Revolutions in mathematics. More than thirty years after Crowe's "Ten Laws". A new interpretation

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1 Introduction

The subject of this paper is whether or not revolutions occur in mathematics. We do not attempt to resolve this problem by arguing for or against the existence of revolutions in mathematics. We want to look at the discussions of the past thirty years from a meta-level. Furthermore, instead of settling the discussion, we want to show what the useful aspects are that these discussions have produced to enrich a study of mathematical practices. We are convinced that research on mathematical revolutions does indeed contribute to such a study.

The paper consists of two major parts. First, we shall give an overview of the discussion about mathematical revolutions and its results. Second, we shall present the way in which Crowe can be fruitfully used by a rather bold interpretation as a basis and framework for a study of the evolving practice of mathematics in all its variety.

2 Overview

Following Kuhn's seminal work on scientific revolutions, attempts have been made to apply the Kuhnian framework to mathematics. The debate within the community of mathematicians is even more passionate and tumultuous than within the sciences in general. One could say that Kuhn himself has unchained some kind of revolution within the philosophy of science by putting an end to the classical image of the continuous, linear and steady growth of sciences. After Kuhn, the image of growth of sciences has obtained a more humane face. And of course this didn't come smoothly. The community of philosophers and historians of science appeared to be at least touched by it. The influence of *The Structure of Scientific Revolutions* lingered until

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many years later after its publication, in the subjects of symposia and on postscripts (Kuhn, 1970).

Within the community of mathematicians, philosophers and historians of mathematics, the debate is at least as tumultuous and is characterised by explicit oppositions. Most diverging opinions exist about the so-called revolutions within mathematics, ranging from philosophers who construct their reasoning and their definition of a revolution in such a way that mathematics can escape from a revolutionary structure, to philosophers who cite numerous facts from the history of mathematics (following Thomas Kuhn) to demonstrate that mathematics does not differ from the revolutionary nature that characterises the sciences. Kuhn himself did not really address the history of mathematics and those that do may argue that the history of mathematics indeed has experienced revolutions. The extreme positions within the debate are represented by respectively Michael Crowe's "*Ten 'laws' concerning patterns of change in the history of mathematics*" (1992) and by Joseph Dauben's "*Conceptual revolutions and the history of mathematics. Two studies in the growth of knowledge*" (1992b).[1] Dauben is one of the philosophers who discussed the history of mathematics. He holds that there are transitions in mathematical development that are 'critical' or 'discontinuous' enough in order to become worthy of the label of 'revolution'. Dauben holds that revolutions within mathematics cannot be 'Kuhnian' in nature. The revolutionary quality of Cantorian set theory, non-Euclidean geometries or Einstein's Relativity Theory, for example, must be found in another explanation that differs from the normal science-anomaly-revolution scheme that typifies a Kuhnian revolution. Cantor's creation of transfinite numbers transformed mathematics by enlarging its domain from finite to infinite numbers and the conceptual step from transfinite sets to transfinite numbers represents a shift or a transformation that revolutionized mathematics (Dauben, 1992b, p. 57). Non-Euclidean geometries did indeed not as much as replace the Euclidean geometry and Einstein's Relativity Theory did not replace Newton's mechanics, but they did affect, surpass and relegate the domain of mathematics considerably (Van Kerkhove, 2005, p. 211).

The discussion on the revolutionary nature of mathematics is studied by Caroline Dunmore—as part of her Ph.D. thesis—which resulted in the argumentation that revolutions may occur in meta-mathematics, but not in mathematics proper. Mathematics is conservative on the object-level and revolutionary on the meta-level (Dunmore, 1992, p. 211).[2] One could argue that this distinction between meta-mathematics and mathematics proper is

[1] Cf. (Dauben, 1992a, p. 206) and (Van Bendegem, 2004, p. 230).

