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Axiomatics and Progress in the Light of 20th Century Philosophy of Science and Mathematics

Dirk Schlimm⋆

Department of Philosophy McGill University Montreal (QC) H3A 2T7, Canada

E-mail: `dirk.schlimm@mcgill.ca`

Abstract. This paper is a contribution to the question of how aspects of science have been perceived through history. In particular, I will discuss how the contribution of axiomatics to the development of science and mathematics was viewed in 20th century philosophy of science and philosophy of mathematics. It will turn out that in connection with scientific methodology, in particular regarding its use in the context of discovery, axiomatics has received only very little attention. This is a rather surprising result, since axiomatizations have been employed extensively in mathematics, science, and also by the philosophers themselves.

1 Axiomatics

Euclid's *Elements* and Newton's *Principia* are beyond any doubt among the most widely known theories in mathematics and in science. They are crown jewels in the development of geometry and physics. What both theories have in common is the structure of their presentation, which is *axiomatic* or *deductive*: A number of statements (called *axioms, postulates, hypotheses,* or *laws*, depending on how their status is conceived) are posited, and the central claims of the theory (e.g., Pythagoras' theorem, or Kepler's 'laws') are derived as consequences. In addition, all

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notions of the theory are definable in terms of the primitive notions that occur in the axioms. Henceforth I shall refer to the practice of developing, employing, or studying systems of axioms as *axiomatics*. Notice that in axiomatic presentations it is not necessary for the primitive terms to be considered as uninterpreted symbols, nor must the notion of logical consequence be made explicit. In the former case we speak of a *formal* axiomatization, while the latter distinguishes axiomatizations from *formalizations*, which require a formal language and formal rules of inference. These notions are often conflated in the literature, but they should be kept apart to avoid unwarranted criticism of axiomatics.

To be sure, neither geometry nor physics ended with Euclid's and Newton's theories. Rather, they have inspired a great number of readers, they have been the starting points of various fruitful developments, and they have led to a great many new scientific and mathematical insights. These observations lead directly to the main motivation behind the present paper, namely the question regarding the role that an axiomatic presentation of theories plays in the development of science and mathematics.[1]

The usefulness of axiomatics in theory development is manifold. For example, the formulation of axioms can bring out hidden assumptions, explicate informal concepts, or reveal gaps in the argumentations; once a theory is axiomatized it can be studied through the axioms, and relations to other theories can be established; manipulations of axioms, which can be motivated by empirical findings that contradict some theorem or by attempts to prove the independence of the axioms, can suggest new theories.[2] Furthermore, I believe that axiomatics has a considerable effect on the perception and formulation of analogies, as well as on our capabilities of reasoning about abstract objects.[3]

Although the utility of axiomatic presentations should be of no surprise to working mathematicians or theoretical scientists, I shall show in the present paper that the contribution of axiomatics for the advancement of science and mathematics has not been properly acknowledged in the philosophical literature. To do so I shall present an overview of the main trends in philosophy of science and mathematics with respect to the following two questions:

[1] Notice that I am open as to what theories *are*, as long as they can be presented axiomatically.

[2] Non-Euclidean geometries are the most famous outcome of the latter.

[3] A more detailed account of this is planned for the future.

- How is the change from one theory to another accounted for, i.e., what are the mechanisms underlying theory change?
- What role is assigned to axiomatics in particular with regard to theory change and discovery?

As it turns out, in philosophy of science very little has been said in this regard. On the contrary, the notions of axiomatics and discovery have often been considered as being opposed to each other. In mathematics there has been a recognition of the creative power of axiomatics, in particular by David Hilbert. However, these views did not catch on in philosophy of mathematics and have been revived only recently.

Before turning our attention to the 20th century, let me briefly mention the major milestones in the history of axiomatics. According to Aristotle, scientific knowledge must be demonstrative, resting on "necessary basic truths" (*Post. Ana., I.6, 74.4*, [McKeo47, p. 21]). Euclid's subsequent axiomatization of geometry in the *Elements* was soon considered to be the prototypical presentation of scientific theory. It has inspired works like Newton's *Principia*, Spinoza's *Ethics*, and many many others.

Due to the use of axiomatics in the natural sciences, and to the development and growing acceptance of non-Euclidean geometries, the idea that axioms express necessary truths has been slowly abandoned. Also, starting with the recognition of the point/line duality of projective geometry, the meanings of the primitive terms lost their claim to uniqueness. Frege's invention of predicate logic led to a sharpening of the language of scientific presentations, reducing ambiguities and vagueness, as well as to an increase of rigor in the deductions (see also section 3, on related developments in 19th century mathematics). These developments form the background for the philosophical reflections in the 20th century that are presented next.

