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The Advent of the Philosophy of Mathematical Practice

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1. Introduction

Both philosophy and mathematics are ancient academic disciplines whose presence in human affairs can be traced back to very early written remains. Eventually, mathematics itself became a subject of philosophical inquiry, and philosophy of mathematics came to be. It is precisely because of the early appearance of mathematics in the history of humankind that philosophy of mathematics is much older than the philosophy of other academic disciplines, like philosophy of biology, or even philosophy of physics. Not only that, but many of the ideas developed by Greek philosophers in philosophy of mathematics prevail to this day. The legacy of platonism, for example, is such that it remains one of the most popular metaphysical assumptions among not only practicing mathematicians, but also among other academics who make use of applied mathematics in their fields of knowledge and the general population.

However, a generation of philosophers originated in the second half of the 20th century and pioneered by Imre Lakatos and Philip Kitcher has noted that, in spite of the long track of philosophy of mathematics and the considerable number of school of thoughts in the discipline, the focus of philosophers of mathematics has been quite limited. The practical aspects of mathematics, that is, how mathematics is done as a whole, have been neglected for the most part. On the other hand, the philosophy of empirical sciences, even with a shorter history at its back, enjoys a broader range of topics among its repertoire. This is, in part, due to a number of particularities of mathematics as a field of knowledge, a lack of philosophical reflection on such mathematical idiosyncrasy, and a passive and limited in scope history of mathematics. In this essay, I shall provide an overview of the differences between the traditional philosophy of mathematics and the philosophy of mathematical practice. Moreover, I will explore the philosophical milestones that gave rise to the genesis of the latter, and discuss the role of the central pillars that currently keep it afloat.

2. The mainstream philosophy of mathematics

From its inception to this day, philosophy of mathematics has been characterized by an obvious and almost exclusive attention to the foundations of mathematics. Foundational studies, often dealing with questions on apriorism and logics or dressed up as ontological inquiries (for example, the nature of mathematical objects, and if they exist, how we can access them) have led to the creation of all major schools of thought in philosophy of mathematics: platonism, empiricism, monism, logicism, formalism and intuitionism, to name a few, all support a given metaphysical perspective on the most basic properties of mathematics. Platonism, for example, advocates that mathematics is real, ahistorical, perennial and transcends the mathematician. Hence, the platonist states that theorems are not invented or created, but discovered; it is implied that all mathematics is already somewhere in a realm of abstract ideas, waiting to be unveiled. Logicism dictates that all mathematics is nothing but, or is reducible to, logic, underscoring its *a priori* character. Empiricism, on the other hand, denies the *a priori* character of mathematics, that there is mathematical knowledge that is independent of experience, and claims instead that all mathematics is ultimately to be subjected to empirical testing, as well as other sciences.

Restricting philosophy of mathematics to the study of the basic foundations of mathematics had an unusual consequence: it became a place where to test large, classic philosophical views in metaphysics and epistemology. For example, if someone were to discuss whether a priori knowledge is attainable, or the virtues of realist versus constructivist ideas, the foundational work in philosophy of mathematics would be a good place to explore. This was noted by William Aspray and Philip Kitcher not very long ago: "Philosophy of mathematics appears to become a microcosm for the most general and central issues in philosophy—issues in epistemology, metaphysics, and philosophy of language—and the study of those parts of mathematics to which philosophers of mathematics most often attend (logic, set theory, arithmetic) seems designed to test the merits of large philosophical views about the existence of abstract entities or the tenability of a certain picture of human knowledge." (Aspray and Kitcher, 1988).

This happened at the cost of leaving out mathematics' own topics as a discipline. How mathematic knowledge grows, what makes some theories better than others to the eyes of mathematicians, how are informal arguments related to formal arguments, and what does it mean to explain and to prove in mathematics, are questions that were plainly disregarded. Philosophy of mathematics was, indeed, receiving a different treatment than that of philosophy of science.

As José Ferreirós and Jeremy Gray (Ferreirós and Gray, 2006) point out, an intuitive way to understand the aim of foundational studies in mathematics is the architectural metaphor. Mathematics and its progress were regarded as a building and its construction. For the building to be built, it is first necessary a solid ground to support the ground floor, which in turn, will sustain further floors. This metaphor is rescued from the 17th century analogies of sciences as buildings, and from the work of Hermann Weyl, who considered it was a good match for foundational debates in mathematics. Weyl, after being trained under the supervision of the formalist bannerman David Hilbert, became heavily influenced by Brouwer's intuitionism –more on this later- and complained that foundational work in philosophy of mathematics was aimed to demonstrate that mathematics was built on stone (identifiable foundations, whatever their nature), and not on sand, although he believed otherwise and called formalism a "fake wooden structure" (Weyl, 1994). This is not to say that this purpose was not shared with the philosophy of the natural sciences, which also strived to study the foundations of physics and other fields. However, philosophy of science has also been concerned about how the building is built, instead of focusing almost exclusively on the ground on which it is built. Even more, paying attention to this process, we can gain insights on the role of foundations in science, and on foundations themselves, which would paradoxically be disregarded with a plain focus on foundations. Hilbert, being a faithful advocate of formalism and of the idea that mathematics is ultimately sustained by solid foundations, was by no means naïve, or oblivious to the fact that this applies to the philosophy of mathematics too, and that a commitment to the actual historical development of the discipline is imperative. According to him, the process of building the edifice is not linear and the foundations are often revisited and adapted to whatever needs arise: "The edifice of science is not raised like a dwelling, in which the foundations are first firmly laid and only then one proceeds to construct and to enlarge the rooms. Science prefers to secure as soon as possible comfortable spaces to wander around and only subsequently, when signs appear here and there that the loose foundations are not able to sustain the expansion of the rooms, it sets about supporting and fortifying them. This is not a weakness, but rather the right and healthy path of development." (Hilbert, 1905).

Therefore, keeping an eye on how the building is erected on the ground seems to be a very sensible thing to do. Philosophical reflection should not be constrained to the foundations of knowledge. Philosophers of science quickly became aware of this, but philosophy of mathematics has historically lagged behind in this respect. It was not until the decades of the 70s and the 80s that a few philosophers, followed by the pioneering line of philosophical inquiry of Imre Lakatos, became interested in this process of building the edifice, the so called mathematical practice. David Corfield, a philosopher concerned with the mathematical practice, controversially asserted that this is, in fact, the only real philosophy of mathematics. In his book *Towards a Philosophy of Real Mathematics*, dedicated to support his stance but also heavily criticized for openly advocating anti-formalist views, he vindicates this still newborn line of enquiry in the philosophy of mathematics: "Continuing Lakatos' approach, researchers here believe that a philosophy of mathematics should concern itself with what leading mathematicians of their day have achieved, how their styles of reasoning evolve, how they justify the course along which they steer their programmes, what constitute obstacles to their programmes, how they come to view a domain as worthy of study and how their ideas shape and are shaped by the concerns of physicists and other scientists." (Corfield, 2003).