[2] Caroline Dunmore finished her Ph.D. thesis at King's College London by June 1987. Her supervisor was Donald Gillies, the editor of the collection on revolutions in mathematics (Gillies, 1992).

not really satisfying for the question of whether revolutions occur in mathematics. How could a meaningful revolution occur in meta-mathematics that does not in some way refer to or affect mathematics? Dauben (1992a,b) argues that the introduction of transfinite set theory transformed much more than the meta-mathematical level. The same can be said about the introduction of complex numbers, Non-Euclidean geometry, the discovery of incommensurable magnitudes by the Greeks, and so on. In all of these cases, there were certainly meta-level changes that were revolutionary, but this affected both the practice of mathematicians and the content of mathematics.

If today we would be surprised by a denial of scientific revolutions within the growth of sciences, within mathematics this denial still appears to be obvious. Mathematics has traditionally been considered as the science (if a science at all) that can attribute to itself the status of generating absolute certainties, resulting in the idea that what once has been proven, will remain so until eternity (Pourciau, 2000, pp. 297–298; Van Kerkhove, 2005, p. 210).

During the nineteenth and a substantial part of the twentieth century, mathematics was considered as the science where revolutions never occur; in so far it was considered as a science at all. Mathematics was taken to be the science that only accumulated positive knowledge without revolutionary transitions and without rejection of the knowledge that was based on old structures. The house of cards that mathematicians are building and forever extending can, apparently, never be rebuilt. These ideas were expressed by among others the German mathematician and historian of mathematics Hermann Hankel in 1871, by Claude Bernard in 1927, by George David Birkhoff in 1934 and by Clifford Truesdell in 1968 (Dauben, 1992a, pp. 205–206). As a consequence, Crowe must protect the house of cards by developing a system in which he himself describes the notion 'revolution' in such a restrictive way that it is not applicable to mathematics. In his definition, an essential property of a revolution (in general) is the fact that existing entities (be it a king, a constitution, a theory, a concept or a mathematical structure) are knocked over, pulled down, and incontrovertibly renounced. The tenth law indeed states that '*Revolutions never occur in mathematics*' (Crowe, 1992, p. 19).

When Cohen started writing his famous book *Revolution in Science* (Cohen, 1985) he could not neglect the case of mathematics since he was not convinced by the statement that revolutions never occur in mathematics. Some examples from the history of mathematics (e.g. Descartes and Cantor) were fruitful examples to explore the discussions concerning whether or not revolutions in mathematics occur. For Cohen, the denial of the occurrence of revolutions in mathematics is one of the reasons why he considers the topic of revolutions in mathematics in his *Revolution in Science* (Cohen, 1985).

> The topic of revolutions in mathematics must, however, be considered here because a number of mathematicians and historians of mathematics have denied that revolutions can ever occur in mathematics (Cohen, 1985, p. 489).

It is not the purpose in his article to try to settle the question of whether or not revolutions have actually occurred in mathematics. However it is of great interest to explore the ways in which some major innovations in mathematics have been considered as revolutions. In his *Revolution in Science* revolutions in mathematics are not generally discussed, although some attention is paid to the Cartesian revolution in mathematics (Cohen, 1985, pp. 505–507) and mention is made of the revolutionary aspects of the nineteenth century probability theory and statistics and the radically new set theory of Georg Cantor (Cohen, 1985, pp. 319–324). Cohen mentioned the unpublished lecture notes on history of mathematics from 1982 by Hawkins who states that revolutions in mathematics occurring when the method of solving mathematical problems are radically changed on a large scale. In this sense, a revolution occurred in the seventeenth century with Descartes as the central figure in initiating this evolution in mathematics. Hawkins describes—with respect to content and influence—Descartes's *Geometry* (1637) as the major work in the transition from ancient to modern mathematics. Indeed, the work of Descartes did not involve a 'rejection' of ancient mathematics (in the sense that for example Euclid's *Elements* were declared false) but his work did involve a rejection of the methods by which ancients solved problems. Descartes—and his contemporaries—introduced new methods. Mathematical problems should be reduced to the symbolic form of equations and the equations should be used to effect the resolution. Hence, the introduction of these new methods altered the nature of the problems posed and ultimately radically altered the scope and content of mathematics. This sounds indeed revolutionary.

