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The development of metamathematics
And proof theory

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"Clarifying the nature of the infinite": the development of metamathematics and proof theory*

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Abstract

We discuss the development of metamathematics in the Hilbert school, and Hilbert's proof-theoretic program in particular. We place this program in a broader historical and philosophical context, especially with respect to nineteenth century developments in mathematics and logic. Finally, we show how these considerations help frame our understanding of metamathematics and proof theory today.

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1 Introduction

The phrase "mathematical logic" can be understood in two ways. On the one hand, one can take the word "mathematical" to characterize the *subject matter*. Understood as such, "mathematical logic" refers to the general study of the principles of mathematical reasoning, in much the same way as the phrase "inductive logic" refers to the study of empirical reasoning, or "modal logic" refers to the study of reasoning that involves modalities. On the other hand, one can take the word "mathematical" to characterize the *methods* of the discipline in question. From this point of view, "mathematical logic" refers to the general study of the principles of reasoning, mathematical or otherwise, using specifically mathematical methods and techniques. We are using the word "metamathematics" in the title of this paper to refer to the intersection of these two interpretations, amounting to the mathematical study of the principles of mathematical reasoning. The additional phrase, "and proof theory," then denotes a particular metamathematical approach, in which the aim is to model mathematical practice by giving a formal account of the notion of proof.

This terminological clarification is not just legalistic hair-splitting. Logic in general, as a separate field of study, finds its roots in Aristotle's treatment of syllogistic reasoning. The application of mathematical techniques to logic dates back, perhaps, to Leibniz, and was revived and substantiated in the works of Boole in the mid-nineteenth century. In contrast, Frege developed his logic specifically with applications to the foundations of mathematics in mind. By the end of the nineteenth century various aspects of logic in both senses had, thus, been recognized and studied separately. But it wasn't until the twentieth century that there was a clear delineation of the field we have called metamathematics; nor was there, before that time, a clear sense that the synthesis of the two disciplines — "mathematical logic" in both senses above — might be a fruitful one. This synthesis was set forth most distinctly by David Hilbert in a series of papers and lectures in the 1920's, forming the basis for his *Beweistheorie.* Hilbert's program, together with its associated formalist viewpoint, launched the field of Proof Theory, and influenced the development of mathematical logic for years to come.

Historical hindsight has, however, not been kind to Hilbert's program. This program is often viewed as a single-minded and ultimately inadequate response to the well-known "foundational crisis" in mathematics early in the twentieth century, and is usually contrasted with rival intuitionist and logicist programs. In the preface to his *Introduction to Metamathematics*, Stephen Kleene summarizes the early history of mathematical logic as follows:

> Two successive eras of investigations of the foundations of mathematics in the nineteenth century, culminating in the theory of sets and the arithmetization of analysis, led around 1900 to a new crisis, and a new era dominated by the programs of Russell and Whitehead, Hilbert and of Brouwer.[1]

Conventional accounts of these developments tell a tale of epic proportions: the nineteenth century search for certainty and secure foundations; the threats posed by the discovery of logical and set-theoretic paradoxes; divergent responses to these threats, giving rise to heated ideological battles between the various schools of thought; Hilbert's proposal of securing mathematics by means of consistency proofs; and Gödel's final *coup de grace* with his incompleteness theorems. Seen in this light, Hilbert's program is unattractive in a number of respects. For one thing, it appears to be too single-minded, narrowly concerned with resolving a "crisis" that seems far less serious now, three quarters of a century later. Moreover, from a philosophical point of view, strict formalism does not provide a satisfying account of mathematical practice; few are willing to characterize mathematics as an empty symbol game. Finally, and most pointedly, from a mathematical point of view Gödel's second incompleteness theorem tells us that Hilbert's program was, at worst, naive, and, at best, simply a failure.

