Mathematical Practice and Naturalist Epistemology: Structures with Potential for Interaction

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Abstract: In current philosophical research, there is a rather one-sided focus on the foundations of proof. A full picture of mathematical practice should however additionally involve considerations about various methodological aspects. A number of these is identified, from large-scale to small-scale ones. After that, naturalism, a philosophical school concerned with scientific practice, is looked at, as far as the translations of its epistemic principles to mathematics is concerned. Finally, we call for intensifying the interaction between both dimensions of practice and epistemology.

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1 Introduction

If it is one's intention, as it is ours, to take mathematical practice seriously, then all of its dimensions, or at least as many as reasonably possible, should be attended to. In current philosophical research, however, there is a rather one-sided focus on the foundations of proof, i.e. the prime *result* of mathematical endeavour. A full picture of mathematical practice should however additionally involve considerations about various *methodological aspects* presumably influencing this result. In section 2, which is rather descriptive in nature, a number of these aspects is identified, leading us from large-scale topics, such as standards of proof, to small-scale ones, concerning the particular tools used by the individual, working mathematician. Ample reference is made to specific cases. As naturalism is a philosophical school from which real concern for scientific practice should be expected, it seems instructive to have a deeper look into translations of its principles in mathematical terms. Consequently, in section 3, a tentative theoretical framework is established for interrelating various of its versions in terms of their epistemological concerns. In the concluding section 4, we suggest to finally get the interaction going between both dimensions of practice and epistemology. Globally, we seek to contribute, however modestly, to the instauration of philosophical mechanisms that lead all the way from mathematical practice to philosophy and back, answering *Hao Wang*'s dramatic appeal, many years ago:

> Foundational studies in this [twentieth] century have been very fruitful in several ways. [...] On the whole, there remains, however, the impression that foundational problems are somewhat divorced from the main stream of mathematics and the natural sciences. [...] The principal source of detachment [...] implies a neglect of mathematics as a human activity, [...]. In a deeper sense, what is more basic is not the concept of set but rather the existing body of mathematics. [...] Rightly or wrongly, one wishes for a type of foundational studies which would have deeper and more beneficial effects on pedagogy and research in mathematics and the sciences. [Wang 1974, 242–243]

2 Structuring Mathematical Practice

We start our observation of mathematical practice at the most general level: that of the entire mathematical community, the notion of which has clearly been evolving over time (2.1). Subsequently, we move down a level to consider the role of research traditions and projects (2.2). Finally, the individual core business of mathematics is highlighted: constructing proof. In this section, which is rather descriptive in nature, we are mainly concerned with raising questions about some standard views of mathematics as practice. We advise the complementary lecture of [Van Bendegem 1999] to this section.

2.1 Macro-level: The Community

The most dramatic shift in the large-scale history of mathematics arguably took place in the seventeenth century. After a relatively tranquil transition period of several centuries, it marked the birth of modern mathematics, with major advances in the fields of analytic and projective geometry, calculus, combinatorial analysis, higher arithmetic, dynamics, and symbolic logic [Bell 1992, ch.7]. Coinciding with these developments, a *professionalization* of the discipline took place. "The seventeenth century witnesses the beginnings of the demise of the amateur mathematicians. Between 1645 and 1715, the role of the mathematical practitioner, a precursor of the professional mathematician, consolidated. [...] By 1910, under the influence of the advances in pure mathematics, mathematics had become a profession; the audience for and beneficiaries of mathematical research and teaching were now the mathematicians themselves" [Resnik 1992, 149 and 143]. Communication is the key word here. "Up to about 1550, mathematics was created by individuals or small groups headed by one or two prominent leaders. The results were communicated orally and occasionally written up in texts – which, however, were manuscripts" [Kline 1990, 396]. But in the seventeenth and eighteenth centuries, all over Europe, societies and academies flourished. Due to their organizing meetings and publishing bulletins, contacts among mathematicians, hitherto mainly restricted to the exchange of letters, boosted.