2 Philosophy of science

In the following I shall discuss what I consider to be four major families of views in the philosophy of science of the 20th century. Due to space limitations this can only be very sketchy, but I hope to be able to bring out the main positions concerning the questions mentioned above. I look at philosophy of science first, because it has had great impact on the discussions in philosophy of mathematics. Hence, the development in philosophy of mathematics can be better understood when seen in this broader context.

2.1 The received view

By the *received view* in philosophy of science, I will refer to the core of the views that emerged from logical positivism and were dominant from the 1930s until the 1960s (see [Sal*et.al.*92, p. 135]).[4] One of the main doctrines of the received view is that theories should be considered as linguistic entities, formulated in the language of first-order logic. Empirical meaning is then conferred on the primitive terms by means of coordinative definitions. As a consequence, the distinction between theoretical and observational terms was introduced and the relations between the two have been studied extensively. Specific views on the assessment of scientific theories ranged from *verification*, over *falsification* [Pop34], to *confirmation* [Hem45]. A most important distinction, for our purposes, is made between the *context of discovery* and that of *justification* [Rei38]. In general, the study of activities related to discovery is relegated to psychology, sociology, and history, but is not considered to be of interest for philosophy. Kekulé's dream of a snake biting its own tail, which suggested to him the structure the benzene ring, is seen as the prototypical example of a discovery about which philosophers could have nothing to say.

According to the received view scientific progress is explicated as a succession of theories. It is considered to be cumulative in the sense that old theories are replaced by more inclusive ones (e.g., rigid body mechanics being replaced by classical particle mechanics), or that theories are reduced to others (e.g., thermodynamics being reduced to statistical mechanics). However, more detailed principles providing heuristics for the development of theories are not investigated, since they are thought to lie outside of the context of justification.

Although formal axiomatic presentations of theories were used by philosophers of science adhering to the received view to study properties of theories, particular axiomatizations were not considered to be of philosophical interest, since they are neither unique for a particular set of statements (since different sets of axioms can determine the same set of state-

[4] Nowadays one can also find the label "once received view" [Cra02].

ments), nor do they determine unique interpretations. Furthermore, syntactic deductions of theorems from axioms yield only tautologies, while scientific discoveries express novel facts. Hempel summarizes these considerations as follows: "[A]xiomatization is basically an expository device," which "can come only after a theory has been developed" [Hem70, p. 250]. Thus, axiomatics was employed for presenting and studying scientific theories (and also for explicating philosophical notions like *justification* [Pop34], *explanation* [HemOpp48], and *existence* [Qui48]), but it was not considered in connection with theory development.

2.2 Reactions: Kuhn and Lakatos

In direct opposition to some of the main tenets of the received view, Thomas Kuhn published in 1962 what might well be the most influential book in philosophy of science of the 20th century, *The Structure of Scientific Revolutions*. In what is commonly referred to as the "historic turn" in philosophy of science, emphasis shifted from the internal structure of scientific theories to the actual development of science. Rather than theories, Kuhn considers broader units of scientific progress (*paradigms*), which embody the shared, accepted, and unquestioned views, standards, methods, theories, problems, and goals of the scientists working within a particular tradition. He distinguishes two phases of scientific development: During *normal science* the scientists work on the solution of puzzles guided by the standards and values of the current paradigm. When a considerable number of such puzzles resist a solution a *crisis* emerges, which leads to a proliferation of theories. This crisis is overcome by a *revolution* when a new paradigm is finally accepted that leads to the solution of the anomalies.

Since Kuhn considers different paradigms to be incommensurable, scientific progress, which according to him happens only in the course of scientific revolutions, is not cumulative.[5] Moreover, the scientific changes that are of interest to Kuhn are broader in scope than the move from one theory to another. Thus, it might not surprise us that his notion of theory is rather vague, and that he does not ask where the theories come from. He considers them as "imaginative posits, invented in one piece for application to nature" [Kuh70a, p. 12].

[5] The ideas of incommensurability and cumulative progress need some clarification, but this is beyond the purpose of this paper.