In this paper we will argue that the conventional accounts are misleading, and fail to do justice to the deeper motivations and wider goals of Hilbert's *Beweistheorie*. In particular, they overemphasize the role of the crisis, and pay too little attention to the broader historical developments. Furthermore, the usual way of contrasting Hilbert's program with rival logicist and intuitionist ones provides a misleading impression of the relationship of Hilbert's views to logicist and constructive viewpoints. This has led to narrow, distorted views not only of Hilbert's program, but also of research in metamathematics and proof theory today.

We are, by no means, the first to argue against the conventional accounts along these lines.[2] As will become clear, in writing this essay we have drawn on a good deal of contemporary scholarship, as well as historical sources. Our goal has been to present this material in a way that helps put modern research in metamathematics and proof theory in a broader historical and philosophical context. In addition, we try to correct some common misperceptions, and establish the following claims.

First, we will argue that Hilbert's proof theoretic program was not just a response to the foundational crisis of the twentieth century. We will show that

[1] Kleene [71], page v.

[2] See, for example, Feferman [33], Sieg [103], Sieg [105], Stein [110], the introductory notes in volume 2 of Ewald [31], or the introductory notes to part 3 of Mancosu [83].

it addresses, more broadly, a deeper tension between two trends in mathematics that began to diverge in the nineteenth century: general conceptual reasoning about abstractly characterized mathematical structures, on the one hand, and computationally explicit reasoning about symbolically represented objects, on the other. We will show that one of the strengths of Hilbert's program lies in its ability to reconcile these two aspects of mathematics. Second, with its sharp focus on deductive reasoning, syntax, and consistency proofs, Hilbert's program is often viewed as philosophically and methodologically narrow. But Hilbert was sensitive to a broad range of mathematical issues, as well as to the diversity of goals and methods in mathematical logic. We will argue that he deserves more credit than is usually accorded him for clarifying and synthesizing the various viewpoints. Third, Hilbert's program is usually associated with a type of formalism that relies on very restrictive ontological and epistemological assumptions. We will argue that both Hilbert's writings and the historical context make it unlikely that Hilbert would have subscribed to such views, and show that far weaker assumptions are needed to justify his metamathematical methodology. We will thereby characterize a weak version of formalism that is still consistent with Hilbert's writings, yet is compatible with a range of broader philosophical stances. Finally, we will show how these considerations are important to understanding current research in metamathematics and proof theory. We will discuss the general mathematical, philosophical, and computational goals that are pursued by contemporary proof theorists, and we will see that many of Hilbert's central insights are still viable and relevant today.

2 Nineteenth century developments

Conventional accounts of the "crisis of foundations" of the early twentieth century portray the flurry of research in the foundations of mathematics as a direct response to two related developments: the discovery of the logical and set-theoretic paradoxes around the turn of the century, most prominently Russell's paradox, and the ensuing foundational challenges mounted by Henri Poincaré, L. E. J. Brouwer, and others. We will argue that it is more revealing to view the foundational developments as stemming from, and continuous with, broader developments in the nineteenth century, thereby understanding the stances that emerged in the twentieth century as evolving responses to tensions that were present earlier.

In Section 2.1, we will briefly summarize some relevant changes to the mathematical landscape that took place in the nineteenth century. In Sections 2.2 and 2.3, we will discuss the ways these internal developments were accompanied by evolving conceptions of the nature of mathematics, as well as of the nature of mathematical logic.

2.1 Developments in mathematics

It is not unreasonable to trace the origins of much of contemporary mathematics to the nineteenth century, "an age of deep qualitative transformations,"[3] and an age during which mathematics underwent, in the words of Howard Stein, "a transformation so profound that it is not too much too call it a second birth of the subject — its first birth having occurred among the ancient Greeks."[4] Though it is well beyond the scope of this paper to do justice to these events, the following brief sketch of some of the major developments will help set the stage for our subsequent discussion.