Cohen mentioned other philosophers, mathematicians and historians of mathematics who discerned revolutions in mathematics, e.g. Fontenelle (the first author who applied the word revolution to the history of mathematics), Kant, Cantor, Bell, Kline, Dyson, Mandelbrot, Dauben, among others. As there is a difference among mathematicians concerning the occurrence of revolutions in the domain of mathematics, Cohen mentioned also the work of Fourier, Hankel, Claude Bernard, Truesdell, Boyer, Raymond Wilder and Crowe who stated that in mathematics there has never yet been a revolution in mathematics.

For Donald Gillies (1992) who situates himself rather in the direction of Dauben, the whole debate turns around a semantic discussion wherein the meaning of the notion 'revolution' is obviously crucial. 'Revolution' is a concept that belongs to the framework of political theory and therefore it is used in sciences and mathematics as a metaphor. As a consequence, the debate on the existence of revolutions is a semantic discussion that slides away from its original theme (Corry, 1993, p. 95). Not only the concept 'revolution' is undetermined, even the concept 'mathematics' is within the discussion subject to fluctuations (McCleary, 1993, p. 995). Even more, next to the fluctuations in the interpretation of what is the object of mathematics and what belongs to the meta-level, there is the discussion about which mathematical paradigm applies. According to Pourciau (2000), the whole debate takes place within a specific particular paradigm, which he calls the "classical paradigm". The classical paradigm determines which mathematical assumptions are true or false and by doing so logical inconsistencies are excluded (Pourciau, 2000, p. 300). Within the classical paradigm, the discussion then takes place at the semantic level. A good example within this discussion is of course the definition of revolution itself by Crowe, as cited above. However, Crowe makes the following remark on the phenomenon of non-Euclidean geometry: this would certainly have resulted in a revolutionary change in views as to the nature of mathematics, which meant a revolution within the 'philosophy of mathematics'. Later on, Dummore will use precisely this argument to situate revolutions at the level of values related to the nature of the mathematical objects. Crowe will nevertheless stick to the fact that this is no revolution 'within' mathematics itself (Dauben, 1992a, p. 207). Joseph Dauben, on the other hand, explores the history of mathematics to demonstrate where revolutions actually do occur within mathematics. He has studied in depth a number of historical developments and states clearly that he is a defender of the idea that revolutions do exist within mathematics.[3]

> To the question of whether or not revolutions occur in mathematics, my answer is an emphatic "yes". (Dauben, 1992a, p. 229).

Finally, there is Pourciau (2000) who believes that the discussion between Crowe and Dauben on the notion of revolution, only existed at the semantic level. Crowe and Dauben represent two distinguished positions within the history of mathematics. Crowe proposed as a law that revolutions never occur in mathematics, while Dauben maintained that such revolutions do occur and gave examples. However, to Pourciau, no 'real' controversy exists. The so-called controversy exists only at the semantic level, the level of the meaning of the term revolution. When they interpret the word revolution in the same way, as a Kuhnian revolution, where results from the old paradigm are rendered meaningless or untrue in the new

[3]Dauben studied among others the independent discovery of the infinitesimal calculus by Newton and Leibniz in the 17th century, the calculus by Cauchy in the 19th century and the creation of the non-standard analysis (NSA) by Abraham Robinson in the 20th century (Dauben, 1992a).

