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THE IMPORTANCE OF BEING EXTERNALIST ABOUT MATHEMATICS – ONE MORE TURN?

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1. Introduction

Over thirty years ago, Stephen Toulmin, in his *Human Understanding*, argued that a bridging of the gap between scientific practice and its reflective analysis was in order.[2] We are convinced that today the argument still stands, above all when it comes to mathematics. Indeed, while opinions might differ over the extent to which bridge-builders have succeeded in their task for science in general, there can be little or no discussion as to the lack of much progress in this respect as far as mathematics is concerned. The thing is that much of the people committed to one of both sides of the divide, respectively mathematicians and meta-reflectors (philosophers, sociologists, and historians), do not themselves think that the gap should be closed, let alone that doing so would be a priority of any sort for any of them. We strongly disagree with this view. Standing in front of the gap, as philosophers *cum* social scientists, looking hopefully towards the other side, but finding only the tiniest traces of durable crossings, we are prompted to ask if mathematicians have become too unreflective, or if

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[2] "There are good reasons (…) for re-establishing links between the scientific extension of our knowledge and its reflective analysis, and reconsidering ourselves as knowers in the light of recent extensions to the actual content of our knowledge" (Toulmin [1972], p.2).

fellow reflective analysts of mathematics have lost (or still haven't found) a sense of practical relevance.

In this paper, we explore existing routes to overcome the divide, starting from the sociology of science (section 2), looking for applications within this field to mathematics (section 3), and examining the influence from and upon the philosophy of mathematics (section 4). After having briefly assessed the present status of the field (section 5), we formulate "one more" alternative sociological approach of mathematics, more particularly on the basis of Bruno Latour's work.

2. Sociology of Science Old and New

As early as in (the first part of) his *Cours de philosophie positive* (1830-1842), the founding pamphlet of systematic or scientific sociology, Auguste Comte (1798-1857) raised the metascientific issue. In a reaction against speculative philosophy, he there emphasized the complementary contribution of induction *and* deduction to scientific progress. So although being far from a positivist (at least in the present day sense), Comte did hail an empiricist spirit. The consequences he drew for the theory of (scientific) knowledge were that individualist and a-temporal preconceptions had to be abandoned: research is to be seen as a continuous process, the results of which are reached and passed on by successive generations. Comte denied that the distinction between eternal logical categories and their variable subjects, i.e. between form and content of thought, or between method and object of inquiry, was more than an analytic one. He instead preached the end of traditional, idealized conceptions of knowledge, viz. as egocentric, immutable, monolithic modes of thinking (e.g., the idea that there would be one and only one scientific method), and strongly condemned the philosophical primacy of normative questions over descriptive ones. All these factors for him had indeed impeded the development of sociology, which was according to Comte the true 'queen' of the sciences, into an autonomous discipline. Norbert Elias (1897-1990), the famous German sociologist, has called this condemnation by Comte of traditional philosophies of (scientific) knowledge nothing short of a 'copernican revolution' (Elias [1970], ch.1).

It would take until Thomas Kuhn (1922-1996), however, with his influential study *The Structure of Scientific Revolutions* (1962, 1970^2), for this deep and original idea to gain any real philosophical momentum. Indeed, an important part of present day sociology of science largely thrives on this social realist perspective.[3] This is remarkable and important to bear in mind: as sociology is conceived, with Comte, we immediately see the fierce contrast with 'the' (typically a-social and a-historical) philosophical or systematic perspective popping up. Actually, this conflict has continued to date. Under labels such as *culture wars* or *science wars,* discussions on the nature of science, as seen from the descriptive and normative viewpoints of metascience and philosophy respectively, have remained inextricably linked to the matter of the scientificity or trustworthiness of the humanities.[4]