This shift of attention from traditional philosophy of mathematics to the philosophy of the mathematical practice was preceded by some key factors in the history and philosophy of mathematics, which will be explored next.

3. Are mathematics and science different to the eyes of philosophers? The dismissal of the analytic-synthetic distinction

I mentioned earlier that mathematics is an idiosyncratic field of knowledge, with a number of particularities that set it aside from science. One of these particularities is the broad influence of logical positivism and its conception of mathematics as a discipline comprised of analytic propositions, in contrast to science, which relies on synthetic propositions. In order to understand what this means, it is necessary to look back to Immanuel Kant's philosophy.

In the *Critique of Pure Reason* (Kant; translated and edited by Guyer and Wood, 1998), Kant notes a significant overlap between the analytic-synthetic distinction and the *a priori-a posteriori* distinction. Let's consider these distinctions.

Analytic propositions are those which can be dictated to be true or not by scrutinizing its internal meaning -they convey non-factual knowledge- while synthetic propositions refer to facts of the world, and therefore are true or not depending on how their meaning relates to the world –they convey factual knowledge. The truth value of the predicate of an analytic proposition depends on the subject of such proposition, while the truth value of the predicate of a synthetic proposition is not to be found within the subject of the proposition, but in information elsewhere. For example, the proposition "all cats are mammals" is analytic, since the predicate "being a mammal" is already contained in the subject "cat", since "being a mammal" is part of the definition of "cat". However, the proposition "all cats are black" is synthetic, since we have to look elsewhere to verify its truth.

On the other hand, *a priori* judgments are those which are not justified by relying upon experience –they convey experience-independent information-, while experience is required for the justification of *a posteriori* judgments. The examples above can be used here as well: since "being a mammal" is part of the definition of "cat", we do not need to check whether it is actually the case that all cats are mammals. However, it is necessary to check whether all cats are black to tell if it is actually the case.

At this point, we can already intuitively abstract how the two distinctions overlap: if the predicate of a proposition is already included within its subject (analytic), it is not necessary to rely on experience (*a priori*), and viceversa; if the predicate is not included within its subject (synthetic), we must resort to experience (*a posteriori*) to validate the claim, and viceversa. But, following Kant, the issue is not that simple, and the overlap, not complete. Propositions can be analytic or synthetic, and *a priori* or *a posteriori*, so that, in principle, there are 4 possible combinations:

-Analytic *a priori*

-Analytic *a posteriori*

-Synthetic *a priori*

-Synthetic *a posteriori*

We have already seen an example of analytic *a priori* (all cats are mammals) and *synthetic a posteriori* (all cats are black). Kant acknowledges that analytic *a posteriori* is contradictory, as he cannot think of any reason why experience would be needed if the answer is already within the proposition. He does, however, arrive to the conclusion that there are cases where the predicate of the subject doesn't follow from the subject of the proposition (synthetic) and still, experience is not required (*a priori*). The foremost example is metaphysics, whose propositions are not validated through experience, but they are not true by virtue of their subjects either. Kant, appealing to "necessities of thought", holds a long discussion on whether truth value can be ascribed to synthetic *a priori* at all. After all, truth is not a value that one would expect from metaphysical stances, at least not in the sense it is expected from scientific propositions or deductions in formal logic. Then, how are metaphysical stances, and by extension synthetic *a priori* propositions in general, justified at all?

Kant finds synthetic *a priori* propositions somewhere else, in a very popular domain whose veracity and justification is seldom called into question, and through it, legitimates both metaphysics and synthetic *a priori* knowledge. This domain is mathematics. According to him, statements like "2 + 3 = 5" are synthetic *a priori,* for the definition of "2 + 3" does not include the predicate "= 5", and experience is not needed to validate the claim. Kant considers the epistemic virtue of mathematical knowledge extends to metaphysics since they are essentially the same kind of knowledge: one that arises from necessities of thought. However, the main point here for us is that mathematics is consolidated as different to science in that it is *a priori*, without resorting to linguistic tautologies. Mathematics, being synthetic knowledge, renders factual knowledge, information about the world.

Later in history, even if logical positivists accepted mathematics as *a priori* knowledge, they considered that Kant's analogies between mathematics and metaphysics jeopardized its status, and by extension, the status of logic, as epistemic bastions. In addition, both mathematics and logic reached new peaks of sophistication in the 19th century, what led to a reformulation of Kant's considerations. The bulk of such reformulation was laid by Gottlob Frege, a mathematician, logician and philosopher from the formalist tradition, who expanded Kant's notion of "containment" (the predicate of a proposition being already contained with the proposition). Frege's extension of the Kantian analytic (MacFarlane, 2002) allowed logic and mathematics to fit within it, effectively 1) putting distance between mathematics and metaphysics and 2) undermining the legitimacy of synthetic *a priori* knowledge. While a discussion of Frege's logic would be too broad for the purposes of this dissertation, let us consider the following statement:

-If $X < Y$, and $Y < Z$, then $X < Z$

This proposition includes transitivity, a central logical relation in mathematical logic. One would intuitively agree that "$X < Z$" is already contained in the proposition even if the subjects do not explicitly contain it, yet Kant's notion of containment –and therefore, of analycity- does not contemplate transitiveness. Frege expands the Kantian analytic with several other logical relations, such as symmetry (if I am his sibling, then he is my sibling), antonymy (if I am not his sibling, then he is not my sibling), among others. Mathematics and the logic at its core became analytic knowledge, while science remained synthetic knowledge. That is, mathematics was consolidated as knowledge of meaning and relations, as opposed to science, which comprises factual knowledge, knowledge of the world that requires, in turn, observing the world.