paradigm, then they agree that Kuhnian revolutions are inherently impossible in mathematics (Pourciau, 2000, p. 299). Pourciau himself wants to go beyond this 'semantic' discussion how to redefine revolution so that it might become a useful notion in mathematics. Pourciau defends that 'nothing in the nature of mathematics logically prevents a Kuhnian revolution' (Pourciau, 2000, p. 301). Therefore he has to explain what a Kuhnian revolution means. Referring to Kuhn's *The Structure of Scientific Revolutions* (Kuhn, 1970, p. 92) a revolution is characterized by 'noncumulative developmental episodes' in which a scientific community, having fallen into crisis, rejects its established paradigm and chooses a new paradigm to guide its specific practice. In such a noncumulative paradigm shift, some truths of the old paradigm become unintelligible, unresolved or false under the new paradigm. Those old truths do not translate into new truths and are lost in the new paradigm. There are no translations of the old conception which are true statements in the new conception. Pourciau now defends the idea that a noncumulative shift is not in contradiction with the nature of mathematics. He states that revolutions in mathematics are logically possible (if one wants to question also the classical paradigm), actually possible and historically possible. The case put forward by Pourciau is the development of the activity called intuitionist mathematics as proposed by the Dutch mathematician L. E. J. Brouwer in 1907. This mathematical system is in Pourciau's view incommensurable with classical mathematics—or would be as it is depicted ad a failed revolution in the course of history. According to Brouwer, the seat of mathematical activity resides in the individual mind which has a primordial intuition of time—in terms of its continuity—but also its discreteness, that is, its falling apart into a 'twoity'—the now and the past—in the passing of an instant. From this basic intuition, the mind of a human being, step by step, builds mathematics. These steps of human beings are finite. Hence there are no completed infinite constructions. Because every intuitionist assertion points to the completion of a finite construction, no infinite object can be the result of such a human construction. As a consequence certain classical theorems are no longer theorems when translated to intuitionist terms. Pourciau elaborated on the law of the excluded middle. Other theorems cease to be meaningful statements, e.g. when explicitly appealing to infinite expansions or sets.

What Pourciau brings to the discussion of whether or not revolutions occur in mathematics is that he goes beyond the semantic discussion on the notion of revolution. Referring to Kuhn's notion of revolutions, Pourciau upholds the logical possibility of a revolutionary mathematical transition into intuitionism. He also advances the thesis that Brouwer's intuitionism failed to convert the classical community due to contingent factors such as injudicious technical moves by and strategic choices against Brouwer

(Van Kerkhove, 2005, p. 214). With the example of Brouwer's intuitionism, Pourciau demonstrates how a possible Kuhnian revolution has even been avoided in the course of history (Pourciau, 2000, p. 303).

Actually we see a development where a deepening of the history of mathematics can reveal new perceptions. The American historian of mathematics Crowe seems to be with his *Ten 'laws'* one of the last ones to try to preserve the traditional ideas about mathematical knowledge. In his footsteps we also see the work of Caroline Dunmore who considers revolutions on the object-level of mathematics as impossible, but who does accept them at the meta-level of mathematics. The meta-level then consists of the meta-mathematical values that exist within a community, concerning the purpose and the method of mathematical objects, and of the general values concerning their nature (cf. the debate on the ontological status of mathematical objects).

Whatever one's position in the debate, one must note that any judgment on Crowe's tenth 'law' *Revolutions never occur in mathematics* depends on the meaning of the concepts of revolution and of mathematics. This observation has already been mentioned by Crowe himself who stressed the fact that the preposition *in* is crucial, for, as a number of examples make clear, revolutions may occur in mathematical nomenclature, symbolism, meta-mathematics, metaphysics of mathematics, methodology, standards of rigor, historiography of mathematics, values and mathematics and philosophy of mathematics, but that does not imply that they occur *in* mathematics itself. Here the question can be put forward that if revolutions can occur in mathematical nomenclature, symbolism, meta-mathematics, metaphysics of mathematics, methodology, standards of rigor, historiography of mathematics, etc. ..., what is left *in* mathematics when we strip all those aspects of mathematics away.

So, where does that leave us? With nothing but an inconclusive debate that should preferably be abandoned? Or is a different perspective required? As it happens, there is an important source, that has not been mentioned so far, and that could provide a creative way out: the works of Imre Lakatos.[4] It is indeed rather striking that in the revolutions-in-mathematics discussion his name is rarely mentioned (notwithstanding the fact that Donald Gillies was his pupil). Part of the key of a possible answer to this puzzle is that Lakatos clearly made a distinction between development and growth in the sciences on the one hand and in mathematics on the other hand. It is sufficient to look at the titles of some of his papers on (the philosophy of) mathematics to see that his views are not a variation on Kuhnian themes,

[4]We are referring here to the "mathematical" Lakatos and not to the "scientific" one. The latter Lakatos is first and foremost a pupil of Karl Popper, but the former one is heir to the works of George Pólya, including its Hegelian flavour. This however does not exclude a possible application of Popper's approach to the growth of mathematics, as Teun Koetsier (1991) has shown.

but rather a new vocabulary on its own: "A renaissance of empiricism in the recent philosophy of mathematics?", "What does a mathematical proof prove?" (not published during his lifetime), and "The method of analysis-synthesis" (although the title is not his own, it fits the content quite well).[5] And, of course, not to be forgotten, the seminal "Proofs and Refutations".