The sociology of (scientific) knowledge as a discipline was established less than a century ago, nearly a century after sociology proper. And contrary to what the father of sociology, Comte, suggested, traditional sociologists of (scientific) knowledge like Karl Mannheim (in the 1920s) and Robert Merton (in the 1940s), held that the view from the outside on scientific practice can only serve to account for actual deviations from the ideal norms established by philosophy.[5] Far from being relativist –a label commonly applied to sociologists of science today– they aspired to safeguard science as a privileged way of producing reliable knowledge, at least *in principle*. Sociology and philosophy, in their eyes, had neatly separated tasks, respectively descriptive and normative in nature. Of course, this applied with full

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[3] "Kuhn opened the door to a new field – the sociology of science. Because science, like any other human endeavour, is influenced by social forces, it provides fertile ground for the historian or sociologist to ask a number of very interesting questions. About this there is little controversy. The argument arises over the extent to which the ideas of relativism and social construction are used in science studies" (Baringer [2001], p.6).

[4] Two of the most famous such episodes were those centred around C.P. Snow's *The Two Cultures* (1959) and, quite recently still, around Sokal and Bricmont's *Intellectual Impostures* (1997), following the so-called 'Sokal Hoax' in 1995.

[5] See, e.g., Mannheim [1936] and Merton [1973].

strength to the case of mathematics, a field not exhibiting any traces whatsoever of human interference and apparently not liable to fallacy, thus uninteresting as a subject of sociological inquiry.

In stark contrast to this so-called *weak programme* in the sociology of science stands a school, established in the 1970s, that trades in the "exceptionalism" with which science, *casu quo* mathematics, is treated for what it has called an "impartialist" and "symmetricalist" stance, i.e. the view that all types of knowledge, lay or scientific, true or false, are to be explained by appealing to the very same social mechanisms. We are of course referring here to the *strong programme* embodied by David Bloor's and Barry Barnes's Edinburgh School. It has relativist implications, in no longer treating science, including mathematics, as epistemically privileged *vis-à-vis* other forms of (lay) knowledge, which *were* seen as determined by social conditions by traditionalists alike. Ever since its estrablishment, the strong programme has been the source of inspiration for a number of other sociological schools looking for explanations of scientific knowledge, treating it in the same realist way as empirical scientists treat nature. They have been subsumed under the term sociology of scientific knowledge (SSK). "The central argument of SSK is that all knowledge and all philosophical divisions are socially crafted by knowledge practitioners" (Ward [1996], p.86). According to Zuckerman [1988], then, SSK presently divides "into two streams: those emphasizing social influences on the structure and development of scientific knowledge and those focusing on the social construction of knowledge itself" (p.541).

The latter division we shall opt to label here as the second of two externalist-internalist ones, viz. one concerning the subject matter under consideration, where the focus is either on the social dynamics of scientific practices as embedded within a vast array of other social practices, or on the more narrow scope of scientific practice as isolated (or at least abstracted) from these broader dimensions. Note that both perspectives, although different in scope, are externalist in the first or primary sense of the word (that of the title of this paper). That is, being sociological in nature, they imply a view on particular sciences in terms other than their proper (methodological) ones. In this sense, on the whole, *qua* approach, the sociology of science is an externalistic discipline. Further on, in section 6, an additional division within (the internalistic part of) this metascientific discipline, viz. between knowledge-centred and practice-centred approaches, will be drawn.

3. Sociology of Mathematics Old and New

3.1 Externalistic Focus

J. Fang, founder and initial editor of *Philosophia Mathematica*, the only professional journal to exclusively focus on philosophy of mathematics, envisaged with it the setting up of "a meeting place, however precarious at the initial stage, for the *four* groups of specialists with some positive interest in interdisciplinary studies: historians, mathematicians, philosophers, *and sociologists*" (Fang/Takayama [1975], p.5, our emphasis). This enlargement of Kenneth O. May's outline of scope at the start of *Historia Mathematica* was meant to counter an aberration resulting from the "common practice among philosophers" to "whenever and wherever they get stuck, [...] draw lines of demarcation" (op.cit., p.33), which in the case of mathematics, according to Fang, had resulted in obscuring "the complementary relation between history and sociology" (op.cit., p.34). This seems to suggest that in order to further the philosophical importance attached to a historical awareness of mathematics, an accompanying attention to its markedly social dimensions may prove vital. As Raymond Wilder has noted, in effect, mathematicians of all time have predominantly been led by what they considered (or what was supposed) to be mathematics in their proper culture, a circumstance historians of mathematics should not at all overlook (Wilder [1998]).