From the nineteenth century onwards, another dimension was added: that of *internationalization*. "Mathematics in national contexts share educational experiences and, hence, research goals and agendas. As mathematics moves beyond national boundaries, these goals and agendas become more universally held. The subject matter — the language of mathematics — comes to unite mathematicians regardless of their national loyalties; the subject matter becomes supranational; it transcends national boundaries altogether" [Parshall and Rice 2002, 10]. Steadily, through international congresses and journals, the distinct local mathematical discourses converged into a single or universal one. The reason for the relative ease of this kind of integration in pure mathematics, despite its highly abstract nature and the clear lack of joint empirical testing, lies in the growing status of formal proof itself, as the medium through which unambiguous communication is secured. In professional and international science, the saveguards of trustworthiness are no longer personal confidence or social status, but the following of *procedures*. In mathematics, this methodological objectivity has come in the shape of standards of rigour [Heintz 2000]; [Van Kerkhove 2004]. Compare also: "Abstraction *depends on* realizing opportunities for producing, publishing, and disseminating ideas in a specialized community of teachers and students that extends over a number of continuous generations" [Restivo 1992, 171] (our emphasis).

Two brief conclusions. First, it seems that the 'universal' language, thus the *content*, of mathematics might not have come out of the blue after all, but (at least) partly got shaped by large-scale sociological developments. Second, the influence of these emerging 'universal' constraints on local mathematical endeavour is evident. For if he is to be taken seriously as a professional, the individual researcher will have no choice but to apply the standards set by the international community. "Early in the nineteenth century, to be considered a research mathematician, it was enough that one had sufficient interest in the subject to write (but not necessarily to publish) books, papers, or articles on the subject. By the twentieth century, however, publication had become a *sine qua non*" [Parshall and Rice 2002, 8n].

2.2 Meso-level: Traditions and Projects

The flight of the sciences, from the seventeenth century onwards, has actually resulted in tens of thousands of professional mathematicians [1] active in dozens of specialties. As [Jaffe and Quinn 1993, 2] would have it, "mathematics is [even] much more finely subdivided into subdisciplines than physics". Poincaré and Hilbert arguably having been the last of generalists, no single scholar is any longer able to survey the whole field. This means that we should be able to discern some *intermediate* level

1. To give an indication, the AMS currently has more than 30.000 members

where theoretical links can be established between the large-scale structure of the mathematical enterprise (2.1) and everyday practice (2.3). One possible perspective is that of [Koetsier 1991], which extends (but also refines and adjusts) the model proposed by [Lakatos 1976]. As it is clear that young mathematicians do not just enter the immense field of inquiry, but a particular segment of it, some additional structure is needed on top of Lakatos's (and Pólya's) low-level heuristics of proving and refuting. To this end, Koetsier identifies research traditions and projects, where a tradition carries *general* assumptions about the appropriate objects and methods of study, while a project sets a *specific* agenda of goals and tools.

Since [Kuhn 1962], the mere mentioning of research traditions (or paradigms, or the like) invariably raises questions about alleged revolutions. The nice thing about Koetsier's approach is that it remains impartial on this issue, as progress as such is its crucial point, whether revolutionary or not. [Gillies 1992] remains the standard collection of elementary papers on this topic. In his book, Koetsier has himself worked out some historical case-studies on (transitions between) research traditions, both from ancient Greek and modern mathematics. Alternative proposals are available though, *e.g.* in terms of structures [Corry 1992] or polarities [Duda 1997]. Examples of mathematical research projects are the *Erlanger Programme*, seeking to organize the various geometries in terms of groups of symmetries, the *Langlands Programme*, aiming at "a synthesis of several important themes in classical number theory" [Gelbart 1984, 178], *Hilbert's Programme*, outlined in his famous speech before the International Congress of Mathematicians [Hilbert 2002], *Category Theory*,[2] and the *Classification Theorem for the Finite Simple Groups*, initiated by *Otto Hölder* in 1892 and now spread over about 15.000 journal pages. [Solomon 2001] offers a recent and concise round-up of this ample history. In the next subsection, we shall hint at some of the methodological influences of research projects.

2.3 Micro-level: Constructing Proof

On the basic research level, individuals are committed to conjecturing and, especially, to proving (or disproving) specific mathematical statements. Traditionally, a mathematical proof is considered to be a written down sequence of symbol strings, each one deduced from (one or more of) the former, in application of one of a limited list of specific rules.