Note that, for the logical positivist, the overlap between the analytic-synthetic distinction and the *a priori-a posteriori* distinction is bigger than it is for Kant. For Kant, the only synthetics that do not require experience are mathematics and metaphysics. For the logical positivist, it is only metaphysics (whose legitimacy as knowledge is challenged) since mathematics belongs in the analytical. Metaphysics aside, the synthetic is also *a posteriori*, and the analytic is always *a priori*. From this we take that, while science relies on empiric testing, all that is to know about mathematics is already contained in its foundations, in its basic propositions and logical rules. Hence, there is a big gap between the activity of mathematicians and that of scientists. This core difference between the natures of mathematics and science accounts for the different conception philosophers have had about what it means to "do" mathematics, and to "do" science. And the mathematical practice failed to draw nearly as much interest as the scientific practice, for the scientists creates knowledge, but the mathematician just describes content that was there already. Rudolf Carnap, a major figure of logical positivism, stated the following on logic and mathematics as non-factual knowledge: "Our solution, based upon Wittgenstein's conception, consisted in asserting the thesis of empiricism only for factual truth. By contrast, the truths of logic and mathematics are not in need of confirmation by observations, because they do not state anything about the world of facts, they hold for any possible combination of facts." (Carnap, 1963).

This view of mathematics was challenged by Willard Van Orman Quine, an American philosopher who made important contributions to logic and the philosophy of mathematics. His most important essay, *Two Dogmas of Empiricsm*, published in 1951, tackles the analytic-synthetic distinction, and criticizes the Kantian and logical positivist notion that there can be analytical, non-factual knowledge at all (Quine, 1951). For Quine, the analytic-synthetic distinction is but a metaphysical proposal, of the very same kind logical positivists casted and ruled out from epistemic matters. It is not rooted in any empirical observation and, for this reason, Quine calls it an unempirical dogma of empiricism (note that logical empiricism is another name for logical positivism). In *Two Dogmas of Empiricsm*, he agrees with Kant and the logical positivists that meanings constitute the basic pillar of analycity: "a statement is analytic when it is true by virtue of meanings and independently of fact". However, he pinpoints two key caveats to analycity: 1) that we have a poorly defined, notion of "meaning", barely shaped by conventional usage, and 2) that synonymy, the relation of equivalence in meaning, ultimately relies on factual knowledge. In the essay, Quine claimed the following on meanings: "(…) are evidently intended to be ideas, somehow -mental ideas for some semanticists, Platonic ideas for others. Objects of either sort are so elusive, not to say debatable, that there seems little hope of erecting a fruitful science about them. It is not even clear, granted meanings, when we have two and when we have one; it is not clear when linguistic forms should be regarded as synonymous, or alike in meaning, and when they should not. If a standard of synonymy should be arrived at, we may reasonably expect that the appeal to meanings as entities will not have played a very useful part in the enterprise."

Quine thus rejects that meanings are necessarily some sort of entity, and believes this trend is a result of a lack of dissociation of the meaning of a concept, and its extension. To illustrate the reader, he retrieves an example from Frege himself: "We must observe to begin with that meaning is not to be identified with naming or reference. Consider Frege's example of 'Evening Star' and 'Morning Star.' Understood not merely as a recurrent evening apparition but as a body, the Evening Star is the planet Venus, and the Morning Star is the same. The two singular terms name the same thing. But the meanings must be treated as distinct, since the identity 'Evening Star = Morning Star' is a statement of fact established by astronomical observation."

As we see, even if Evening Star and Morning Star refer to the same object, their meaning is not necessary the same, that is, their synonymy is not granted. Evening Star = Morning Star is actually a synthetic proposition.

Quine goes further and claims that synonymy based on term definition does not entail analycity either. He states that, because "bachelor" is defined as "unmarried man", the proposition "all bachelors are unmarried" is traditionally considered analytical. However, the definition of "bachelor" is such because a lexicographer says so, and the tasks of lexicographers are, in a sense, empirical. The relation of synonymy between "bachelor" and "unmarried man" is, in fact, a belief the lexicographer holds. In Quine's words: "'Bachelor,' for example, is defined as 'unmarried man.' But how do we find that 'bachelor' is defined as 'unmarried man'? Who defined it thus, and when? Are we to appeal to the nearest dictionary, and accept the lexicographer's formulation as law? Clearly this would be to put the cart before the horse. The lexicographer is an empirical scientist, whose business is the recording of antecedent facts; and if he glosses 'bachelor' as 'unmarried man' it is because of his belief that there is a relation of synonymy between these forms, implicit in general or preferred usage prior to his own work. The notion of synonymy presupposed here has still to be clarified, presumably in terms relating to linguistic behavior. Certainly the "definition" which is the lexicographer's report of an observed synonymy cannot be taken as the ground of the synonymy."

In *Two Dogmas of Empiricism*, Quine challenges analycity at several other fronts. He translates the aforementioned problem of synonymy based on term definition to formal areas like mathematics and logic. He also confronts the statement that analytical propositions are those whose denials are self-contradictory, by asserting that it is a form of circular reasoning (the denial of the proposition is self-contradictory because its truth is necessary, and vice versa); self-contradictoriness needs as much clarification as analycity itself. But we will stop here for the sake of brevity. Suffice it to say that Quine casted doubt upon analycity itself, and his ideas have had great influence in philosophy ever since.

It is of special interest to us that Quine's dismissal of the analytic-synthetic distinction accounts, at least in part, for mathematics being regarded as not radically different from natural science. If mathematics is not confined to an analytic niche subjected to idiosyncratic rules, then all the developments and considerations from philosophy of science could, in principle, be translated to philosophy of mathematics. For instance, according to Paolo Mancosu, Quine himself stated that, if the analytic-synthetic distinction does not hold, "the concepts of logic and mathematics are as deserving of an empiricist or positivistic critique as are those of physics" (Mancosu, 2005).

For philosophy of mathematics, restricted to foundational studies as it has traditionally been, to catch up with philosophy of science is a big deal. Case studies in physics, chemistry, biology and other disciplines have enriched philosophy of science and increased our understanding of what science, as a whole, is. They provided philosophical insights of the practical aspects of these sciences, and in turn, led to new epistemological challenges. In mathematics, however, providing they are comprised by analytic knowledge, case studies make little sense: all mathematics are linearly derived from a set of basic foundations; whatever mathematical breakthroughs we have encounter fall into the category of historical anecdotes –after all, we were always supposed to arrive to that breakthrough, for it was laid in the foundations; who arrived there and when is but historical trivia. Whether the idea of all mathematics being already laid down somewhere waiting to be discovered might still sound intuitive, that an epistemologist of mathematics can afford to turn a blind eye to the practical aspects of mathematics is definitely not. Since the vision of mathematics as simply the study of relationships and meanings is no longer so dominant in philosophy, and its methodology is considered by some circles as fundamentally tantamount to that of science, case studies in mathematics are starting to draw interest from philosophers.