Prominent contemporary historians of mathematics, such as Bell, Struik, Kline, Grattan-Guinness or Glas,[6] have indeed increasingly shown to be sensitive to the matter. Moreover, some have at a point addressed the very topic itself. For example, Kline and Glas (like Wilder) have written entire books on the role of mathematics in (Western) culture, and

[6] We suffice here with giving some of the most famous or general references: Bell [1992], Struik [1987], Kline [1990], Grattan-Guinness [1998]. For representative work by Glas, see for example recent volumes of the journal *Studies in History and Philosophy of Science (part A).*

it was Struik who coined the term "sociology of mathematics" to begin with, in his [1942].[7] Be that as it may, the philosopher of mathematics *qua* epistemologist, that is, as someone with an eye on developing a systematic theory of knowledge, would expect more from a sociological approach than the mere illumination of mathematical elements *in* culture, i.e. the influence exerted by mathematics on society and vice versa, as laudable and important an effort as this most probably is. That is, (s)he would also, and even primarily, be interested in mathematics *as* culture, that is in the discipline's internal (but possibly externally conditioned) social dynamics of knowledge creation, a focus on which would be more than helpful in order to properly evaluate the epistemological significance of mathematics as a human *practice*.

David Bloor, upon spelling out the strong programme, has coupled the content-context distinction hailed by sociologists of science 'old style' to two different sorts of explanatory principles in thinking about science, including mathematics. He literally speaks of an inside and an outside. Traditionally, sociology was only to speak of the latter, that is: about mechanisms facilitating or preventing the getting inside, not about the goings on within this inside. Clearly, Bloor has thereby opposed himself to mainstream philosophy, for whom mathematical practice is considered a purely internalistic affair, more particularly a matter of logic or mathematics proper, and consequently to be dealt with by some or other foundationalist programme. In contrast, for Bloor, the very same mechanisms are in play within *and* without: all is liable to sociological explanation. Hence his conclusion "that the sociologist is no longer excluded *a priori* from dealing with mathematical activity itself" and that "the grip that logic has upon us [becomes] a fact to be explained rather than the revelation of a truth to be justified". Put otherwise, with reference to the traditional programmes referred to in the previous section: "A sociology of knowledge rather than a sociology of error is possible" (Bloor [1973], p.190). So, before Bloor, only 'false beliefs' were to be explained by reference to social factors, or Society, whereas 'scientific truths' were to be explained by reference to Nature. With

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[7] Admittedly, Oswald Spengler seems to have foreshadowed this evolution, especially in his rather controversial book *Untergang des Abendlandes* (The Decline of the West; 1918-1922).

Bloor, however, scientific truths have become as much liable to sociological scrutiny as false beliefs.

A mathematical application of the Bloorian analysis pertains to the 19th century Hamilton-Peacock divergence over the essence of algebra, emphasizing metaphysical (viz. religious) and political factors influencing the courses chosen (Bloor [1981]). Other instances are to be found in the fifth chapter of his programmatory book *Knowledge and Social Imagery* (Bloor [1991]), viz. his contested development of 'alternatives' to current 'universal' mathematics. In both cases, what primarily determines the course mathematical history takes, for Bloor, is the pressure of socialization and power relations, not scientific practice as such. Because of the utter contingency and social realism involved, enthusiasm for this strong programme among students of mathematics and its reflective analysis, has been tempered.