2. For discussion, see *e.g., Philosophia Mathematica (III)* 2(1), 1994.

The initial, non-deduced strings are called axioms, premises, or definitions, while the final ones constitute 'fresh' theorems. This, however, is a strictly *formal* notion of proof, viz. as an end result. In order to obtain a more complete picture of what proof is *really* like, it seems desirable to also attend to the informal process of its construction and evaluation as well. It might be instructive, for one, to assess the role and impact of the *tools* mathematicians use in order to find a proof, *e.g.* metaphors and analogies [Van Bendegem 2000], pseudo-proofs, or computer graphics and calculations. In the latter field, the proof of the *Four-Colour Theorem*, by Appel and Haken, has triggered intensive philosophical controverse. For a technically refined historical appraisal, see [Fritsch and Fritsch 1998]. Other types of questions concern the development and functioning of *proof methods* (infinite descent, proof by cases, visual reasoning, experiment [Van Bendegem 1998], induction, ...) as well as the *evaluation criteria* for recognizing a 'good' proof (simplicity, elegance, explanatory value, beauty, depth, importance, relevance, ...).

In the same respect, further points of interest are *specialization* and *division of labour*. In an introductory article on *Fermat's Last Theorem*, at a moment *Andrew Wiles* had not yet released his manuscript (ironically, a serious gap had been identified only the month before by one of the referees, *Nick Katz*), one reads, quite reassuringly: "Most experts continue to believe in the fundamental correctness of the proof" [Cox 1994, 13]. It is interesting to know, however, that this particular 'expertise' was shared by a dozen of highly specialized scholars at most, and this situation, it should be clear, leaves substantial room for error. Moreover, nowadays ample use is being made of mathematical techniques for cutting down the size of formal proofs, in order to improve their readability. The dropping of steps is often justified in view of *splicing* because of extensive argument elsewhere, and *skipping* because of intuitive obviousness [Davis 1972]. In the latter case, clearly, no formal argument is available, as grasping the move is supposed to be an irreducible feature of the particular community's membership. A special case of splicing is the division of labour, geographically as well as in time, and either intentionally or not, when constructing proof. Proving Fermat, for instance, has not been the work of just one man (although he indeed did the substantial final efforts), but of several generations of mathematicians spread all over the world and in a variety of subdisciplines. See [Cox 1994] for a brief and accessible overview. The same goes for *Goldbach's Conjecture*, as yet unsolved [Cheng Don and Cheng Biao 1992] and for the Finite Simple Groups Theorem (see 2.2).

Summing up, a lot of what has been touched upon here might be fundamentally at odds with what is traditionally required of a proof, namely that "there must be final contact which lights up the whole thing", and that "only a man can establish this contact by taking in the whole process that makes up the calculation or the proof" [Wang 1974, 230]. The upshot of the present discussion could then sound thus: "As a definition of the word 'mathematics', perhaps the proof is indeed dead, or dying. In which case, I would proclaim: Long live the proof as an important part of a broad spread of mathematical activity" [Devlin 1993].

3 Structuring Naturalist Epistemology

We begin the markedly philosophical section of this paper, in 3.1, by giving a general assessment of naturalism, its main variants, and some objections directed against it. On the basis of that, we then devote ourselves, in 3.2, to the specific case of naturalism as a philosophy of mathematics. For the latter, we have in part drawn on [Van Kerkhove 2002], where further details and references can be found.

3.1 Internalism and Externalism

Naturalism is most widely known as the school of artistic expression entertaining a fatalistic conception of existence, and consequently concerning itself with depicting 'harsh' social reality. The encompassing metaphysical doctrine, which influenced by the ascent of (human) science reached the height of its popularity in the nineteenth century, states that *all* things and events, particularly human affairs, are to be understood in terms of natural causes and effects. The consequences for epistemology were drawn under the impulse of *W.V.O. Quine* and *T.S. Kuhn* in the mid twentieth century. They amount to the empirical nature of the question as to how, if at all, genuine knowledge is (to be) arrived at. "Questions about how we actually arrive at our beliefs are thus relevant to questions about how we ought to arrive at our beliefs. Descriptive questions about belief acquisition have an important bearing on normative questions about belief acquisition" [Kornblith 1997, 3]. As, according to this school, scientific epistemology is rooted in scientific practice, consequently, it is not up to *a priori* philosophy, but to science itself to determine what its object ('reality') is like and how it should best be approached. This has brought about nothing short of a revolution in the modern agendas of philosophy of science:

> The new naturalisms [...] shift epistemology away from idealized abstraction to establish connections with epistemic practice that could enable theories of knowledge to engage constructively and critically with everyday cognitive activities. Neither committed to analyzing what ideal knowers ought to do nor constrained to devoting their best efforts in silencing the sceptic, naturalists assume that knowledge is possible and seek to understand its real-world (natural) conditions. They abandon any quest for a priori, necessary and sufficient conditions for knowledge in general, to examine how epistemic agents actually produce knowledge, variously, within the scope and limits of human cognitive powers. [Code 1996, 1]

The epistemological naturalist is challenged to cope with two prominent ambiguities without taking recourse to a form of monism and/or reductionism. One is the age-old problem of justifying the is-ought or fact-value derivation, widely known and referred to as the *naturalistic fallacy*, the original formulation of which is generally attributed to the empiricist *David Hume*. The other is that of establishing methodological criteria in the face of the non-monolitic character of modern science, an issue embedded in that of the emergence, in society at large, of numerous independent and apparently incommensurable spheres of interest, resulting in what is presently called our *postmodern condition*.

In order to be able to distinguish more clearly between some differing stances, within naturalism, towards these central problems, we shall exploit an epistemological antagonism that traditionally hinges on the justificatory privileges of the *individual*, and apply it here, in a metascientific context, to that of the individual *discipline*: that between internalism and externalism. In this way, full blooded *internalist* epistemological naturalism can be defined as proclaiming a discipline's methodological independence over any external influences, philosophical or scientific. It has a strong objectivist gist, severely contrasting the contexts of discovery and justification, and discarding the former while claiming exclusivity over the latter. The complementary task left to philosophy of science is the providing with "a pleasing gloss on the history and discovery of sciences. But we should not expect it to provide today's scientists with any useful guidance about how to go about their work or about what they are likely to find" [Weinberg 1993, 133]. Adding an *externalist* dimension is then acknowledging science as essentially a human enterprise, which should be open to examination of its practices, thereby using the specific methodological tools developed to that end: those of history, sociology, or psychology.

The movement of 'naturalized epistemology' has mainly been identified with Quine's condemnation of *a priori* first philosophy as it had been reigning (the philosophy of) science since *Descartes*. However, Quine's theses in this respect neither allow for an unequivocal reading, especially with regard to descriptive or prescriptive tendencies, nor exhaust the possible interpretations of the naturalist principle. Despite the instauration of epistemology as a branch of psychology, it is only fair to say that Quine's holism retains a traditional reductionist flavor, in holding that reliable scientific knowledge is best grasped in a concentric model the quasi-untouchable hard core of which is formed by the exact sciences, mathematics and logic in front.

Hence the importance of acknowledging as naturalist also another current sweeping through postwar philosophy of science, viz. in the wake of [Kuhn 1962]. In this inspiration, the perfectibilist picture of science associated with logical positivism, viz. as a unitary and linear endeavour, as well as strictly normative top-down accounts, decreed by 'unworldly' armchair philosophers, got heavily challenged. Instead all sorts of perspectives on actual science, 'as it is done', were promoted, on the common assumption that first comes practice, and only then there is proper material for reflection. According to radical versions of the externalist tendency, as in the internalist case, philosophical labour is considered vacuous. The result is now a kind of relativist account, where things are as they happen to be (though they could have been different), and all left to do is to put these facts on record. There is thus no proper function for regulative principles of any sort.

Let us now turn back to the ambiguities introduced above. Hume's problem of the difference in character, hence impossible transition, between description and prescription is solved, by exclusive internalists and externalists alike, simply by denying room to any normative dimension whatsoever. Philosophy is not welcomed here, as it is thought of as irrelevant at best, but more likely far worse than that: confusing, misguided, thus harmful. Only moderate naturalists seem prepared to bite the bullet on this point, allowing philosophers a backward mediating role between science studies and its subjects, viz. opening up the metascientific material for them. They are convinced that "evaluation and description are interwoven and interdependent" [Putnam 2002, 3], and their strategies of coping with the naturalistic fallacy, both more delicate and vulnerable than absolutist ones, involve calling into question the genuine character of the dichotomy to begin with. We shall implicitly comment on the second problem, that of disciplinary fragmentation, in 3.2, from within the particular context of mathematics.

3.2 Faces of Mathematical Naturalism

In view of the intra-naturalistic distinction drawn in 3.1, it has become clear that we roughly consider internalism as a Quinean legacy, and externalism as a Kuhnian one. We are now in a comfortable position to have a brief look at some particular naturalist accounts of mathematics along the lines of this distinction, the severeness of which is intended to have but analytical purposes.