To begin with, during this period geometry moved well beyond the long dominant Euclidean paradigm. Poncelet launched the field of projective geometry, which deals with those properties of geometric figures that are invariant under projections, and he saw that introducing "points at infinity" allowed for a uniform treatment of the conic sections. Early in the century Gauss, Lobachevsky, and Bolyai came independently to the conclusion that one could not prove Euclid's fifth axiom from the other four, and they studied the consequences of its negation; concrete interpretations of non-Euclidean geometries were later provided by Beltrami, Cayley, Klein, and Riemann. Moreover, in a sweeping work presented in 1854 (but not published until 1868), Riemann brought even greater generality to the term "geometry" by introducing the study of general geometric manifolds of varying curvature.

Algebra was similarly transformed. The century saw the development of Galois' theory of equations; the rise of the British school of algebraists, including Hamilton, Cayley, Boole, and Sylvester; and Grassmann's work on algebras generalizing Hamilton's quaternions. Even more dramatic were the evolving interactions of algebra with other branches of mathematics. In France and Germany, algebraic methods in geometry took their place beside traditional synthetic methods, a trend which reached a high point with Klein's *Erlanger Programm* of 1872. Having established a first foothold with Gauss, algebraic methods also gained currency in number theory, in the work of mathematicians like Kummer, Germain, and Dedekind. Niels Abel used algebraic methods in his studies of the elliptic functions, and such methods in analysis flowered in the hands of Jacobi, Dirichlet, Riemann, and Hamilton. Similarly, Boole was respected for his algebraic treatment of integration, though, of course, he is best known for his algebraic approach to logic. These developments are summed up by the observation that "between the beginning and the end of the last century... the subject matter and methods of algebra and its place in mathematics changed beyond recognition,"[5] and, even more pointedly, that "there was a manifest tendency of algebraization of mathematics."[6]

The nineteenth century also saw major changes in analysis, including the

[3]In Chapter 2 of [76], "Algebra and algebraic number theory," by I. G. Bashmakova and A. N. Rudakov with the assistance of A. N. Parshin and E. I. Slavutin, page 35. (In general, when the translation of a work is quoted, pages numbers refer to the translations.)

[4]Stein [110], page 238.

[5]Chapter 2 of [76], Page 35.

[6]Ibid.

development of Fourier analysis, the study of elliptic functions and differential equations, and the rise of complex analysis. Analytic methods in number theory, incipient in the work of Euler, were put to dramatic use by Dirichlet and Riemann; for example, in Dirichlet's proof that there are infinitely many primes in any arithmetic progressions in which terms are relatively prime, and in Riemann's studies of the zeta function. Of course, there was the now familiar "rigorization" of analysis, carried out by Bolzano, Cauchy, Weierstrass, Dirichlet, and others. This resulted in the modern epsilon-delta definitions of limits, continuity, differentiation, and integration, which grounded calculus on a foundation free of Leibniz' infinitesimals and Newton's fluxions and fluents.

Finally, there was a good deal of work of a foundational nature. Bolzano's 1851 *Paradoxes of the Infinite* (published posthumously) can be seen as an early call to buttress the foundations of mathematics. The rigorization of analysis was one contribution to this general program, and was complemented by efforts from Dedekind, Cantor, Kronecker, etc., to move the conception of the real numbers from a geometric foundation to a new foundation based on the natural numbers. In addition, modern logic was born with the work of Frege, Boole, and Peirce. Towards the end of the nineteenth century, Frege and Dedekind concerned themselves with the foundations of arithmetic; Pasch addressed the foundations of geometry; and Peano and the "Italian school" pushed for a symbolic and axiomatic treatment of vast tracts of mathematics.

To help make sense of these developments, let us identify a few important and interrelated trends. The first is a general shift towards abstraction, accompanied by a growing division between mathematics and the physical sciences. Geometry evolved from the study of "space" to the study of general "spaces." Algebraic developments emphasized not the study of the natural or the real numbers, but rather, the study of general algebraic systems, with a clear emphasis on the fact that such systems can carry multiple interpretations. Moreover, there was an increasing focus on general descriptions and abstract characterizations of mathematical objects. This can be seen, for example, in the characterization of geometric spaces by groups of transformations, or by their intrinsic metric properties; and in the characterization of fields by groups of automorphisms. The various axiomatic approaches to algebra, arithmetic, and analysis that emerged towards the end of the century can also be understood in this way.