3.2. Internalistic Focus

As said, in contrast to this Bloorian externalist or 'interest' model, there happens to be also an internalist or 'discourse' variant of the knowledge-centred sociological view. Here, one focuses on distinctive characteristics of communication within the setting of scientific practice itself instead of on external (institutional, societal) pressures exerted on scientists. To varying degrees, there is room here for considering the distinctive features, if not privileged status, of scientific knowledge, while nevertheless scientific and lay knowledge will not be considered different in quality, but rather as continuous. A famous representative is Harry Collins, the founder of the Bath School, whose central thesis is that accounts of reality or facts are negotiated between practicing scientists with different (empirically supported) claims.

Another school concerned with practice in the stricter or more limited sense is Ethnomethodology, one of its main proponents being Michael Lynch. A mathematical application of the latter approach was made by Eric Livingston in his [1986]. His concerns have certainly been more internalistic (or turned towards practice), although it can and has been argued that he has mainly done an anthropological instead of an explanatory job. David Bloor for example, in a review article, has dismissed this type of 'descriptive analysis' for being philosophically biased or circular, because it "actually uses the very concepts whose questionable significance provided us with the philosophical problem with which we began. The whole problem was to illuminate what goes on when we 'realize' something in the course of a mathematical proof" (Bloor [1987], p.349).

What all the SSK or knowledge-centred approaches presented up till now seem to have in common, are a number of philosophical predispositions that are anti-positivist or naturalistic in nature: empirical underdeterminacy, interpretative flexibility, theory-ladenness of observation. Below, in section 6, a practice-centred approach will be added to the spectrum that will move philosophical preoccupation (e.g., on the basis of a crafting of divisions) even more to the background, by focusing instead on what, for practitioners, *really* seems to matter: overcoming resistances to the progress of their research.

4. Philosophy of Mathematics Old and New

In order to very roughly reconstruct the recent history of the philosophy of mathematics, a focus on foundational studies largely suffices. To begin with, consider the following nice analogy grasping the nasty circumstances in the philosophy of mathematics, by Noel Curran. "In constructing a building," he writes, "the foundations are laid first and the building is raised on top of that, reaching completion with the roof. In mathematics the procedure appears to be the reverse. There is an immense structure of mathematics built up over the centuries, but the very foundations of mathematics –what it really means– have not yet been provided" (Curran [1997], p.16). The foundational problems in the discipline indeed largely originated from the particular development modern mathematics has gone through. Especially in the course of the 18^{th} century, a lot of spectacular *cum* successful results were arrived at (notably in the calculus), but these were accomplished on an intuitive basis, and it was not until the turn towards the 19^{th} century that scholars came to see the need of securing this vast body of knowledge, of putting it on firm footings. The subsequent search for foundations then reached its height in the first third of the 20^{th} century, and Kurt Gödel famously terminated the crisis in the early nineteen thirties, not by establishing the foundations of mathematics, but by showing the formal impossibility of carrying through this particular task within a reasonably rich system.

Roughly, the sequel of the story has been, however, that mathematicians have largely ignored its purported deep implications, for being too remote from daily practice (where one does not regularly tend to meet undecidable results), while most philosophers disputed its significance, and continued their foundational labor.

When browsing through the early literature on ‘humanistic’ approaches to mathematics (say from the latter half of the former century on), the most striking thing seems to be that nearly all of it is by mathematicians, not philosophers. One could take Raymond Wilder as an exemplar, addressing the International Congress of Mathematicians as early as in 1950, calling upon his colleagues “to get outside mathematics, as it were, in the hope of attaining a new perspective”, i.e. a “vantage point from which one can view such matters more dispassionately” (Wilder [1998], p.186). We can hardly think of any philosopher of mathematics who would have put forward this type of externalistic claims at the time. The authors of the two most ground-breaking monographs of this alternative movement were also professional mathematicians: Kline [1980], declaring foundationalism bankrupt with renewed strength, and Davis/Hersh [1980], challenging philosophical perfectibilism by offering internal perspectives on mathematical practice. These dissident voices were picked up by scholars working in the ‘post-analytic’ philosophy of science in general (particularly in the wake of Thomas Kuhn or W.V.O. Quine). Especially after the publication of the seminal case-study *Proofs and Refutations* by Imre Lakatos, that is since the 1970s, the issue has been taken to heart, though very gradually, by an increasing number of *philosophers* who came to realize that a full understanding of mathematics indeed also involves a grip on mathematical activity itself, as a social process.