Quine famously argued "that even mathematical principles, which by most accounts are just as unfalsifiable and devoid of empirical content as the law of inertia, share in the empirical content of systems of hypotheses containing them" [Resnik 1998, 229]. But despite tensions in his framework, in reality, Quine was unprepared to abandon logic and the mathematical foundations reducible to it. It was in view of these tensions, resulting from Quine's "convert[ing] philosophical questions into scientific ones" [Maddy 1997, 177], that *Penelope Maddy* developed a radicalized version of naturalism, to be catalogued here under the heading 'internalism': "Mathematical methodology is properly assessed and evaluated, defended or criticized, on mathematical, not philosophical (or any other extra-mathematical) grounds" [Maddy 1998, 164]. As the latter constraints include natural science, there is even no room here for criteria with a bearing on the applicability or usability of pure mathematics that is being developed. Despite all this, Maddy does not abandon the foundational idea, and thus, as a complement of the 'voidness' of epistemology, she also defends the embodiment of actual foundational consensus in formal, viz. set-theoretical, terms [Maddy 1997], thus simply equating the naturalist 'ought' with 'is'.

Other authors have stayed more closely to the original Quinean ideas. *Michael Resnik* mediates somewhat between Maddy and Quine, holding that "scientists working in a given context assume that they may take large blocks of theory for granted. [...] By appealing to this practice we may roughly rank the sciences in terms of their scopes and methodologies as more or less global [...]. Mathematics is our most global theory as it is presupposed by physics, which in turn is presupposed by chemistry, etc. [3] [...] Corresponding to this rough division of the sciences has developed a division of labour: mathematicians normally do not meddle in physics

3. *I.e.*, Quine's reductionism in disguise, referred to in 3.1.

nor physicists in mathematics, [...]" [Resnik 1998, 238]. Compared to the inherent but elusive holism of both Quine and Resnik, *Nicolas D. Goodman* has opted for a more progressive position, in putting mathematics unambiguously on a par with the other natural, viz. *a posteriori*, sciences: "We should try to show that mathematics is as well-founded as the most secure parts of physics, but not that it is better founded than physics" [Goodman 1990, 185]. He gives the example of "[o]ur confidence in the reliability results of computations done by computer [which] rests on our confidence in the physical principles underlying the design of the computer. It is very odd to say that the results of those computations are known a priori" [op.cit., 189-90]. From here, foundations can be considered turning (at least in some sense) 'relative'. "Most classical mathematicians view logic as a theoretically neutral framework. [...] In the last few years, however, even this consensus has shown signs of breaking down as mathematicians have come to see that the choice of a logical framework is simply another part of theory construction" [op.cit., 185].

Turning from internalists to externalists, Kuhn, who discerned in the history of science a pattern of successive normal periods, with revolutionary ones engendering paradigm shifts in between, was however inclined (just like Quine) to grant mathematics an exceptional status, and to see its history as a series of cumulative results, with only temporary disagreement in between. It was on this point, among others, that *Imre Lakatos* departed from Kuhn, holding that mathematics *is* a science like all the others after all. As a result, mathematically, Kuhn's legacy can be rightfully renamed Lakatos's, for frankly tranferring the general idea common to Quine and Kuhn, viz. the fundamental revisability (and historicity) of science, to mathematics. In the decades since, [Lakatos 1976] has been the source of inspiration for a wide variety of approaches granted here the label 'externalist'. In concreto, this means that constraints can be laid upon mathematics, *e.g.*, that it should be succesfully applicable elsewhere, or that it should not contradict otherwise accepted and empirically verified results. Just above, we have pointed to a (varying) sensitivity within the internalist tradition on this point, hence the gradual conception of the dichotomy. It also means that all of the spatio-temporal circumstances that could have a bearing on the investigative practice of the mathematician, thus contributing to setting the academic agenda in one way or another, are considered as potentially relevant. This dimension, viz. the possible importance of historical, sociological and psychological aspects, does constitute a main point of difference with Quinean approaches. What gets added here is a pragmatical concern with the context of (philosophical) problems, with their embeddedness in a larger setting, *e.g.* of human relations, rather than just with their internal or formal dynamics.