Even more, notions like the Kuhnian paradigm shift, originally devised for science, can now make their way into philosophy of mathematics. Questions such as whether incommensurability is a phenomenon with a place in mathematics, whether mathematics is revisable, whether there is progress in mathematics, and whether there are revolutions -such as the ones philosophers, aided by historians and scientists alike, study in scientific disciplines- are perfectly legit if we are to accept Quine’s arguments.

4. Antecedents of the philosophy of mathematical practice: the rise of anti-foundationalism and constructivism

I have argued before that an excessive focus on foundational work took the attention of philosophers away from other aspects of mathematics. This fact, coupled with the popularity of platonism –the view of mathematics as a network of abstract entities which linger, eternally immutable, within an ideal realm- suppressed any motivation to deal with practical aspects of mathematics. However, some advances and twists in this foundational work actually pointed to the necessity to cover other aspects of mathematics: if there were no solid foundations in mathematics at all, but "sand instead of rock" at its base, it should be explained how we have arrived at the point in where we currently stand in the discipline. Before continuing, it should be clarified here that the claim that there are not solid foundations in mathematics still belongs in the foundational work of philosophy of mathematics. Furthermore, a focus on the practical aspects of mathematics does not necessarily involve any particular stance regarding the nature of the foundations of mathematics. However, here I support the notion that a progressive undermining of the faith on solid foundations leads philosophers to consider issues beyond them.

An early school of thought following this lead is psychologism. One of its first appearances occurred in the late 17th century, when John Locke brought into discussion the idea that epistemological problems from a variety of subjects could be addressed by the psychological study of the individual and mental processes (Locke; edited by Nidditch, 1979). In the 19th century, psychologism in logic and mathematics drew strength from the support of John Stuart Mill (Mill, 1843), who in turn inspired other logicians and psychologists alike, especially in Germany. Logic and mathematics would not be based on foundations of their own, but they were relegated to the foundations of psychology. Regarding logic, Mill claimed that "so far as it is a science at all, it is a part, or branch, of Psychology; differing from it, on the one hand as the part differs from the whole, and on the other, as an Art differs from a Science. Its theoretical grounds are wholly borrowed from psychology, and include as much of that science as is required to justify its rules of art" (Mill, 1889). Therefore, to understand mathematics, one should study the mental processes of the individuals. Frege himself, who actually was almost contemporaneous to Mill, unsurprisingly criticized this line of thought (Frege, 1980). Although psychologism did not underscore the need to pay attention to the mathematical practice, it certainly was a step in this direction.

A related school of thought in philosophy of mathematics is intuitionism. Although some notions of intuitionism first appeared in the 19th century, it gained popularity in the early 20th century thanks to the work of Luitzen Egbertus Jan Brouwer (Brouwer, 1912), a Dutch philosopher and mathematician. Intuitionism shares with psychologism the idea that mathematics is relegated to the laws of the mind and, in a sense, it is a form of psychologism. However, it stresses different notions and its range of applicability is more limited. In fact, and unlike psychologism, intuitionism rejects a substantial body of classical mathematics.

Intuitionism holds that mathematics, as an activity, consists in a process of creating increasingly complex objects by means of methods which display internal consistency with the laws of the mind. Hence, for the intuitionist, all mathematical objects are constructed by the mind, and engrossed in mental processes. These mental processes are the "intuition" of the mathematician, whose role is to "construct" mathematics. But, unlike psychologism, intuitionism is concerned only with mathematical principles and objects, but not –classical- logical relations. For example, the intuitionist rejects the principle of the excluded middle, ($A \vee \neg A$) (Brouwer, 1879). If all objects are constructed by the mind, then only mathematical objects than can be constructed exist. The refutation of $\neg A$ does not mean that A is actually a constructed object. Furthermore, that body of mathematics aided by logical resources discarded by intuitionism, such as proof by contradiction, is of no avail for the intuitionist, and is disregarded. To fill the gap left by the rejection of classical, Aristotelian logic, and resources like proof by contradiction, the intuitionists –with Brouwer's student Arend Heyting as a leading figure (Heyting, 1930)- created intuitionist logic and the method of constructive proof. Psychologism, advocating that not only mathematical objects and principles, but also classical logic is a construction of the mind, does not reject the body of logic that intuitionism disregards or the mathematics sustained by it. If mathematicians and logicians accept a given principle, say, ($A \vee \neg A$), psychologism would propose that an underlying psychological law accounts for it –how they come up with the principle, and why they accept it. Intuitionism instead challenges the *a priori* validity of such principle, and requires the object A to be derived from a certain mental model to assert its truth. Regardless of the differences between them, both seem to point to the need of studying how mathematics is done: intuitionism leads to the question of how mathematicians constructed A, in a similar fashion as psychologism draws interest in how mathematicians accept principles and build upon them.

Intuitionism achieved considerable popularity and led to fruitful work in logic and mathematics. It even succeeded in converting Hermann Weyl, a student under direct guidance of David Hilbert -a major figure of formalism. Weyl, who came to fully embrace intuitionism, was later pressured by his former mentor and settled in what seemed to be, strangely enough, a middle ground: "Mathematics with Brouwer gains its highest intuitive clarity. He succeeds in developing the beginnings of analysis in a natural manner, all the time preserving the contact with intuition much more closely than had been done before. It cannot be denied, however, that in advancing to higher and more general theories the inapplicability of the simple laws of classical logic eventually results in an almost unbearable awkwardness. And the mathematician watches with pain the greater part of his towering edifice which he believed to be built of concrete blocks dissolve into mist before his eyes." (Weyl, 1949).

Although the first works in sociology of knowledge appeared already in the beginning of the 20th century –with Émile Durkheim at the lead- it was not until the second half of the century that philosophy witnessed the advent of social constructivism in a large number of academic disciplines, with Peter Berger and Thomas Luckmann deserving much of the credit for their pioneering book *The Social Construction of Reality* (Berger and Luckmann, 1967). Mathematics was among the disciplines which fell under the focus of social constructivist ideas. Social constructivism in mathematics does not hold the claim that mathematics and its foundations ultimately rest on natural laws from psychology. Instead, abandons references to foundations and natural laws, and consider mathematics as a product of social dynamics: conventions of social groups, traditions, communication, differential deposition of interest in certain problems and methods, meaning and value construction, etc. Mathematical principles and objects exist only inasmuch as such social dynamics keep sustaining them. Therefore, as a product of culture, mathematics can, as well as the sciences, be subjected to revision. It is not a compendium of *a priori* rules and objects or an accumulation of eternal truths. Mathematicians are embedded into these social dynamics, and act as carriers of knowledge and mathematical tradition, but also contribute to their shaping and evolution.