This concern with what it *actually* is that mathematicians do when they do mathematics, implicit in Lakatos’ work, for them pointed to new ways of coping with the foundational problem, a problem that, even if officially terminated, was in effect felt to be lingering. Several fresh, *in casu* ‘contextually’ or ‘socio-historically’ coloured, items were thus put on the philosophical agenda, and, in the course of the past decades, came to occupy an ever more prominent place there. Some examples of such newly arising subjects for philosophical inquiry and discussion have been: the status of picture and probabilistic proofs, epistemological consequences attached to the digital revolution (computer proofs), and influences of institutional circumstances such as raging specialization and fragmentation on the trustworthiness of even mathematical knowledge (e.g., feasibility).[8] Today, in philosophy, this field of interest is best characterized, we think, as as being involved with 'mathematical practice', and it indeed appears to be in full development, at least in the sense that it is still mostly separate topics that are being attended to by philosophers active in the domain, and hardly any encompassing theories are attempted at. The most solid encompassive reference remains the timely reader *New Directions in the Philosophy of Mathematics* (1985, 1998^2) by the late Thomas Tymoczko, retaking the Wilder speech referred to above, some papers by Lakatos, a discussion on the status of computer proof, and much more. A more recent exponent of this field is David Corfield, particularly in view of his book *Towards a philosophy of real mathematics* (however, see also this issue).

5. Sociology of Mathematics Ahead

Apart from the Lakatosian quasi-empiricism in philosophy, and apparently largely independent from it, the humanist inspiration set out above has also spread throughout the sociological (or more generally: metascientific) approach to mathematics. This was apparently largely due to the influence of the later Ludwig Wittgenstein (or so a number of major proponents have themselves testified), particularly because of his scepticism about rule-following in the *Philosophical Investigations* (first published in 1953), arguably in combination with the rather sketchy *Remarks on the Foundations of Mathematics* (conceived in the period 1937-1944). However, it will be rather easily seen that there is nowadays no more or less homogeneous field of inquiry (discipline or subdiscipline, however tiny or dispersed) that can be identified or by lack of better identifies itself as "sociology of mathematics"; this arguably in contrast to the "history of mathematics" (which might in its turn be considered as to have largely remained at the level of passtime for mathematicians). As we have seen, there is the field known as SSK, with a few dispersed applications to mathematics, but a systematic study of

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[8] For more details, see, e.g., Van Kerkhove [2005].

mathematical activity does not seem to have been carried out under its wings.

For at least two reasons, this might not be so surprising. First, SSK is itself a relatively, if not very, young field of inquiry (as indeed is sociology simpliciter), still in full conceptual development. Second, mathematics is a peculiar thing, very unlike any other human activity or even science, and thus a sociologist wanting to deal with it must first be occupied with a central question: is mathematics liable to his type of analysis, and if yes: how is this best done? In other words: a methodological framework is in order to avoid that preconceptions are taken into the inquiry. Sociology of mathematics appears to be stuck at this point, and is under serious threat of remaining at the level of *mere* storytelling. Here surely philosophers could prove of great help, particularly the humanistic or naturalistic philosophers of mathematics themselves, whose epistemic theories might more quickly gain ground with constitutive empirical studies properly confirming or indeed falsifying them. "The idea that mathematics, or any other form of knowledge, falls from the sky is quickly fading. But sometimes, as in the case of our ideas about the gods, the difficulty of coming up with a satisfactory alternative explanation keeps the old idea alive. That is the case in mathematical studies today" (Restivo [1993], p.15).