Without claiming exhaustiveness, attempts at this type of 'humanistic' theories of mathematics, usually coming with a strong socio-historical import, have been undertaken from within

- *neofunctionalism*, reconstructing the development of the specific epistemic structure of mathematics, viz. towards rigourous proof as a symbolic 'generalized' communication means [Heintz 2000], [Van Kerkhove 2004];
- *semiotics*, analyzing mathematics as an essentially written – not spoken – system of man made, interpreted, exchanged and adjusted signs [Rotman 2000], [Otte 1999];
- *ethnomethodology*, urging an acknowledgement of the (multicultural) diversity of mathematical expression, thereby widening the history of mathematics from a predominantly western elitist and academic one to one processing the daily and local social foundations of mathematical precision [Livingston 1986], [Ascher 1998], [Ascher 2002];
- *cognitive science*, focusing on elementary operations, trying to establish whether (these) mathematical capacities are innate or rather learnt, and whether or not mathematics is an exclusively human affair [Dehaene 1998], [Butterworth 2000];
- *strong sociology*, the externalist counterpart of Maddy-ism, where 'ought' is equally anathema, and what thus remains are various actual and potential mathematical practices, without a possibility of finding any common regulative principles or foundation [Bloor 1991].

4 On the Interaction between Mathematical Practice and Theory

In this final section, we give a brief assessment of the philosophical traditions distinguished in 3.2, and hint at their benificiary reconciliation.

On the whole, the externalist approaches touched upon above are typified by a lack of mutual coherence and *epistemological depth.* For sure, Lakatos's work is a common and even explicit source of inspiration, but epistemologically, the ground laid by him has only barely been cultivated. We are apparently still in the era of letting a thousand flowers bloom, as there is an increasing supply of daring deconstructions and analyses of actual mathematical knowledge, whereas meticulous theoretical reconstructions of the resulting fragments seem to be largely lacking.[4] We contend that this is a transitional circumstance, that will be remedied by consecrating persistent effort to the cause. As the *phenomenological* material presented in section 2 has shown, mathematics actually *is* much richer and varied than, thus differs thoroughly from, the idealized accounts of it by foundationalists. Consequently, internalists are invited to no longer simply brush aside externalist considerations, and strive to reverse their moderate *epistemological breadth.*

As suggested above, in 3.1, the 'family resemblance' between internalists and externalists can indeed be recast in *Hans Reichenbach*'s terms of *epistemological focus*: while the interest of the former is rather limited to the markedly formal, individual and normative *context of justification*, the latter are busy developing various descriptive perspectives on the largely informal and social *context of discovery.* This artificial distinction has been wrongfully objectified, in mathematics as in general, with dramatic consequences. Despite the implicit recognition that a formal proof "does not dispose of the creative element in mathematics, which [...] belongs to a domain in which no very general rules can be given; experiment, analogy, and constructive intuition play their part here" [Courant and Robbins 1996, 15], only rather isolated attempts at an exploration of mathematical discovery have been undertaken, especially by *George Pólya* (see, *e.g.*, [Polya 1973]). As this domain seems to have been largely neglected in foundational studies, efforts to better understand the ways of cognition are up to an urgent revaluation. Hand in hand with it go questions about the proper function of foundations as instruments for *practical use*, i.e. the output or feedback component to the philosophical machinery called for by Wang (see section 1).[5]

But then, conversely, it should be equally clear that to any 'externalist' unwilling to sink into scepticist quicksand, sound formal foundations are of vital importance, even when accessible only by descriptive means. "Just as in science, the factors that determine which areas of pure mathematics are of interest seem to be of an essentially pragmatic nature. In

4. We owe *Jaakko Hintikka* and *José Ferreiros* here, for their remarks in this connection at the occasion of our lecture at PILM2002.

5. We are grateful to *Lawrence Stout* for pointing this out in his talk at the same conference.

fact, the pragmatic factors often come indirectly to mathematics *through* science. [...] But again, as in science, once pragmatics fixes an area, it makes sense to speak of non-pragmatic epistemological principles. More specifically, once an area is fixed, mathematicians seek to develop, in the loosely specified area, formal theories that have consistent and manageable foundations, [...]" [Peressini 1999, 263]. In effect, some 'social constructivist' exercises have already surfaced tentatively, *e.g.* [Koetsier 1991] or [Ernest 1998], but the scope of their methodologies has been widely called into question. If it is indeed the case that similar efforts have still a long way to go when it comes to the devising of proper foundational systems, it seems only natural to suggest that at least there is a great deal to learn from more internalist or foundationalist oriented ones.

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