Investigation of predictions and determination of values for theoretical constants are regarded by Kuhn as typical problems during normal science. Similarly, he acknowledges that in the process of matching facts with theory scientists work on their theories in order to obtain more statements that can be confirmed or disconfirmed directly and to increase the precision of the predictions. Reformulation of theories in "equivalent but logically and aesthetically more satisfying form," as well as "to exhibit the explicit and implicit lessons" of particular paradigms are also regarded as part of the theoretical work [Kuh70b, p. 33]. This part of Kuhn's account of science is very similar to the conception of the received view. However, Kuhn does not consider these developments to be of great value, remarking that "perhaps the most striking feature of the normal research problems [. . .] is how little they aim to produce major novelties, conceptual or phenomenal" [*ibid.*, p. 35]. Kuhn also implies that the process of codification and axiomatization occurs late in the development of a discipline and only in response to a crisis:

> It is, I think, particularly in periods of acknowledged crisis that scientists have turned to philosophical analysis as a device for unlocking the riddles of their field. [. . .] To the extent that normal research work can be conducted by using the paradigm as a model, rules and assumptions need not be made explicit. [Kuh70b, p. 88; see also 44–48]

Thus, it seems to me that axiomatics is compatible with Kuhn's account of science, but the little he says about it implies that he did not regard it as an important factor for scientific development.

Another very influential reconstruction of science was offered by Imre Lakatos as an advancement of Popper's falsificationism.[6] According to the latter, scientific theories must be empirically falsifiable and should be rejected when such a falsification occurs. One obvious difficulty with this account is that it does not square well with actual scientific practice, where some theories continue to be pursued despite the existence of facts that stand in conflict with them. To overcome this difficulty, Lakatos proposes distinguishing between an irrefutable *hard core* and a *protective belt* of auxiliary hypotheses, which serve to make predictions and can be adjusted when confronted with contradictory empirical evidence [Lak70, p. 135]. For example, the hard core of Newton's gravitational theory consists of just his three laws of mechanics and the law

[6] Lakatos's account of science differs in important respects from his views on mathematics, which are discussed in section 3.3 below.

of gravitation, while whatever else is needed to apply them is considered to be part of the protective belt.

Lakatos's view is similar to Kuhn's in that it considers broader units of scientific development than theories. These units, called *research programmes*, are successions of theories that share the same hard core. Research programmes are called *progressive* when they allow for novel predictions some of which are confirmed by experience, or *degenerating* when they can only account for empirical evidence in retrospect. Science evolves by replacing degenerating research programmes by progressive ones, i.e., by changes of the theoretical hard core of a programme. But on how these changes come about also Lakatos is silent.

Despite the differences, Kuhn's account of normal science and Lakatos's progressive research programmes are both similar to the characterization of scientific progress of the received view. The notion of theory is more sophisticated in Lakatos than in Kuhn, but, again, the mechanisms of theory change are not explicated and axiomatics is not assigned any particular role in this process.

2.3 Discovery and models

By the mid-20th century most of the tenets of the received view had been challenged. Of particular importance for the present discussion are Norwood R. Hanson and Mary Hesse, who brought the notions of discovery and analogical reasoning back onto the philosophical table.[7]

Recall, that according to the received view the origin of the formulation of scientific laws was a subject matter for psychology, sociology, or history, but not for philosophy. The fundamental scientific inference was considered to be deduction of data from laws, which served as explanation of the observed phenomena (hypothetico-deductive account, [HemOpp48]). Hanson criticizes this view for not being justified in rejecting the investigation of the origin of scientific laws or hypotheses. He argues that the inference from data to plausible hypotheses is in fact logical, rather than merely psychological [Han58b]. Rather than just being lucky guesswork, Hanson considers the suggestion of new hypotheses to

[7] I say "back," because long before modern times, both analogical and deductive reasoning had been discussed in connection with scientific progress (e.g., by Aristotle and Proclus, see [Pos89, p. 148] and [HinRem74]).

be a reasonable affair that goes beyond inductive generalization, and as such it should be the subject of philosophical reflections.

Peirce's notion of *abduction*, also called *retroduction*, is taken over by Hanson as the logical inference from data to a hypothesis. He explains the origin of scientific laws by the perception of a particular *pattern*, which reveals the conceptual framework within which the data can be systematically organized. Discoveries of scientific laws, according to Hanson, begin with a problem, difficulty, or surprising empirical fact P that the scientist wants to solve or explain. Her reasoning is thereby directed towards developing a hypothesis H, such that if H were true, P would be accounted for [Han58a, p. 1086–7]. Such a hypothesis may be obtained, for example, from reasoning by analogy [*ibid.*, p. 1078].

Hanson vehemently rejects the hypothetico-deductive (HD) view of scientific theories, but he also acknowledges that the deduction of consequences from general laws is a crucial ingredient for science. So he writes, for instance, that we can not determine what counts as an *anomaly*, i.e., a deviation from our expectations, "until we have some fairly full theories whose consequences *constitute* our expectations" [Han65, p. 52, emphasis in original]. Hanson later obscures his own observation by introducing anomalies as conclusions that "although logically 'expected,' are psychologically quite unexpected," and the aim of retroduction is to come up with hypotheses that entail the anomaly "as the 'previous' theory may not have done" [*ibid.*]. Presumably, he means that the new hypotheses and consequences are psychologically more satisfactory. However, in the next paragraph Hanson describes the retroductive activity of the scientist as seeking "a novel HD framework within which to reveal the anomaly as logically-to-be-expected" [*ibid.*, p. 53].