No talk about (grand) revolutions is to be found here but rather a detailed analysis of microfeatures of the history and development of mathematical proofs. A proof is proposed, it is refined, hidden lemmas are revealed, made explicit, incorporated, a new proof proposed and the process can be repeated. It is quasi-empiricist because a process of proofs *and refutations* is at work or, as the subtitle of "Proofs and Refutations" states: The Logic of Mathematical Discovery.

So, in what sense, could Lakatos's work present a way out? It is undeniably true that the distance between the macrolevel where revolutions are to be situated and the microlevel reduced to the search and improvement of mathematical proofs, is indeed immense and seems hardly bridgeable. But what if the microlevel is enriched, what if more elements of what mathematicians do in their daily business, are added so that a richer mathematical picture emerges, for we believe that such additional elements are an integral part of mathematics? Might such an "intermediate" description not make it easier to help to decide the matter whether yes or no revolutions occur in mathematics? The bold conjecture we put forward in the next section is that Crowe's Laws can be read as such bridge principles.

3 Crowe as a basis and outline

In this section we propose to have a second look at Crowe's laws and to investigate whether they could serve as a basis and outline to study mathematical practices. Our proposal is meant to stimulate discussions about and research of the full mathematical practices by interpreting these *laws* as statements about different types of (local) changes in mathematics. In other words, they are worthy of study not with a view to their justification or refutation within some philosophical framework but solely with a view to obtain a more complete descriptive study of mathematical practices. Each 'law' can be considered as a topic to set up a comparison between mathematics and the sciences, thus illuminating the relevant features that such a full-fledged theory of mathematical practices should study. Let us first of all recapitulate Crowe's ten laws (Crowe, 1992, pp. 16–19):

1. New mathematical concepts frequently come forth not at the bidding, but against the efforts, at times strenuous efforts, of the mathematicians who create them.

[5] All these papers can be found in Worrall and Currie (1978).

2. Many new mathematical concepts, even though logically acceptable, meet forceful resistance after their appearance and achieve acceptance only after an extended period of time.

3. Although the demands of logic, consistency, and rigour have at times urged the rejection of some concepts now accepted, the usefulness of these concepts has repeatedly forced mathematicians to accept and to tolerate them, even in the face of strong feelings of discomfort.

4. The rigour that permeates the textbook presentations of many areas of mathematics was frequently a late acquisition in the historical development of those areas, and was frequently forced upon, rather than actively sought by, the pioneers in those fields.

5. The 'knowledge' possessed by mathematicians concerning mathematics at any point in time is multilayered. A 'metaphysics' of mathematics, frequently invisible to the mathematician yet expressed in his writings and teaching in ways more subtle than simple declarative sentences, has existed and can be uncovered in historical research or becomes apparent in mathematical controversy.

6. The fame of the creator of a new mathematical concept has a powerful, almost a controlling, role in the acceptance of that mathematical concept, at least if the new concept breaks with tradition.

7. New mathematical creations frequently arise within, and depend in the mind of their creators upon, contexts far larger than the preserved content of these creations; yet these contexts, for all their original importance, may impede or even prohibit the acceptance of the creations until they are removed by the mathematical community.

8. Multiple independent discoveries of mathematical concepts are the rule, not the exception.

9. Mathematicians have always possessed a vast repertoire of techniques for dissolving or avoiding the problems produced by apparent logical contradictions, and thereby preventing crises in mathematics.