Many philosophers interested in the mathematical practice have been influenced at some extent by social constructivism. This does not come as a surprise, even if being interested in the mathematical practice does not entail any presupposition about whether there are foundations in mathematics or what their nature is. Social constructivism in mathematics heavily emphasizes the study of how mathematicians do mathematics, either as individuals or as groups. In fact, this might well be the only important factor to consider.

5. The pioneering philosophy of mathematics of Imre Lakatos

Finally, the first elaborated philosophical work concerned with the practical aspects of mathematics to achieve considerable renown was crafted by the Hungarian philosopher Imre Lakatos. Although he never defined himself as a social constructivist, his philosophy is believed to be influenced by such movement. More notably, his close friendship with Paul Feyerabend led to a mutual influence between the two philosophers (Lakatos and Feyerabend; edited by Motterlini, 1999). Feyerabend's epistemological anarchism is grounded on the idea that there are no universal, fixed rules that dictate the proceedings of science, and that much of the success of science is due to its methodological flexibility (Feyerabend, 1975). Lakatos indeed thought that mathematics, as a discipline, allowed for, and its historical development was based on, such flexibility. Against previous formalist assumptions, he claimed that informal methods (visualization techniques, thought experiments, heuristics, etc) were at the base of most of the developments in mathematics in the 19^{th} century, that mathematics is in constant change and is revisable, and that therefore the practical aspects of mathematics should be granted more attention than they traditionally get (Lakatos, 1976b).

Most of Lakatos' work in philosophy of mathematics was compiled in his book *Proof and Refutations*, posthumously published in 1976, two years after his death. To support his controversial claims, he developed a case study (which are so popular in philosophy of science, but previously inexistent in philosophy of mathematics) on Euler's conjecture for polyhedra (for all polyhedra, the number of vertices, minus the number of edges, plus the number of faces equals two ($V – E + F = 2$). The case study is presented, however, in a very unconventional fashion: it takes the form of a fictional dialogue in the context of a mathematics class, where the students provide counterexamples to Euler's conjecture for polyhedra. With this dialogue, Lakatos aims to illustrate how mathematics is done from a philosophical perspective while being faithful to the actual practice of mathematicians.

The title of his book, *Proof and Refutations*, is actually an allusion to Popper's *Conjectures and Refutations*. Lakatos was deeply influenced by Popper's philosophy of science, borrowed much from it, and in a sense, his own ideas suppose a continuation of it. Popper's famous point that science is always preliminary or fallible –there are no true theories, principles or laws, only theories, principles or laws for which no counterexample has been yet found- was brought to mathematics by Lakatos, even when Popper never thought his philosophy would apply to this discipline (Gregory, 2011). Lakatos claimed that Popper “made the mistake of reserving a privileged infallible status for mathematics”. According to Lakatos, counterexamples for well established mathematical theorems can appear at any point in the development of mathematics. But, unlike Popper in regard to scientific knowledge, Lakatos claims that a counterexample alone (or a given number of them) is not reason enough to discard a mathematical theorem –not even scientific knowledge, as he claims in his own philosophy of science. In short, Lakatos agrees with Popper that *proof* does not warrant permanent and intrinsic truth value to knowledge, but he assures that *refutation* is not reason alone to discard knowledge either, and very importantly and to the disdain of a very dominant-at-the-time formalist tradition, that these two considerations apply not only to science but to mathematics as well.

Lakatos’ general philosophy of science will not be explored here. Suffice it to say that, according to him, when well established scientific theories are confronted with empirical counterexamples, scientists often opt to either reduce the scope of the theory in question, or modify *auxiliary hypotheses* of what he calls *research programmes* (networks comprised by sequences of theories), instead of their *hard core* (Lakatos, 1976a). But what is the role of proofs and refutations in his philosophy of mathematics? Let us come back to Euler’s Polyhedral Formula and the fictional dialogue in *Proof and Refutations*. Lakatos claims informal mathematics is at the base of the discipline, and he intends to represent how mathematicians actually dealt with Euler’s conjecture. In the dialogue, the teacher presents informal proof -after Augustin-Louis Cauchy’s proof (Cauchy, 1813)- based on the mathematical activity of visualization, a typical resource in the mathematical practice) for the conjecture at class, followed by a panoply of counterexamples posed by the students. For example, one of the students presents the case of a hollow cube, for which $V – E + F = 4$. Right away, another student remarks that, after Euler’s conjecture has been refuted, we can no longer rely on it, and it should be disregarded and discarded together with previous proof. He also claims that “A single counterexample refutes a conjecture as effectively as ten”. The teacher, however, defends a particular idea of proof and its role in mathematics: “I agree with you that the *conjecture* has received a severe criticism by Alpha's counterexample. But it is untrue that the proof has 'completely misfired'. If, for the time being, you agree to my earlier proposal to use the word 'proof' for a 'thought-experiment which leads to decomposition of the original conjecture into subconjectures', instead of using it in the sense of a 'guarantee of certain truth', you need not draw this conclusion. My proof certainly proved Euler's conjecture in the first sense, but not necessarily in the second. You are interested only in proofs which 'prove' what they have set out to prove. I am interested in proofs even if they do not accomplish their intended task."

In addition, Lakatos described a number of strategies mathematicians employ in order to adjust and preserve the core integrity of a theorem when faced against counterexamples. He takes proofs and refutations away from the absolutist role ascribed to them by former traditions in philosophy of mathematics -that is, ahistorical, culturally invariable, universal judgements about the virtues of mathematical propositions. Instead, proof and refutations constitute what Lakatos calls a "dialectical unity", ultimately depending not on formal criteria, but on the mathematical community –which has the last word on matters such as the validity of, and the reactions to, each proof and refutation. Lakatos puts forward this dialectical unity of proofs and refutations as a key element of mathematical progress and refinement. We see how Lakatos' intent is to contrast how mathematics is assumed to work by a dominant formalist school of thought, against how he considers it actually works. It is worth noting that Lakatos dealt with, and underscored the importance of, aspects of the mathematical practice that would later be studied in more detail, such as proving and explaining (different proofs for the same theorem may have unique epistemic properties), visualization (mental imagery techniques with special relevance in areas like geometry), and the interrelations between informal and formal mathematics, and how mathematics "grows". All these issues have shaped the development of mathematics, and therefore, a philosophical study of mathematics that overlooks them is either partial or lax.