Thus, although Hanson appears to be quite hostile towards deductive methods and does not give credit to the role of logical deductions in scientific progress, he employs them himself for obtaining consequences of hypotheses. Indeed, it seems to me that both approaches (deductive and retroductive) should be regarded as complementing each other, and that in fact scientists often alternate between them when developing theories. The psychologist Clark Hull, for example, describes theory construction as a process of recurring cycles of hypothesis formulation and testing of consequences. When certain facts can not be accounted for, or certain consequences do not conform to the facts, then the hypotheses have to be amended [Hul52].

Generally, one can interpret Hanson as arguing for widening the scope of philosophy of science by demanding a philosophical investigation of the creative processes behind theory construction. Mary Hesse pursues a very similar goal in her *Models and Analogies in Science* [Hes66]. She distinguishes between *material* and *formal* models; the former are based on pre-theoretic analogies between two observable domains, while the latter are different interpretations of a formal system. Hesse argues that material models surpass formal ones in regard to producing novelties and justifying scientific predictions. Thus, concerning the status of models in science, she maintains, against the received view, the existence of an "essential and objective dependence between an explanatory theory and its model that goes beyond a dispensable and possibly subjective method of discovery" [Hes72, p. 356]. To grant that material models are necessary ingredients of scientific theories, however, does not imply that formal models (and the axioms they are models of) do not play any significant role.

Let me point out here what I consider to be an unfortunate pattern in the previous arguments. When new aspects of scientific activity are introduced into the discussion, the new views are often set in contrast to other specific views. This is important for highlighting the values of the new approaches, but it also tends to devalue the insights that have been gained previously. In particular, Hanson and Hesse showed the importance of retroduction and analogical reasoning for theory construction, but in doing so they employed much unnecessary rhetoric against the use deductive methods, which can in fact very easily be seen to complement their own accounts.

Both this pattern of argumentation and the focus on models are also characteristic for the fourth trend in philosophy of science I want to present, namely the semantic view of theories.

2.4 The semantic view of theories

The *semantic view* of theories is a major trend in philosophy of science, which also developed in reaction to the received view. Building on work by Beth and Suppes, its main proponents are van Fraassen, Giere, Suppe, Sneed, and Stegmüller ([vFra80], [Gie88], [Sup77], [Sne71], [Ste76]).

In a series of papers in the 1960s Patrick Suppes argued for an extension of the then still current received view of scientific theories. Regarding theories as an abstract logical calculus in the language of first-order logic augmented by coordinating definitions or empirical interpretations to relate them to the world is too simple a picture, according to Suppes [Sup67]. In particular, he maintains that in practice "formalization [...] in first-order logic is utterly impractical," and suggests including models (understood in the mathematical sense of Tarski, [Tar44]) into the philosophical considerations about science. This, he argues, has the advantage of being more natural when complex scientific theories are discussed, and of allowing for a rigorous mathematical (i.e., model theoretic) treatment of various aspects of scientific practice. Moreover, by studying arithmetical models of theories one can also obtain insights into the isomorphic empirical models [Sup67, p. 59].

One of Suppes's main points is that actual scientific practice is much more complicated than the simple account of theories suggests: "If someone asks, 'What is a scientific theory?' it seems to me there is no simple response to be given" [*ibid.*, p. 63]. In light of the future developments it should be noted here that Suppes does *not* define theories as a class of models. Rather, he points out that "the explicit consideration of models can lead to a more subtle discussion of the nature of a scientific theory" [*ibid.*, p. 62].

Suppes's considerations have been taken up by van Fraassen, who presents his view, called the *semantic* approach, as being opposed to the "axiomatic and syntactical" analysis of theories [vFra70, p. 326]. In contrast to Suppes, who regards semantic and syntactic approaches as complementary, van Fraassen, after initial hesitation, is comfortable of presenting "a view of theories which makes language largely irrelevant to the subject" [vFra87, p. 108]. He characterizes the contrast between the syntactic and the semantic view of theories as follows:

> The syntactic picture of a theory identifies it with a body of theorems, stated in one particular language chosen for the expression of that theory. This should be contrasted with the alternative of presenting a theory in the first instance by identifying a class of structures as its models. In this second, semantic, approach the language used to express the theory is neither basic nor unique; the same class of structures could well be described in radically different ways, each with its own limitations. The models occupy center stage. [vFra80, p. 44]

The observation that a particular axiomatization of a theory is not unique had been made already by proponents of the received view. However, there the conclusion was to not consider particular axiomatizations as being philosophically illuminating, while van Fraassen draws the conclusion of rejecting a linguistic account of theories altogether.