10. Revolutions never occur in mathematics.

As said, the first thing that is in our view absolutely striking about these ten statements, exception made perhaps and somewhat obviously for the tenth law, is that they highlight essential features of mathematical practices seen as a complete process. Not merely the end results are referred to, as, e.g., in the 7th law—no mention shall be made about the creator or the creation process in the end result—but nearly every aspect of what a mathematician practices when doing mathematics. The origin of and resistance to new concepts, the fact that multiple discoveries occur over and over again, the elimination of anomalies, etc., all refer to moments in the mathematical process, different from the final moment or end result.

The second thing that is absolutely striking is that, if in the first nine laws "mathematical" and "mathematics" is replaced respectively by "scientific" and "science", few would disagree with the resulting laws: the importance of the growth of scientific concepts, the resistance to new scientific concepts, the acceptance of new scientific concepts, the curious phenomenon of multiple discoveries, the objectivity of scientific laws that do not refer to specific scientists, the different techniques that allow to deal with inconsistencies and contradictions in scientific theories and so on. Of course, the question then forces itself upon us: if that is indeed the case, why then "deny" that mathematical revolutions occur as well? The first nine laws make no distinction between science and mathematics, and revolutions in science are generally accepted by philosophers of science. As nearly all philosophers see mathematics as different from science, this implies that some basic, fundamental or essential property needs to be found that can make such a distinction, to start with on the level of mathematical and scientific practices.

The Lakatosian key, as it were, to the solution is, we believe, to be found in the 4th law. The development of rigour makes reference to the most important (though not unique) element in mathematical practice, viz. proofs, as Lakatos rightly emphasized. Of course, one might cautiously claim that this law talks about mathematical proofs as there should be a law on such entities. After all, missing out proofs would be a serious shortcoming. As we already made one bold conjecture, let us add another one to it: the ten laws Crowe formulated, exception made for the tenth law itself, should all be interpreted *with reference to* the role they play in the process of finding, constructing, rejecting, and/or refining proofs. In different words, we claim the following: mathematical practice is composed of a set of different types of activities, but they are all related to the one core activity, which is the "proof business" with its possibly revolutionary dynamics. What follows is a closer examination.

The first three laws all refer to (mathematical) concepts. One of the most important elements in the process of constructing a proof for a given statement is the search for the "right" set of concepts to be used in the proof. A perhaps somewhat trivial example is this: give an easy proof that the equation $12445454545\,x^2 + 789878823823\,x - 989086789237921 = 0$ has no solutions in the integers. One could, of course, follow the standard procedure for calculating the solutions of a quadratic equation, but, once one realizes that the core concept to be used here is “odd-even”, then it is immediately obvious. The sum of the two terms involving x is always even as the coefficients are both odd, whereas the last term is odd, hence the sum can never be zero, QED. This simple example also shows that the questions what concepts are relevant in what context, is itself a hard problem. And, of course, it will happen that some concept is truly useful in the framework of a proof, but meets resistance if the concept is considered as a concept on its own. One thinks immediately of the introduction of the square roots of negative numbers tolerated within the context of polynomial equations of third degree, but considered “silly” outside of it (an example, by the way, that Crowe himself refers to, cf. p. 16), as Cardano himself testifies. When discussing the famous square root of -15, he refers to “mental tortures that one has to put aside” (Cardano, 1545, p. 219) and the famous quote that “so progresses arithmetic subtlety the end of which, as is said, is as refined as it is useless” (Cardano, 1545, p. 221).

The fourth law and the ninth law form a nearly obvious pair from the viewpoint of proofs. Start from the ideal situation from a formal logical position. Proofs in that context, it is generally understood, are represented by an ordered list of formulas, involving premises, axioms, outcomes of the application of formal rules, and such that the last formula of the list is the theorem to be proved. We all know, though perhaps some of us are not all too keen to emphasize it, that “real” mathematical proofs hardly ever reach this high quality level. (The usual claim is that the first chapters of any introductory book in whatever area of mathematics satisfy this standard, but from the third chapter onwards the standard is left behind). Seen from this angle, the fourth law sets a standard, so difficult to meet, that is far more interesting to develop subtle techniques that allow the mathematician to deal with problems such as the occurrence of contradictions, where the logical ideal has nothing to contribute, except the rule that inconsistencies should quite simply not occur.