Despite the potential of his ideas, Lakatos' philosophy of mathematics was not too warmly received by the academic community of his time. His quarrel with foundational studies and formalism (and particularly his disdain towards formal proofs) was met with strong opposition. In addition, it is argued that he died too early for his ideas to have a greater impact on the philosophy of mathematics of his time (Ferreirós and Gray, 2006; Mancosu, 2008). This led to Lakatos' failure to establish a new trend in philosophy of mathematics, at least in the short term. In fact, the next work following a similar line, Philip Kitcher's *The Nature of Mathematical Knowledge*, did not appear until almost two decades later.

6. Historical awareness and its unification with philosophy of mathematics: a renovated spirit of the history of mathematics

Although changing interests and perspectives of historians of mathematics could well be in the section of antecedents of the philosophy of mathematical practice, this factor is, strictly speaking, coetaneous to the philosophical movement. Philosophy and history go hand in hand; they provide mutual feedback and guidance to each other: while history provides material for philosophy to scrutinize, philosophy provides insights on how to address and what to seek in history. In fact, as we will see, not only historians and philosophers do their best when working in parallel, but many philosophers are concerned with historical analyses themselves.

History, by no means, is neutral: not only it is situated and takes –a- perspective, but it also has a scope that can be narrower or wider, and is selective: it can grasp some features of the factual knowledge, or others. Given the cultural and academic influence of not only naïve realist philosophies of mathematics such as platonism, but also of formalism and traditional philosophical concepts like apriorism and analycity, it does not come as a surprise that the heavy bulk of history of mathematics has revolved around trivia: if all mathematics is already there, waiting to be "discovered", or is already contained within the premises of its solid foundations, then the relevant questions for the historian of mathematics are who discovered what, and when. Similarly, this trivia-based history of mathematics does not invite philosophers of mathematics to direct their attention to other questions in philosophy of mathematics beyond those of foundational studies: it fails to both draw their interest to topics concerned to the actual mathematical practice, and even more importantly, to provide philosophers with content to scrutinize nuances of the practice of mathematicians and develop case studies of breakthroughs in mathematics. Therefore, to address the kind of questions we have seen in previous points, the history and philosophy of mathematics are required to operate synergistically.

We have already seen that mathematics, as a whole, is much more than just a set of perennial axioms that can be stretched indefinitely. For this reason, it is the task of historians of mathematics to shed light on the evolution of mathematics over time, paying attention to details that are easily overlooked by both mathematicians and philosophers. And some, like Michael Beanie, take this to the next level: he argues that historical elucidation is indispensable irrespectively of any metaphysical assumptions held about mathematics, especially since foundational work itself, the almost exclusive occupation of traditional philosophy of mathematics, needs to be complemented with historical approaches (Beanie, 2006). Even more, for fictionalism, a recent philosophy of mathematics founded by Hartry Field in 1980, the reliance on the history of mathematics is such that any mathematical statement, regardless of its form, boasts truth value or not by exclusive virtue of the historical development of mathematics (Field, 1980).

This lackluster history of mathematics contrasts with the richness and sophistication that the history of science displays. But then again, the philosophy of science has covered many more topics than the philosophy of mathematics, as we already know. It is not surprising that the history of science has yielded plenty of historical on which the philosophy of science has had the opportunity to reflect upon. Consider the vast amount of historical approaches in physics and chemistry, which inspired Thomas Kuhn to develop the core ideas of his magnum opus, *The Structure of Scientific Revolutions*, and many other philosophical debates in science –from ideas on scientific (anti)reductionism, to causality, (un)determinism, etc. This kind of historical work has emerged even for younger disciplines like molecular biology and psychology, which has made possible, for instance, the development of case studies in the context of the debate around reductionism (see, for example, how Schaffner turns a recapitulation of the progress made in molecular biology, neuroscience and psychology in a paradigmatical organism, into a case study sustaining his views on reductionism [Schaffner, 2011]) and given rise to insightful ontological and epistemological considerations in these sciences (for instance, the gradual, multileveled metamorphoses of the concept "species" in evolutionary biology [Marcos, 2009] and of the concept "life" in biology as a whole [Diéguez, 2008], the implications of the resurgence of early 20th century organicist views in modern theoretical biology [Nicholson, 2014; Etxeberria and Umerez, 2016], and plethora more). This list, unlike its equivalent for mathematics, could go on for much longer. This is all the more striking when considering that mathematics, as a discipline, is older than physics, chemistry, biology and any other field of knowledge based on the scientific method. Considering, after

Quine’s dissolution of the analytic-synthetic distinction and Lakatos’ fallibilist account of mathematics, that mathematics and science should be treated similarly, it can be concluded that historians of mathematics have yet a lot to explore. The historian of mathematics should not be satisfied with assuming that there is a theorem that was always lying somewhere, which was discovered by someone at a given time, whose never-changing properties were finally known, and whose proofs and refutations constitute absolute judgments of character. The historian of mathematics should be deeply immersed in the mathematical debates of the past and in the nuances of breakthroughs and the work of historically important mathematicians instead of just documenting who ended up being “right”, elaborating biographies and compiling general trivia.

It has been only recently that this type of historical approaches has dealt with mathematics. As the reader probably speculates by now, such historical approaches started with Lakatos, very much like the philosophical approaches focus on the mathematical practice did. Lakatos’ work not only portrays his philosophy of the mathematical practice, but also aims to introduce the reader to some historical details relevant to his philosophy. After all, his proposed role of proof and refutations in mathematics is taken from actual historical examples of the works and debates in mathematics. He insisted that the nature of the mathematical practice required historically sensitive studies in order to be properly comprehended. He famously paraphrased Kant, claiming that “Philosophy of science without history of science is empty; history of science without philosophy of science is blind” (Lakatos, 1971). Finding a lack of such historically sensitive studies particularly in mathematics, and treating mathematics equally to other sciences, he then stated: “The history of mathematics, lacking the guidance of philosophy, has become blind, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become empty.” (Lakatos, 1976b).

After Lakatos, there have been more philosophers concerned with the mathematical practice that have stressed the need for historic studies in mathematics. Kitcher was the next one. Even if his focus lied out of localized case studies of historico-philosophical interest, he intended to develop a general theory of mathematical growth accounting for transitions between delimited mathematical practices, for which he needed to revisit many episodes in the history of mathematics (Kitcher, 1984). Even if it is still young and represents a small minority, this new trend in the history of mathematics has been progressively becoming more popular and sophisticated. Looking back at the time when Lakatos developed his philosophy of mathematics, Ferreirós and Gray realized how much progress has been made, but citing as a reason for this the fact that there was (and there is) plenty to do and choose from: "(…) It should be noted that there are now rich historical accounts of many topics in the nineteenth century and a number of topics in the twentieth century, and their growing sophistication illuminates our theme (…) The new generation of historians of mathematics that came along in the 1970s did not have mountains to climb in order to get started (…) No better indictment of the poor state of the history of mathematics when Lakatos wrote (and of mathematics teaching in general, one must presume) can be given than the lasting popularity of this book, because as a work of history it is disappointing." (Ferreirós and Gray, 2006).