The new emphasis on abstract characterization brought with it a corresponding de-emphasis on calculation. This second shift was, in fact, not just a by-product of the new developments, but often a central goal. In a later retrospective on Dirichlet's work, Hermann Minkowski identifies and endorses this goal when he extols "the other Dirichlet Principle: to conquer the problems with a minimum of blind calculation, a maximum of clear-sighted thoughts."[7] Of course, the preference for abstract characterization over "blind calculation" was not universal and uncontroversial; we will return to a discussion of this issue later.

[7]From Minkowski's 1905 address in Göttingen, found in his *Gesammelte Abhandlungen*, volume 2, Leipzig, 1911. This passage is also quoted in in Stein [110], page 241.

A third notable trend is a growing confidence in dealing with the infinite. From Archimedes' method of exhaustion to the development of the calculus in the seventeenth and eighteenth centuries, statements about the infinite were viewed as statements about arbitrarily large quantities or limiting processes — that is, about "potential" infinities — while references to the "actual" infinite were treated with suspicion. Now, in contrast, completed infinitary structures were taken at face value. In the case of geometry, G. H. Hardy has emphasized the importance of understanding points at infinity as objects in their own right,[8] and much has also been written about the shift from the concept of a function as an analytic expression or rule to the concept of an "arbitrary" function, often attributed to Dirichlet.[9] Most centrally, by the 1880's a number of mathematical developments had contributed towards establishing a methodological framework for dealing with infinite totalities, including constructions of the natural numbers by Dedekind and Frege; constructions of the real numbers by Weierstrass, Dedekind, Cantor, and others; Dedekind's development of the theory of ideals in algebraic number theory; and Cantor's investigations of the transfinite.

The last trend we wish to highlight is a dramatically fruitful cross-fertilization between the various branches of mathematics. We have already noted several examples, such as the use of algebraic methods in geometry, number theory, analysis, and logic; and the use of analytic methods in geometry and number theory. With this increasing interaction came a growing interest in unification, as mathematicians tried to view the various aspects of their subject as part of a coherent whole. The desire for a general mathematical framework involved a good deal of reflection, as mathematicians tried to identify the common features of their various mathematical activities; and it involved an interest in rigor and explicitness, as general methodological standards for mathematics emerged.

2.2 Changing views of mathematics

The developments we have just discussed can be seen as changes that are internal to mathematical practice. But they both precipitated and were facilitated by a shifting understanding of the nature of mathematics itself, first, at the level of informal views held by mathematicians as to the general goals and the proper methods of their subject, and, later, at the level of more extended philosophical reflection.

Before the nineteenth century, the subject of mathematics was commonly divided into two branches, the science of number and the science of space. Both of these were sometimes subsumed under the general characterization of mathematics as the science of measure, magnitude, or quantity. Thus, in the seventeenth century, Descartes observed that only those disciplines that involve order or measure are regarded as "mathematical," and suggested recognizing a "pure" branch of the subject:

[8] Hardy [53], Appendix IV, pages 502–3.

[9] But see Monna [86] and Youschkevich [122] for a fuller discussion.

> It then follows that there must be a certain general science which explains everything which can be asked about order and measure, and which is concerned with no particular subject matter, and that this very thing is called "pure mathematics" [*mathesis universalis*].[10]

In the next century, Euler wrote:

> Mathematics, in general, is the science of quantity; or, the science that investigates the means of measuring quantity.[11]

And Gauss maintained:

> Mathematics really teaches general truths which concern the relations of magnitudes.[12]

Nineteenth century developments, however, strained these definitions. To begin with, the multiplicity of geometric spaces, including higher dimensional ones, that were now within the ambit of mathematical research indicated that geometry could no longer be viewed as the study of physical space, and highlighted the fact mathematics need not have direct application to the physical world. The acceptance of infinitary notions, like points at infinity, algebraic ideals, and transfinite ordinals, also served to distance mathematics from direct application to the physical world, and stretched the notions of "quantity" and "magnitude" considerably.