The sixth, seventh, and eight laws refer, in this interpretation, to the social organization of, what we propose to call, a proof community (thus, by the way, making a distinction with a scientific community). A trivial observation: proof implies statement to be proved. The next question is equally obvious: what statement are we talking about? In less banal terms: what mathematical problems are interesting enough to merit our attention and to invite our attempts to (dis)prove it? Why, e.g., so much attention for the Riemann hypothesis (RH)? Among the manifold reasons that can be produced, let us just mention one: there is at this moment already a wealth of statements of the form “If the RH holds, then Φ”. Imagine the day that RH is proved: all of a sudden, all these theorems transform into quite simply “Φ”. It needs expert mathematicians—“of fame”, to use

Crowe's expression—to identify these interesting statements and, hence, the importance of the 1900 speech of David Hilbert, outlining the research agenda for a century to come. Given this rough picture, it follows that, if such figures as Klein, Hilbert or Poincaré "declare" that this or that problem should be solved, then a host of mathematicians will actually take up the invitation. Little wonder that multiple discoveries are made. Of course, much more needs to be said here, but we do believe that these laws too can be seen as relevant to proof practices.

This leaves us finally with the fifth law, perhaps the most intriguing one. From the proof viewpoint, it seems obvious that different layers are required: the search for a proof and related concepts involves heuristics, proof search methods. As in many cases such techniques become interiorized, it is quite understandable why there is so much talk about mathematical intuition (whether given by birth as God's gift or as a genetic code, or the result of a long and hard training process or, more likely, a combination of both). To identify interesting problems, be they internal to mathematics or with a view to possible applications, requires having a broad picture of the mathematical domain, how it relates to other areas of scientific interest, and how mathematics connects to society at large. Just one example: almost every mathematician knows the "feeling" (really no other term for it) that a specific proof of some statement is not the "right" proof. It is, e.g., too clumsy, too long-winded, or somehow arbitrary. Perhaps this idea is no more than a particular state of mind of the mathematician, but, at the same time, it does feed the idea that the proof is "out there" somewhere to be "discovered". Thus, this proof practice "feeds" a particular metaphysical view that supports and stimulates the mathematician's search and that definitely should be taken into account.

We repeat that, at this stage of our argument, no effective theory of mathematical practice has been presented but rather a list of ingredients, necessary to make the undertaking succeed. We hope to elaborate further on these matters; more specifically, we believe that the approach of Reviel Netz (1999) and the background furnished by Jerry A. Fodor (1983), roughly identifiable as "cognitive history of mathematics" will prove to be quite fruitful.

4 Conclusion

Let us summarize our findings. In the first place, we demonstrated the curious nature of the discussion about the occurrence of revolutions in mathematics. We noted that the different positions in the debate are highly dependent on the meaning of both terms, viz. mathematics and revolution. Although the impression might be that the discussion seems to be rather sterile, we defend the thesis that the fruitfulness of the research concerning revolutions and mathematics consists in the fact that the discussion need not settle the basic question "Are there or are there not revolutions in mathematics?", but that it stimulates the study of mathematical practices.

In the second place, as a positive piece of evidence for the above claim, the work of Crowe is used as such a frame of reference to study mathematical practices. Each of his famous ten 'laws' can be considered as a topic to set up a comparison between mathematics and the sciences as we showed by emphasizing the importance of mathematical proofs and all the elements in mathematical practice it ties up with. Although the laws in themselves are equally applicable to mathematics and the sciences, the characteristic element is that they can be interpreted in mathematics as all related to proof, whether in the context of the search and/or creation of proofs or of formulation, presentation, and importance.

We reiterate the observation at the end of the previous section. We have now brought together a set of ingredients to prepare the dish that will be called "A full-fledged theory of mathematical practice". We are quite sure that without any of the ingredients, the result will be disappointing. It remains to be shown that they are sufficiently good "chefs" out there, who will guarantee a splendid meal.

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