Philosophers indeed have the conceptual apparatus to explore the proper relations between the two fields, viz. those of normative and descriptive theories of mathematics respectively. As with the theory of science in general, the prevailing historical pattern in the relation between those fields seems to be one of alternation between phases of conflict and disinterestedness: now one is reduced to the other (and vice-versa), now both perspectives are proposed as radically different. A third possibility suggests itself though: that both stances interpenetrate, are at most analytically distinguishable, and thus constitute only a gradual difference in perspective. We would largely side with the interpenetration option, without however remaining blind for its inherent limitations. On the one hand, every inquiry of reality contains an empirical component, and there is no reason why the theory of knowledge, even scientific, even mathematical knowledge, should escape this. On the other hand, as was hinted at, neither of the sociological theories, however dispassionate, can be totally free of philosophical preconception; and shouldn't be, for that matter. Clearly, huge interdisciplinary efforts will be required to further sort out the intricacies involved, and perhaps it might even be doubted if the set task is actually a feasible one. Therefore, let us also explore a more radical avenue.

6. One More Turn After the Social Turn?

6.1 Articulation

The French social scientist Bruno Latour has argued that Bloor's strong programme, although having been the pivotal point of progress in social studies of sciences, is currently at a standstill, cornered in what appears to be a blind alley, while meanwhile, researchers in literary theory, biology, cognitive science, cultural history, ethnology, ethnography of skills, moral economics, interactionism and networks theory have been looking for ways out of the deadlock. By being a little more radical, Latour thinks, we could pass to "… a productive and commonsensical research programme that would allow us to capitalize on the last twenty years' work and resume our swift pace" (Latour [1992], p.273). Therefore he advises to take one more turn after the social turn, a turn he claims from which epistemology, ontology, the practice of science and the reflective studies of science will all benefit. Latour notes that both SSK (including Bloor) and its critics have worked within the framework of the object-subject polarity. Now, the attribution of causal agency to any things –Bloor attributes causal agency mainly to the social– is itself an exercise of knowledge that presupposes the division of reality into categories of 'the natural' and 'the social', that is a division between the world in itself on the one hand, and our knowledge of that world on the other. Therefore, Latour argues, Bloor [1991] is itself the high-tide mark of an asymmetrical philosophy.[9]

One may argue whether or not Latour has rightfully understood Bloor's causal presuppositions. Indeed, perhaps Bloor does *not* try to

[9] "Bloor designated Durkheimian social structures to occupy the Sun's focus and gave the name 'symmetry' to the principle that required us to explain successes and failures in the development of science in the same sociological terms" (Latour [1992], p.278).

explain everything (including nature) through a purely subjectivist theory in terms of society. However, the core of Latour's attack remains: Bloor presupposes, or better assumes, an object-subject distinction. The latter even admits this. "Yes," he says, "we make substantial assumptions about what the world is like" (Bloor [1999], p.93). But then again, he is swift to add, we can't do without them! Bloor rightfully argues that the difficulty is to decide which things should be topicalised for investigation and which should be reserved as resources, thereby failing to topicalise the very object-subject distinction. To him, the important point is to "… separate the world from the actor's description of the world. It is the description that is the topic of enquiry, and the proposed separation is one of our resources" (Bloor [1999], p.93). Bloor thinks that it is only by sustaining the distinction between subject and object, that we can highlight the problematic character of our descriptions. Latour, on the other hand, thinks that this object-subject distinction, contrasting the world and its knowers, has done much more wrong than good to scientific practice.

As a direct consequence of the proposed abolishment of the classical object-subject polarity, one no longer stands in need of defining the essence or substance of what the world and our knowledge are like, but "rather an interface [that] becomes more and more describable when it learns to be affected by many more elements" (Latour [1999], p.1). In the case of mathematics, we no longer have to establish what the mathematical world is like, but we are immediately ready to describe the *process* of mathematical knowledge growth, suffice with a *generic* definition of mathematics that becomes more and more articulated as it gets affected by additional elements. The basic assumption is that knowledge grows (essentially, though not exclusively) out of previous knowledge. This definition of what is a construction is not undermining science's claim to truth, its objectivity. It only refers to our slow and progressive access to objectivity, and, for this reason, all the subtle mediations of mathematical practice should be protected and cherished instead of being debunked and epistemologically destroyed.