Of course, the view of mathematics as an abstract science was not new in and of itself. It was commonly accepted that mathematical abstractions could be assigned multiple interpretations; for example, Eudoxus' theory of proportions could be applied to different kinds of magnitudes, including lengths, areas, and volumes; various astronomical systems could be described as instances of, say, circular or elliptic motion; and the methods of probability could be applied, alternatively, to problems involving gambling, problems of insurance, and problems having to do with measurement subject to errors. But as mathematical abstractions were applied increasingly to the study of other mathematical objects, like equations or geometric spaces, the abstractions themselves began to take on a life of their own. What was sought after, therefore, was a general characterization of mathematics that could accommodate the new developments.

One route to such a characterization involved altering the conception of the subject matter of mathematics, i.e. revising the view as to what mathematics is *about*. This is illustrated by a proposal due to Boole, in *The Mathematical Analysis of Logic* (1847). Boole begins by acknowledging the traditional view (here, "Analysis" refers to mathematical activities in general):

> Thus the abstractions of mathematical Analysis, not less than the ostensive diagrams of ancient Geometry, have encouraged the notion,

[10] From "Rule four" of Descartes [27], page 17.

[11] From "On Mathematics in General", reprinted in [100], pages 37–41, source and translator not named. The quoted passage is from p. 37.

[12] Gauss [43], page 42 of the reprinting in [100].

> that Mathematics are essentially, as well as actually, the Science of Magnitude.[13]

But he goes on to argue that this definition is too restrictive, and based on a sample of mathematical activities that is too small. He proposes the following redefinition:

> We might justly assign it as the definitive character of a true Calculus, that it is a method resting upon the employment of Symbols, whose laws of combination are known and general, and whose results admit of a consistent interpretation. That to existing forms of Analysis a quantitative interpretation is assigned, is the result of the circumstances by which those forms were determined, and is not to be construed into a universal condition of Analysis.[14]

That is to say, mathematics is to be viewed as the science of symbolic calculi, which may or may not have quantitative interpretations. With this definition, Boole stands firmly in the tradition of British algebraists, for whom an algebraic system consisted of a collection of symbols together with explicit rules governing their use. What is novel, however, is his adoption of a syntactic standpoint to characterize mathematics in general, by proposing to define mathematics as the science of such systems.

An alternative route to redefining mathematics is to focus on the abstract concepts used in mathematical descriptions and characterizations, rather than on their symbolic representations. This shift of emphasis suggests allowing mathematicians the latitude to study these concepts, independent from immediate questions as to their applicability in physics and other sciences. Something of this sort underlies Georg Cantor's famous characterization of mathematics, from 1883:

> Mathematics is in its development entirely free and is only bound in the self-evident respect that its concepts must both be consistent with each other and also stand in exact relationships, ordered by definitions, to those concepts which have previously been introduced and are already at hand and established.[15]

The context of this passage is a discussion of Cantor's new theory of transfinite numbers, but the same emphasis on freedom can be found in connection with traditional branches of mathematics as well. Thus Richard Dedekind writes in 1889:

> In speaking of arithmetic (algebra, analysis) as merely a part of logic I mean to imply that I consider the number-concept entirely independent of the notions or intuitions of space and time .... Numbers are free creations of the human mind...[16]

[13]Boole [14], page 4 of the original edition.
[14]Ibid.
[15]Cantor [20], Section 8, paragraph 4.
[16]Dedekind [25], pages 790–791.

After having put aside intuitions of space and time, Dedekind then characterizes the system of natural numbers purely "logically," and hence abstractly. His earlier 1872 characterization of the real numbers [24] has a similar character.