By pointing to the inadequacies of particular versions of the syntactic approach, van Fraassen argues indirectly for the semantic picture. However, van Fraassen's criticisms may affect particular versions of syntactic approaches, but by no means the syntactic approach in general, as has been noted also by Worrall [Wor84, p. 71–73]. The direct argument for the semantic approach is that it is more faithful to the way scientists actually talk and write (see also [Gie79]). As an example, van Fraassen discusses four "axioms of quantum theory," as they can be found in books on quantum mechanics, and claims that

> they do not look very much like what a logician expects axioms to look like. [. . .] To think that this theory is here presented axiomatically in the sense that Hilbert presented Euclidean geometry, or Peano arithmetic, in axiomatic form, seems to me simply a mistake. [vFra80, p. 65]

It is not clear to me what the distinction is that van Fraassen here alludes to, but it appears to be a result of conflating axiomatization with formalization. When discussing the inadequacy of the syntactic approach he argues against understanding scientific theories as formal deductive systems in the language of first-order logic. In the above quote, however, he contrasts his view with an axiomatization in the sense of Hilbert, which is neither formulated in the language of first-order logic, nor uses explicitly stated rules of inference. Rather, Hilbert presents the primitive terms as uninterpreted, thereby defining a hierarchically structured class of models. Quite similarly, van Fraassen considers the axioms of quantum theory to be "a description of the models of the theory plus a specification of what the empirical substructures are" [*ibid.*]. Thus, despite van Fraassen's claim to the contrary, it seems to me that the practical differences between axiomatic (e.g., Hilbert [Hil99]) and semantic approaches are only a matter of emphasis.

In particular, the classes of structures that van Fraassen discusses are all characterized in terms of a system of axioms that they satisfy. So, van Fraassen claims to present an alternative to a linguistic account of theories, but in fact he relies on axioms to determine the class of models that constitute a theory. In other words, his account makes essential use of axioms, but he refuses to regard them as part of what he calls 'theories.' In addition, he conflates the notion of axiomatization and formalization, and has only very little to say regarding heuristic mechanisms for theory construction and development.

Suppes's suggestion of employing model theoretic techniques in philosophy of science was also put into practice in Joseph Sneed's characterization of the development of scientific theories, in particular of mathematical physics [Sne71]. Sneed attempts to reconstruct the dynamic aspects of theories, i.e., how they grow and change, how they become accepted and rejected. His *nonstatement* view of theories (also referred to as *structuralist* view) rejects the traditional view of theories as sets of sentences formulated in a first-order language, but identifies theories with a class of models (the 'core') together with an open set of intended applications. The development of a theory is then characterized by a series of expansions of the core or by changes in the set of intended applications, neither of which need result in more inclusive models or a greater number of applications.

Sneed's account of theory development was put to use by Stegmüller to explicate the theses put forward in Kuhn's *The Structure of Scientific Revolutions*. According to Stegmüller, theories develop in time "through the discovery of new or the rejection of old laws, or the addition of new constraints" [Ste76, p. 133]. Notice how claims about core extensions, i.e., about models, are made here in terms of laws or constraints, i.e., in terms of linguistic entities.

Sneed and, following him, Stegmüller give a logical reconstruction of theory development and change, but they do not address (other than in most general terms) how these theory changes come about. In fact, although rejecting the view that theories are best understood as linguistic entities, they do speak of models as being determined by axioms and of changes of models as resulting from changes of axioms. So, Sneed and Stegmüller's account tacitly assumes that axiomatizations affect scientific progress, but, just like Kuhn, Lakatos, and van Fraassen, they do not address this directly.

2.5 Summary

In the received view, theories were understood as sets of sentences, but they were studied in isolation, as if they were static, so to speak. Axiomatic presentations of theories were used in the study of scientific theories, but very little concern was shown for the actual development of theories, nor for the process of discovery in general. Dynamic mechanisms underlying theory development or hypothesis formulation were considered as belonging to the context of discovery and thus as being outside the scope of philosophical investigations. The turn towards the historical and dynamic aspects of science, which include processes of discovery, was accompanied with a move away from theories and linguistic representations. Presumably this was motivated by the need to highlight the contrast to the received view. Thus, we can formulate the two slogans "axiomatic theories without discovery" and "discovery without axiomatic theories" as characterizing the two main directions in 20th century philosophy of science. The relation between axiomatics and discovery has not been the focus of attention in the mainstream and only few philosophers paid careful attention to it, most notably, Patrick Suppes. Unfortunately, it seems that Suppes has either been misinterpreted or neglected.