The word 'construction' is often substituted with the expression 'social construction', meaning that the construction is made of social stuff, a material so light so as to be destroyed by the slightest wind. However, according to the above, the word 'social', no matter how vague, actually refers to the *process* through which all things, including matters of fact, have been built. Like houses, mathematical facts do not fall from the sky. It is this common and collective process to which 'social construction' refers, not to the various materials from which things are made. Like Ludwik Fleck (1896-1961),[10] Latour believes that 'social' is not a quality that destroys or limits the quality of results, but on the contrary authorizes it. Although this might seemingly yield a house of mathematics consisting of solid factual walls instead of fragile scaffolds of social ties, the process leading to it is essentially collective, requiring the complex collaboration of many trades and skills. As soon as the word 'construction' succeeds in gaining some of the metaphorical weight connected to building, e.g., as concrete poured into forms held by scaffolds, it will be clear that it is not the solidity of the resulting construct that is being put into question, that is, not the solidity of the mathematical body of knowledge, but rather the many heterogeneous ingredients, the long process, the many trades, and the subtle coordination necessary to achieve such results.

Consequently, if there is one thing toward which 'making' or 'constructing' does *not* lead, it is to the concept of a human actor fully in command. This is the great paradox of the use of the word 'construction': it is used by critical sociology to show that things are not simply and naturally there, but at the same time that their creators have to share agency with a mass of other actants over which they have neither mastery nor control. Transposing this to our domain of interest: no mathematician is fully in command of the numbers and their laws. Describing mathematics as a similar construction introduces new uncertainties about *what* is to be built ánd *who* is responsible for it. In practical contexts, building, creating, constructing, laboring means to learn how to become sensitive to the contrary requirements, exigencies and pressures of conflicting agencies, with none of those being fully in command. If it is clear, e.g., in the case of architects, that the only real interest is in deciding between good and bad construction, and not between construction and autonomous reality, why should this not be the case for scientific or even mathematical facts? In the practical parlance of

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[10] See his *Entstehung und Entwicklung einer Wissenschaftlichen Tatsache* (Genesis and Development of a Scientific Fact; 1935), first edited in an English translation by Trenn and Merton (Chicago, 1979). See also Cohen/Schnell [1986], a collection of Fleck's papers.

working mathematicians, it is because they work and work well that facts are autonomous and get to stand independently of their own actions. Therefore, if we describe mathematics as a construction, we can make a difference between good and bad mathematics, well designed and badly designed proof, well fabricated and badly fabricated mathematical facts, etc. In the end, Latour delivers a plausible way of saying that because a mathematical fact has been well constructed it is solid, durable, independent, autonomous and necessary. No choice between reality and construction is in order.

6.2. Applications

Ideas like Latour's have given rise to a pragmatical school in the sociology of science, one that drops the priority that is given to theory over observation, and moves action and material equipment centre-stage. A famous philosophical advocate of this view is Ian Hacking (Hacking [1983]). Empirical studies in this field are also known as 'laboratory studies' (e.g., Latour/Woolgar [1986], Knorr-Cetina [1981]). This school holds that the epistemical or SSK school has not been paying enough attention to the influence of practice upon theory. It takes knowledge as manufactured or constructed, but in various ways. First option, ontologically (construction of objects): reality comes into existence together with knowledge. Berger and Luckmann [1966] have defended this on the social level, but laboratory studies transfer this to natural sciences as well, and hold that phenomena of a 'second' nature are produced with instruments.[11] In mathematics, there seems to be no such obvious differentiation to begin with: object and tool, knowledge and thing apparently have much of the same nature already. Representations are certainly not straightforward mirror images but largely exist in their own right, so to speak. Especially in contemporary 'higher' pure mathematics, external reality is hereby relegated to the background more than in any of the other sciences (if not completely).