What we have arrived at, then, are two new conceptions of mathematics, both generalizing the narrower characterizations offered by Descartes, Euler, and Gauss above. To be explicit: one might view mathematics as the science of symbolic representation and calculation; or one might view mathematics as the science of conceptual reasoning about structures, characterized abstractly. These two conceptions are not necessarily incompatible, but they differ in important respects. In particular, at the very down-to-earth level of everyday mathematical practice, they suggest different styles of research, involving different methods and goals.

The divergence becomes more pronounced when one considers the views of Leopold Kronecker, for whom symbolic and algorithmic issues are central. In "On the concept of number" (1887), he writes:

> [A]ll the results of the profoundest mathematical research must in the end be expressible in the simple forms of the properties of integers. But to let these forms appear simply, one needs above all a suitable, surveyable manner of expression and representation for the numbers themselves. The human spirit has been working on this project persistently and laboriously since the greyest prehistory....[17]

This focus on symbolic representation pervades Kronecker's work.[18] But he does not just emphasize the general symbolic nature of mathematics, as Boole did; he places issues of calculation at the core of any mathematical pursuit. The corresponding methodological stance is nicely captured in the following passage, written by his student Hensel:

> He [Kronecker] believed that one can and must in this domain formulate each definition in such a way that its applicability to a given quantity can be assessed by means of a finite number of tests. Likewise that an existence proof for a quantity is to be regarded as entirely rigorous only if it contains a method by which that quantity can really be found.[19]

The gradual rise of the opposing viewpoint, with its emphasis on conceptual reasoning and abstract characterization, is elegantly chronicled by Stein[110], as part and parcel of what he refers to as the "second birth" of mathematics. The following quote, from Dedekind, makes the difference of opinion very clear:

> A theory based upon calculation would, as it seems to me, not offer the highest degree of perfection; it is preferable, as in the modern theory of functions, to seek to draw the demonstrations no longer

[17] "Über den Zahlbegriff," Kronecker [81], page 955.

[18] See, for example, [29, 30, 85] and the introductory notes on Kronecker in volume 2 of [31].

[19] Found in L. Kronecker, *Vorlesungen über Zahlentheorie*, K. Hensel ed., Teubner, Leipzig, 1901, page vi. Quoted in Stein [110], page 250, and Marion [85], page 191.

from calculations, but directly from the characteristic fundamental concepts, and to construct the theory in such a way that it will, on the contrary, be in a position to predict the results of the calculation (for example, the decomposable forms of a degree).[20]

In other words, from the Cantor-Dedekind point of view, abstract conceptual investigation is to be preferred over calculation.

The significance of the opposition between Dedekind and Kronecker becomes clearer when we consider how the two viewpoints result in very different approaches towards dealing with the infinite. On Kronecker's view of mathematics, one is only permitted to deal with infinitary notions, such as that of a real number or an ideal, via explicit symbolic representation; and one is then only allowed to consider operations and relationships that are algorithmically presented. On the view expounded by Dedekind, infinitary mathematical objects may be characterized abstractly and taken at face value, subject to constraints of "determinateness" and "consistency." This second — revolutionary and liberal — conception is backed by strong rhetoric, especially from Cantor:

> It is not necessary, I believe, to fear, as many do, that these principles present any danger to science. For in the first place the designated conditions, under which alone the freedom to form numbers can be practised, are of such a kind as to allow only the narrowest scope for discretion. Moreover, every mathematical concept carries within itself the necessary corrective: if it is fruitless or unsuited to its purpose, then that appears very soon through its uselessness, and it will be abandoned for lack of success. But every superfluous constraint on the urge to mathematical investigation seems to me to bring with it a much greater danger, all the more serious because in fact absolutely no justification for such constraints can be advanced from the essence of the science — for the *essence* of *mathematics* lies precisely in its *freedom.*[21]

It is important to be clear as to what is at stake in these debates. From a contemporary point of view, it is tempting to read into this opposition general disagreements about the ontology and epistemology of mathematics. In other words, one may try to find in the associated writings precursors of the philosophical stances that emerged more clearly in the early twentieth century: logicism, realism/platonism, formalism, finitism, intuitionism, etc. But although Cantor indulges in some metaphysical speculation, and although some of Dedekind's writings suggest a structuralist point of view, Kronecker has almost nothing to say about metaphysics; and all three writers are primarily concerned with the mathematical implications of the divergent viewpoints. The effects that these viewpoints have becomes evident when one compares the mathematical work of Kronecker and Dedekind: both developed theories of algebraic ideals building

[20] Dedekind, "Sur la théorie des Nombres entiers algébrique," in his *Werke*, volume 3, page 296; quoted in Stein [110], page 245.