Ferreirós and Gray themselves are members of the current community of philosophers concerned with the mathematical practice. They, and some other members of this community, address the mathematical practice of, especially, the 19th century, due to the vast changes mathematics experienced during that time and their repercussions in today's mathematics (some call this set of changes "the advent of modern mathematics"). However, a few also approach 20th century mathematics and, more rarely, earlier mathematics. A prominent example of the latter case is Paolo Mancosu work on 17th century mathematics, compiled in his book *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*, where he reflects upon René Descartes' *La Geometrie* and his unification of algebra and geometry, and Gottfried Wilhelm von Leibniz's take on the mathematization of physics, among other topics (Mancosu, 1999). He blames the reduced scope of the history of mathematics for giving "the false impression that the majority of philosophers of the period could only tackle very elementary questions in geometry and were not aware of the significant transformation mathematics was undergoing", and for blinding us from "the significant contribution that many philosophers at that time made to the clarification of foundational issues of central concern to mathematical practice".

Unlike Lakatos and Kitcher, whose aim was to create grand theories of mathematics (a philosophy of mathematics in the frame of a more general philosophy of science, and a general account of how mathematics rationally evolves over time, respectively), current philosophers of the mathematical practice seem to focus on more confined spaces of inquiry (understanding the nuances and legacy of, for instance, a local tradition, a given breakthrough, or a given mathematician's methods). This comes with its advantages and disadvantages: while the details of a given practice is analyzed in depth, and its implications in mathematics as a whole are better understood, it could be argued that the big picture is lost and the unification of studies for a grand theory in philosophy of mathematics is a complicated enterprise. Another way to see it though is that the two proceedings have different purposes and are complementary. I believe that the current trend towards more localized studies responds to a need for more analytical depth, which is precisely was history and philosophy of mathematics have been lacking all along. However, once historians and philosophers of mathematics catch up with their counterparts concerned with the sciences, and light is shed on a certain number of episodes in the history of mathematics, it is possible that we will again see the ambitious kind of work that Lakatos and Kitcher set out to consummate. It might be that Lakatos and Kitcher were pioneers far ahead of their time, who nevertheless put the cart before the horse.

Let us consider Andrew Warwick's *Masters of Theory: Cambridge and the Rise of Mathematical Physics (Warwick, 2003).* Warwick devotes an almost 600-page book to cover a spatially localized and relatively brief chapter of the history of mathematics: it encompasses the evolution of the study and practice of mathematics (and mathematical physics) at Cambridge University in the late 18th, 19th and early 20th centuries. The book is divided in two halves. The first, covering a series of events between roughly 1770 and 1880, is concerned with the university's pedagogical methods and traditions, its evolution and how it affected the mathematical practice and academic developments. On the other hand, the second part discusses how such pedagogical legacy hindered the incorporation of foreign achievements from the late 19th century and the early 20th century into the Cantabrigian academic scene.

At the time, Cambridge University was the most prominent institution for mathematics in all Britain, and as such, educated many famous mathematicians and gave birth to a vast amount of mathematical knowledge –including the inception of mathematical physics. This, coupled to the fact that the University of Cambridge has always advocated a high degree of competitiveness, led to the establishment of a very particular way of teaching and examining mathematical knowledge. Across centuries, this demanding and meritocratic Cantabrigian spirit has been well reflected in the examinations on mathematics sat by the students, commonly called "mathematical Tripos". It has been mentioned that the mathematical Tripos was the "most difficult test that the world has ever known, one to which no university of the present day can show any parallel" (Roth, 1971).

Warwick argues that the Cantabridgian mathematical microculture, at all levels, revolved around the Tripos: mathematics in Cambridge evolved together with the Tripos, so that modifications in the implementation of the latter were intensely reflected in the mathematical practice of the time, not only regarding educational matters and preparatory means for the exams, but also in the research conducted and the attitudes towards different mathematical practices. For instance, Warwick describes that in the 19^{th} century a big modification was implemented to the Triposes, whose focus steered away from oral disputation to meet a more rigorous written format. This allowed placing a stronger emphasis on concatenations of mathematical arguments, but this shift in the Tripos' format was leveraged to adopt as well two key features: 1) a strong and particular interest in the multiple nuances of problem solving (analyzing and defining the problem, contemplating solutions, reaching meaningful results, etc), and 2) a preference for geometric, visual representations of problems and the rendering of solutions in diagrammatic form, which was believed to offer a better grasp of the nature of problems (as opposed to the leading-at-the-time French symbolic reasoning). As the complexity of the mathematical Tripos escalated in the 19^{th} century, the problems presented became increasingly more abstract in their own way, following such a different path to those of mathematics in Continental Europe that each tradition sounded alien to each other, to the point of reaching a state of almost mutual incommensurability in the proper Kuhnian sense. A very particular mathematical ecosystem began to consolidate, Cambridge mathematics became a language of its own and, as Gregory

Moore notes (Moore, 2005), its influence would spread even to fields such as economics. In regard to the preference for the visual representations of problems, Moore claims the following: "It is, I believe, this preference that explains why those Cambridge men who migrated to the social sciences were so ready to represent their ideas diagrammatically (with Keynes being a famous exception). Stephen represented the laws of demand and supply diagrammatically in the early 1860s, Venn reconstructed Boole's logical processes diagrammatically, Marshall found it entirely normal to translate passages from Mill's Political Economy into relationships in two-dimensional space, and Bowley constructed extremely messy contract curves in the Edgeworth-Bowley box."

The reasons why the Tripos exerted so much influence in the mathematical practice in Cambridge as a whole are varied. The highly demanding exams prompted the students to hire private coaches to instruct and prepare them, and it so happened that these coaches were former students who succeeded in the Tripos, and were therefore versed in the problem solving approach to mathematics in question –Warwick even claims that is was very unlikely for a student to succeed in the Tripos without the assistance of one of these coaches. This way, the cycle is perpetuated in the educational sphere of mathematics in Cambridge, but there is more. More often than not, the faculty members themselves (who unsurprisingly were in their majority former Cambridge students) would elaborate their papers and present their research in the abstract, diagrammatic, problem solving Tripos format, and in fact, their research and particular problem solving techniques made their way to the Triposes very quickly. This process indirectly served as a normalization mechanism for new work. A prominent example of this is the lasting legacy of the work of James Clerk Maxwell, especially *Electricity and Magnetism* and Maxell's equations –it should be also considered here the Cambridge academic community approached mathematics and physics in a very similar manner, and this accounts in part for the inception of mathematical physics.

