the effect of shifting the celebrated problem of the "applicability" of mathematics.[19] In fact, the terminology of "pure" versus "applied" mathematics is already a bias. From the perspective of my practice-oriented approach, the problem of "applicability" ceases to be posed as external to mathematical knowledge, and becomes internal to its analysis. This is a move toward the dissolution of the problem, since in some respects mathematical frameworks (all mathematical frameworks) are designed to be applicable, and so there is little to explain—that's nothing unreasonable. At the same time, however, articulating a novel application of mathematics can be very difficult, and obtaining from a mathematical model the results one might desire may be impossible—hence the uncooperativeness.

Critics of my views may perhaps argue that such apparent progress by dissolution is only attained at the very high cost of complicating our

[18] Cavaillès was reflecting the influence of neokantianism and phenomenology, but also that of the German mathematical tradition that he knew well from Göttingen in the 1920s and 1930s.

[19] Be it the "unreasonable effectiveness" of math (Wigner 1960) or its "unreasonable uncooperativeness" (Wilson 2000). On this topic, see Ferreirós (2013).

analysis of mathematics, and of science in general. I agree that I am complicating those analyses, but in my view this is no disadvantage. We should abandon previous models of the workings of science—which were simplistic to the point of being of little use in understanding real cases—in favor of more complex models that can help us explain and understand.