A recent study by Bettina Heintz, a German sociologist, was largely inspired by this latter school, although in combination with

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[11] Hacking entertains a quite strict conception of 'laboratory', Knorr-Cetina a broader one, including most of natural science, also outside a laboratory context.

another sociological interpretation. We have seen that the strong programme in the sociology of science, devised by Bloor and Barnes, denies that mathematics is epistemically exceptional, but claims it to be liable to sociological scrutiny like any other type of (scientific) knowledge. In the wake of Mannheim's 'weak' programme, the ethnomethodological school of Livingston accepts mathematics' exceptional character, but without apparent proper explanation of its compelling force (which 'emerges'). Heintz [2000] will follow the latter option, in accepting that mathematics is exceptional, thereby however proposing the gap left by Livingston, viz. between local production and apparent universal validity of mathematical knowledge, as hitting an important and sociologically relevant question. In order to answer it, Heintz collected anthropological material in the 'laboratory' sense and interpreted it using the neo-functionalist framework of Niklas Luhmann, a German disciple of Talcott Parsons. In Heintz' resulting socio-historical theory, the major function of mathematical proof is to facilitate proper communication. Formal proof as we today know it, so she argues, is the result of a historical evolution started at the end of the 19th century, when changing circumstances (mathematics turning professional, anonymous, and international) urged for the development of new ways of unambiguous communication, replacing the informal one through letters and social meetings. In this study, we clearly see internalism and externalism go hand in hand, bridging the gap between practice and theory.[12]

There is a second variant of taking knowledge as manufactured, namely in the epistemological sense of construction of facts. Facts, so it is claimed, are established by bringing in harmony several components, through 'resistances' of both theoretical and technological nature. Objectivity is a matter of coherence, to be produced by changing both thinking and handling. But although truth is system-relative, with a constitutive role for the empirical, it is not relativistic, as independent reality is not denied. One of the most important names in this respect, apart from Latour, is Steve Woolgar.[13] Another representative of this

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[12] For a more detailed appreciation of the book, see Van Kerkhove [2004].

[13] For a compact introduction, see Woolgar [1988].

school, Andrew Pickering, has also applied its ideas to mathematics. More particularly, like Bloor (see section 3a), he has considered the case of Hamilton. The difference between both approaches is striking. While Bloor tells the story of Hamiltionian mathematics (implicitly including that of quaternions) from a mainly political and metaphysical point of view, Pickering approaches the nascency of quaternions primarily in technical rather than social terms, viz. in terms of disciplinary agency, mathematical inquiry mainly proceeding through internal conceptual resistances. For Pickering, it was indeed the latter that shaped Hamilton's metaphysical wanderings, and not vice-versa. In other words: mathematical practice *stricto sensu* was primary.

7. Conclusion

In this paper, we have aimed at sketching a vast array of externalist or sociological, or 'other than' internalist or metamathematical, reflective accounts of mathematics. While the sociological approach to science is about as old as sociology proper, its philosophical endorsement, even at the outset of the 21st century, remains a fragmented affair. *A fortiori*, in the case of mathematics, there is no homogeneous field of inquiry where the sociological and philosophical perspectives are encouraged or even allowed to interpenetrate. On the contrary, while perhaps having become ever more mutually tolerant, both continue to largely disregard each other's approach: sociologists restricting attention to the descriptive, and philosophers to the justificatory side of the scientific coin. In the wake of a broadly Latourian approach, recent interdisciplinary efforts such as that by Heintz [2000] therefore have great appeal to us, containing insightful chapters on the sociology of knowledge ánd the philosophy of science, to proceed to an informative socio-historical theory of mathematical proof. In studies like this one, we see a first glimpse of the rich possibilities of mathematical internalism and externalism going hand in hand.

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