After this brief recapitulation of 20th century philosophy of science, let us now repeat this exercise, but this time from the point of view of philosophy of mathematics.

3 Philosophy of mathematics

Philosophy of mathematics in the 20th century was highly influenced by late 19th century developments in mathematics. In particular, Frege's invention of the language and calculus of predicate logic [Fre79] began his *logicist* program of reducing mathematical notions to logical ones [Fre84], which was then carried through (revealing its weaknesses) by Whitehead and Russell in their monumental *Principia Mathematica* [WhiRus10–13]. Closely related are the trend of arithmetizing mathematics [Kle95], i.e., developing mathematics without recourse to geometric intuitions, and the emergence of projective and non-Euclidean geometries, which led to reconsideration and eventual abandonment of the notion of axioms as self-evident truths. Another very influential development was the emergence of set theory in the works of Cantor and Dedekind (see [Fer99]). Around the turn of the century, however, the paradoxes discovered by Zermelo, Russell, and others, showed that neither the prevailing conception of sets, nor Frege's system of logic provided an ultimate foundation of mathematics.

3.1 Early 20th century

In the wake of the developments just mentioned, but not necessarily causally related to them, two very different approaches to mathematics emerged: On the one hand, L. E. J. Brouwer, building on Kantian views, formulated his philosophy of *intuitionism*, according to which mathematics is an “essentially languageless activity” [Bro52, 510]. He considered language merely as an aid for communication and memory, and formal logic as restricting mathematical thinking, rather than assisting it. Thus, Brouwer regarded the relation between axiomatics and mathematical creativity as a negative one. On the other hand, David Hilbert worked extensively and very successfully on axiomatizations, in particular in geometry and logic, and he actively promoted axiomatizations in other areas of mathematics and physics (see [Pec90]). In 1917 he referred to the axiomatic method as a “general method of research” [Hil18, p. 405]. For him, axiomatizing a body of knowledge displays the internal conceptual connections and provides a fertile soil for further investigations. He regarded the aim of axiomatically “deepening the foundations” as a fruitful one for all domains of inquiry. Hilbert saw clearly that axiomatics plays an important role in mathematical discovery in a number of ways, only one of which is that it allows rigorous investigations of formal theories themselves, which led to the development of the prosperous discipline of *proof theory*. In the course of the ensuing debate with Brouwer and his followers, the so-called *Grundlagenstreit*, Hilbert’s position became known as *formalism*. This is quite unfortunate, since nothing could be more wrong than saying that Hilbert considered mathematics to consist just of formal manipulations of meaningless symbols (see [Ewa96, p. 1106]).

3.2 The received tradition

Despite the great influence Hilbert and his Göttingen school exerted upon mathematics, the mainstream in philosophy of mathematics followed the views of Frege, Russell, and logical positivism, echoing the development in philosophy of science.[8] Accordingly, mathematics was regarded as a purely deductive science, and philosophical discussions revolved around

[8] In the following I use the term *received tradition* for these and related views in philosophy of mathematics.

the status of mathematical knowledge (analytic, a priori), mathematical truth (deductivism vs. platonism), the proper foundations of mathematics (logic vs. set theory), and the nature of mathematical objects (platonism, nominalism, neo-logicism, structuralism).

The received tradition considered mathematical discovery as a largely irrational process, just as scientific discovery was seen in the the contemporary reflections on science. For mathematics, the paradigmatic example of a discovery was Poincaré's theorem on Fuchsian functions. According to Poincaré's own account, the theorem popped into his mind quite unexpectedly while he was boarding a bus. Hadamard discusses this and similar examples in his *The Psychology of Invention in the Mathematical Field* [Had45] and especially emphasizes the role of unconscious processes in mathematical creativity. Although formulated over fifty years ago, Hadamard's views are still popular among mathematicians (see [ChaCon95]).

3.3 New directions

At the time when philosophers of science began formulating alternatives to the received view, a similar turn towards history and practice took place also in philosophy of mathematics, albeit on a much smaller scale. In general, however, the new considerations about science were not carried over to mathematics. Instead, the development in philosophy of science seemed to highlight the fact that science and mathematics are entirely different enterprises. Of the philosophers who followed the shift towards history and practice and who are thus more likely to reflect on the relation between axiomatics and mathematical progress, I shall discuss Polya, Lakatos, and Kitcher, and conclude by commenting briefly on some very recent developments in philosophy of mathematics.