[21] Cantor [20], Section 8, paragraph 5.

on the work of Dirichlet and Kummer, and both wrote on the foundations of arithmetic; but despite the similarity of subject matter, their approaches stand in stark opposition to one other.[22]

We are not suggesting that general ontological and epistemological considerations played no role in the late nineteenth century debates, but, rather, that such philosophical issues are often intertwined with mathematical ones. While nineteenth century changes in mathematical practice were fueled by developments internal to the subject, coming to terms with these changes required the more broadly philosophical task of characterizing mathematics and its goals more generally; and such reflection influenced the informal viewpoints and attitudes that guide choices of subject matter, problems, language, and methods.[23]

Finally, we want to emphasize that a real tension between abstraction and calculation does not arise until one tries to come to terms with the infinite and its proper role in mathematics. In the finitary realm, there is no sharp distinction between characterizing a mathematical structure uniquely and determining what properties its elements have; indeed, this is essentially what one means by calling a characterization "finitary." A distinction only arises when one deals with infinite structures. The difference is made clear by Turing's later discovery that the halting problem is unsolvable; this tells us that one can specify a subset of the natural numbers (that is, describe a set of natural numbers via reference to the infinite structure $\langle \mathbb{N}, 0, S, +, \times \rangle$) without having an algorithmic procedure to determine whether or not a number is in this set. We have made this distinction with the hindsight of a twentieth century analysis of computability and definability. But even without the modern distinction, in the nineteenth century it became clear that one could describe infinite sets and operations on them without explicit reference to algorithmic representation. In the wake of the Cantor-Dedekind revolution, these are the kinds of issues that mathematicians had to come to terms with.

2.3 Developments in logic

Nineteenth century developments in logic, though perhaps not quite as dramatic as the developments in mathematics, were still remarkable and substantive, easily refuting Kant's earlier claim that the logic of his time was complete and in no need of revision. In particular, the century brought the development of relational and full quantificational logic in the work of Frege; it brought innovative formal and algebraic studies of a number of systems of logic by Boole, Peirce, and their followers; and the end of the century brought axiomatic characterizations of various mathematical structures and related studies of mathematical reasoning, in the hands of Pasch, Dedekind, Peano, Hilbert, etc. Just as nineteenth

[22] See Edwards [28], Marion [85], and Stein [110].

[23] One might view these developments as the evolution of two divergent "styles" of doing mathematics, the first based on the understanding of mathematics as a science of abstract description and reasoning, and the second based on the understanding of mathematics as a science of symbolic representation and calculation. An analysis of this phenomenon, akin to Ian Hacking's analysis of "styles" in the sciences [52], may be illuminating.

century developments in mathematics altered common views of the subject, so too did developments in logic require a revised assessment of the field, as well as of its role in mathematics and philosophy.[24]

One thing that evolved were common views as to the relationship between logic and mathematics. At the beginning of this essay, we described two ways of interpreting the phrase "mathematical logic"; this distinction hinged on whether one views logic as a a study of mathematical reasoning, or as a branch of mathematics. We can characterize this succinctly as the difference between studying the "logic of mathematics" and studying the "mathematics of logic." A second, related issue is whether one's primary goals in pursuing the subject are philosophical or mathematical. From a philosophical point of view, one may aim to clarify, characterize, or justify the basic principles of reasoning; in particular, one may wish to provide a philosophical foundation for mathematics. From a mathematical point of view, the standards of success are likely to differ, emphasizing intrinsic mathematical interest, interaction with other branches of mathematics, and applicability to the sciences.