It is not a lack of success of the Cambridge model what Warwick criticizes; in fact, it led to great scientific achievements. Rather, Warwick finds fault with both an inability and unwillingness to acknowledge and deal with foreign developments in the late 19th century and early 20th century, like the notable case of Einstein's work on general relativity. However, even the work of other British scientists, like Poynting's theory of energy flow –boasting the anti-Newtonian idea that energy can exist in empty spaces- found resistance within the academic community at Cambridge. Foreign achievements in mathematical physics, like those of Hertz and Einstein, made their way into Cambridge with a considerable delay, and were met with certain disdain. Warwick quotes the following from a letter sent from George Frederick Charles Searle to Albert Einstein in 1909: "I am sorry that I have so long delayed to write to thank you for sending me (…) a copy of your paper on the principle of relativity. I have not been able so far to gain any really clear idea as to the principles involved or as to their meaning and those to whom I have spoken about the subject in England seem to have the same feeling." Warwick claims that the Cantabrigian microculture and in particular the mathematical Tripos account not only for the difficulty for Cambridge mathematicians to understand Einstein's research, but also for much of the disdain with which Einstein's accomplishments were met in Cambridge and, in general, for the criteria the academic community followed to judge, and ultimately accept or dismiss, mathematical work. In Warwick's words: "We have seen that mastering mathematical physics in the Cambridge style was a process that required a protracted period of guided study, problem solving, and, ideally, face-to-face interaction with a competent practitioner. These were the pedagogical resources that generated the wrangler's sense of what criteria a good theory had to fulfill and how it was properly applied to solve problems. It follows that a symmetrically firm and sympathetic grasp of Einstein's work would be virtually impossible to obtain simply by reading his published papers."

Warwick's *Masters of Theory* is a representative example of what a historical account of the mathematical practice, in my opinion, should be: it keeps a narrow spatio-temporal focus, allowing a deep immersion into details that would normally not draw the attention of historians of mathematics. It offers insights on how different mathematics are done in different places (and different times), on the role of institutionalization of knowledge, and on how mathematics and culture interact. Even if it is true that these kind of historical accounts of mathematics have only appeared as of recently, there are other works in this line that cover different episodes of mathematics and yet teach us similar overall lessons. Ferreirós and Gray, in the line that different mathematics are done in different places, note how the Berlin school of mathematics from the 1850s to the 1890s run by Kummer, Kronecker, and Weierstrass, differed substantially from the Göttingen school founded by Felix Klein in the 1890s: the former exhibited a marked neo-humanist spirit with little interest in applications and lived up to Gauss' views considering geometry lacking in rigor and not belonging in pure mathematics, while the latter avoided such neo-humanism and considered geometry to be not only a proper subject of study of its own, but also as a means for mathematical discovery in other areas (Ferreirós and Gray, 2006). Jeremy Gray alone, in the line that mathematics is intertwined with culture, discusses in *Modern Mathematics as a Cultural Phenomenon* that the mathematical practice evolves in unison with science, literature and art, since similar paths of evolution in these areas can be abstracted by looking closely at the developments of the 19th century (Gray, 2006). In closing, we see that the mathematical practice is sensitive to sociohistorical factors, and hence, for philosophers to properly deal with it, they must be provided with relevant, rich and detailed descriptions of different practices, as well as historical reconstructions of what was going on at the places where, and the times when, such practices developed.

7. Conclusion

Mathematics, for a very long time already, has enjoyed a privileged position among the sciences. The famous saying that “Mathematics is the queen of all sciences.”, ascribed to Gauss (von Waltershausen, 1856), is but a token of this lasting conception of mathematics. Indeed, mathematics has been considered a source of immaculate knowledge, more resilient to “contaminating” factors that can be found in the sciences from time to time: mathematical knowledge is presented to us as true by its own virtue and, if there is something at all worth double-checking in mathematics, is the nature of their foundations.

In this essay, I have argued that, rather than having enjoyed such privileged position, mathematics has suffered from it, since they have not partaken in a long, fruitful tradition of philosophical studies of science. It has missed out on so much that philosophers now face an area with so many ideas worth exploring and possibilities ahead of it that can be certainly overwhelming: the philosophy of mathematical practice. I have also explained how the mathematical practice differs from the traditional philosophy of mathematics, which has been concerned with what lies ultimately below all mathematics, but not with mathematics itself, with what mathematicians actually do.

The privileged status of mathematics is not the only factor that accounts for mathematics eluding philosophical inquiry. We have seen how hard wired, traditional ideas in philosophy like the analytic-synthetic distinction have hindered the rise of the kind of philosophical work I advocate here. Likewise, there have been some other developments in philosophy that, I believe, promoted the appearance of the first works in this line, like Quine’s criticism of analycity and some foundational work in mathematics that questioned apriorism, logicism, formalism, and paradoxically, the very existence of steadfast foundations. These developments might have prompted philosophers to understand mathematics by looking beyond their foundations. At long last, it was in the second half of the 20th century that Lakatos investigated, if only at a reduced scale, what role the practical aspects of mathematics played in the development of the discipline in the 19th century, effectively inaugurating a new line of philosophical inquiry in mathematics.

Finally, I have characterized the role reserved for historians of mathematics in this complex scenario. It only makes sense to work on the philosophy of mathematical practice if there are historical documentations of such practices available, and compilations of trivia impregnated with naïve metaphysical assumptions are of no use to this enterprise. As well as the philosophy of mathematics, the history of mathematics has to catch up with their counterparts in science. Also, the historian must rely on philosophy –and ideally, have some degree of familiarity with the philosophy concerned with his interests as a historian- to know what to look for and seize relevant information. There is therefore a mutual reliance between the history of mathematics and the philosophy of mathematical practice. Moreover, the historian of mathematics must keep in mind how sensitive the mathematical practice is to sociohistorical factors and establish links between them and the practices in question in their historical reconstructions.

As some philosophers have shown by now, there is a lot that the philosophy of mathematical practice brings to the table: a better understanding of what doing mathematics really means, why some courses of action and techniques are chosen instead of others, the critical consequences of given practices in the development of mathematics and in the sciences served by mathematical applications, and much more. However, given the overly tender age of this new kind of philosophy of mathematics, it is difficult to predict through what paths and to what realizations it will lead us.

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