In 1945 the mathematician George Polya initiated almost single-handedly the turn of philosophy of mathematics towards mathematical practice. He distinguishes between two sides of mathematics, which resembles the familiar distinction between the contexts of justification and discovery:

> Yes, mathematics has two faces; it is the rigorous science of Euclid but it is also something else. Mathematics presented in the Euclidean way appears as a systematic, deductive science; but mathematics in the making appears as an experimental, inductive science. [Pol45, p. vii]

By means of numerous examples Polya investigates the heuristics involved in the invention of mathematics. He is well aware of the novelty of this presentation and writes that "mathematics 'in statu nascendi,' in the process of being invented, has never before been presented in quite this manner" [*ibid.*]. In his 1954 two volume work *Mathematics and Plausible Inference* [Pol54] Polya continues the line of inquiry he began in 1945, distinguishing between *demonstrative* reasoning, by which mathematical results are presented, and *plausible* reasoning, which serves "to distinguish a guess from a guess, a more reasonable guess from a less reasonable guess" [*ibid.*, p. vi]. According to Polya, the two major forms of plausible reasoning in mathematics are reasoning by induction and by analogy. He explicates the notion of analogy in terms of structure preserving mappings (homomorphisms and isomorphisms) or as being based on "relations that are governed by the same laws." An example that Polya mentions is the analogy between addition and multiplication of numbers, since they are both commutative, associative, and admit an inverse relation. On similar grounds, subtraction and division are analogous, as are the roles played by 0 and 1.

> In general, *systems of objects subject to the same fundamental laws* (or axioms) may be considered as analogous to each other, and this kind of analogy has a completely clear meaning. [*ibid.*, p. 28; orig. emphasis]

Here Polya points out the importance of axiomatic characterizations of mathematical notions for finding and formulating analogies, i.e., one of the fundamental processes of plausible reasoning by which new mathematics is created.

Imre Lakatos explicitly acknowledges "Polya's revival of mathematical heuristic and [...] Popper's critical philosophy" as the background of his *Proofs and Refutations*, which is subtitled "The Logic of Mathematical Discovery" [Lak76, p. xii]. Against the received tradition, which he refers to as the *deductivist* view of mathematics Lakatos aims at elaborating the point

> that informal, quasi-empirical, mathematics does not grow through a monotonous increase of the number of indubitable established theorems but through the incessant improvement of guesses by speculation and criticism, by the logic of proofs and refutations. [*ibid.*, p. 5]

Accordingly, Lakatos rejects the attempts of establishing ultimate foundations of mathematics, and also the traditional notion of proof as formal derivations.

Through a careful and detailed analysis of the historical development of the Euler-conjecture on the relation between the number of vertices, edges, and faces of polyhedra, Lakatos's work shows how the content of mathematical concepts is changed in the process of developing proofs. He calls the results of this process *proof-generated concepts* and shows that they completely replace the naive concepts with which the mathematical investigations began.

Since Lakatos considers axiomatic theories to be intimately connected to the view of the received tradition, he does not connect his conclusions to axiomatics, but regards them as being opposed. However, once we admit that axiomatizations do not have to be static, but can evolve, it is only a small step to transfer the strategies that Lakatos identifies to axiomatically characterized notions. Thus, against his own intentions, we can read Lakatos as identifying techniques for reformulating axioms in the light of failures of proof attempts or counterexamples. We can therefore infer from Lakatos's investigations that, by explicitly stating the assumptions made in arguments, axiomatics contributes to the development of mathematics.

The publication of Philip Kitcher's *The Nature of Mathematical Knowledge* has been hailed as another "event of great importance for philosophy of mathematics" [Gro85, p. 71]. As the result of a detailed examination of the history of mathematics Kitcher proposes a *naturalist* account, which regards mathematical knowledge as *quasi-empirical* and *fallible* (see the above quotation by Lakatos). He argues for a close connection between science and mathematics and sees himself as standing in what he calls the "maverick tradition" in philosophy of mathematics that originated with Lakatos [AspKit88, p. 17].

Kitcher regards the historical development of mathematics as a sequence of *practices*, which are individuated by five distinct, but interrelated, components: The language in use among mathematicians; the set of accepted statements; the questions regarded as important; the reasonings used to justify accepted statements; and methodological views about the character of mathematical proof, and the ordering of mathematical disciplines [Kit83, p. 163]. Mathematical progress is characterized by Kitcher as *rational interpractice transitions* that aim to maximize the chances to attain one of the following two epistemological goals: To provide idealized descriptions allowing us to structure our experience, and to attain an intellectual understanding of these descriptions themselves

[Kit88]. As particular activities that yield such rational interpractice transitions Kitcher suggests five patterns of mathematical change: Question-answering, question-generation, generalization, rigorization, and systematization [Kit83, p. 194].

Although one might be able to find ways in which axiomatics is of use in all five of these patterns, Kitcher discusses axiomatizations only in relation to systematization. Here he mentions the introduction of new terms and principles that provide a unified perspective. He distinguishes between systematization by *axiomatization*, where a small number of principles and definitions are fixed from which previously "scattered" statements are derived, and systematization by *conceptualization*, which "consists in modifying the language to enable statements, questions, and reasonings which were formerly treated separately to be brought together under a common formulation" [*ibid.*, p. 221]. To me both kinds of systematization are aspects of axiomatics, and Kitcher himself seems to conflate the terms of his own distinction by discussing the introduction of the concept of an abstract group as an example of axiomatization. In any case, in contrast to Lakatos, here the usefulness of axiomatics for mathematical progress is explicitly acknowledged.

In addition to its role in rational interpractice transitions, axiomatics can also contribute to the cumulative character of mathematics, which, according to Kitcher, is achieved through *reinterpretation* of previous theories. For example, the discovery of non-Euclidean geometry did not overthrow Euclidean geometry, but rather it led us to change our views about its necessary character and the meanings of the primitive terms. This move can be explicated by the transition from a particular interpretation of an axiom system to another, or a class of other interpretations.

The considerations of Polya, Lakatos, and Kitcher have recently been taken up by more and more philosophers of mathematics. Regarding the interplay between axiomatics and mathematical discovery I would like to draw attention to the collection edited by Grosholz and Breger, *The Growth of Mathematical Knowledge* [GroBre00]. Herein, many different aspects of the development of mathematics are discussed, the traditional approaches are criticized for not being able to tell an adequate story about the development of mathematics, and the role of abstraction and axiomatization for mathematical progress is emphasized.

3.4 Summary

The development of philosophy of mathematics that I presented can be followed in more detail by considering the following anthologies, each of which contains a number of important contributions reflecting the various trends discussed. Regarding the early views, van Heijenoort's *From Frege to Gödel* [vHei67] and Ewald's *From Kant to Hilbert* [Ewa96] provide many sources; the received tradition is best represented by the articles in the collection *Philosophy of Mathematics* by Benacerraf and Putnam [BenPut83], while articles pertaining to the newer directions can be found in Tymoczko's *New Directions in Philosophy of Mathematics* [Tym98]. Aspray and Kitcher's *History and Philosophy of Modern Mathematics* [AspKit88] contains an interesting juxtaposition of contributions in the received tradition and also following the newer directions. It also contains an excellent introduction, which presents the development of philosophy of mathematics from a more general perspective than the present paper.

From my, admittedly sketchy, overview about what has been said in philosophy of mathematics regarding the relation between axiomatics and discovery, the parallels to the developments in 20th century philosophy of science should have become obvious. In both areas the received view and received tradition have dominated the discussions for a long time. They were followed by polarized reactions, mainly antagonistic in spirit. Regarding the reflections on the interplay between axiomatics and mathematical progress, we can see a revival of the views first formulated by Hilbert in the early decades of the 20th century; a similar move in philosophy of science has yet to be made.

4 What's next?

Returning to the questions posed at the beginning of this paper, it has now become clear that neither of them has been addressed in a satisfactory manner in 20th century reflections on science and mathematics. In particular, a systematic study of the role that axiomatics plays in theory development is still missing.

I have been deliberately vague regarding the term 'axiomatics,' because what I consider to be various aspects of it, namely 'axiomatic method,' 'symbolization,' 'formalization,' etc., have been understood in a number of very different ways in the past. For future discussions on methodology in science and mathematics, a better disentanglement of notions and terminology is sorely needed. Moreover, reflections about what scientists say and do seem to profit when the approach is less dogmatic in character, i.e., without the imposition of too strict a priori assumptions. Clearly some focus is necessary, but this should not be gained by completely dismissing alternative aspects and approaches. This is related to what I have found to be an unfortunate recurrent pattern in the discussions, namely that when new points of view are proposed, they are often set in stark contrast to some previous position. This is important for highlighting the novelty of the new approaches, but also tends to devaluate the insights gained by the earlier reflections.

After all, "Die Mathematik ist ein BUNTES *Gemisch*" [Wit84, p. 176], and this should be reflected also in the study of and considerations about the theoretical aspects of science.

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