Various orientations towards logic can be found in the nineteenth century. For example, Frege's work is primarily, though not exclusively, motivated by philosophical, foundational goals. In *The Foundations of Arithmetic*, his most programmatic work, he writes with regard to the relationship between logic and arithmetic:

> The aim of proof is, in fact, not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into the dependence of truths upon one another. The answers to the questions raised about the nature of arithmetic truths – are they a priori or a posteriori? synthetic or analytic? – must lie in this same direction.[25]

In the background here are traditional philosophical questions as to the status of various kinds of truths, such as those of arithmetic, especially in the context of Kant's philosophy of mathematics and of nineteenth century responses to it.

In contrast, Boole's work represents a more mathematical perspective. In the previous section, we considered Boole's expansive definition of mathematics as the general science of calculation. Now we can note that Boole's explicit reason for proposing this new definition is to argue for the inclusion of symbolic logic among the branches of mathematics:

> It is upon the foundation of this general principle, that I purpose to establish a Calculus of Logic, and that I claim for it a place among the acknowledged forms of Mathematical Analysis, regardless that in its object and in its instruments it must at present stand alone.[26]

[24]Much of the corresponding evolution is nicely and more completely chronicled by Peckhaus [88].

[25]Frege [41], pages 2–3.

[26]Boole [14], page 4.

Later, in the introduction to *The Laws of Thought* (1854), Boole supplements his mathematical analysis with a discussion of general philosophical issues. In fact, the further development of logic from Boole to Jevons, one of Boole's students, may be characterized as a further step in this general direction.[27] Nevertheless, taken as a whole, *The Laws of Thought* has more the flavor of an algebraic monograph than that of a philosophical one.

A further dichotomy that began to appear in logic was an incipient form of what we now refer to as the distinction between syntactic and semantic conceptions of mathematical reasoning. In Section 2.2, we distinguished two basic conceptions of mathematics that emerged in the nineteenth century: mathematics as the science of symbolic representation and calculation, versus mathematics as the science of abstract characterization and reasoning. These orientations have a bearing on one's view of logic. If one chooses to emphasize the former, then logic can be construed as the science of symbolic representations of logical notions, and the means of calculating with them. This viewpoint underlies Leibniz' notion of a *calculus ratiocinator*, and is made explicit later in Alfred North Whitehead's *Universal Algebra* of 1898. There Whitehead writes:

> The ideal of mathematics should be to erect a calculus to facilitate reasoning in connection with every province of thought, or of external experience, in which the succession of thoughts, or of events, can be definitely ascertained and precisely stated. So that all serious thought which is not philosophy, or inductive reasoning, or imaginative literature, shall be mathematics developed by means of a calculus.[28]

In contrast, if one focuses on abstract characterization, one might look to logic to provide a formal means of describing mathematical objects and structures. Axiomatic characterizations of the natural numbers, the real numbers, and Euclidean space are important in this respect, even without a specification of the corresponding deductive rules. This view of the nature of mathematical logic helps explain two historical facts that may seem odd from a contemporary point of view: first, that Dedekind considered his characterization of the natural numbers to be a logical one, even in the absence of a deductive framework; and second, that Peano and his Italian school at the end of the nineteenth century set up elaborate symbolic frameworks for mathematics, while not feeling compelled to specify a corresponding formal system or rules of inference.

The divergence between syntactic and semantic viewpoints persisted in the twentieth century, as Huntington, Veblen, and others in the school of American "postulate theorists" presented axiomatic characterizations of various mathematical structures with a clear interest in semantic categoricity, but with only an informal notion of semantic consequence and no attempt to develop a deductive counterpart. In contrast, in Russell and Whitehead's *Principia Mathematica* one finds the development of an explicit, elaborate deductive system

[27]See Peckhaus [88].
[28]Ibid., page viii.

for mathematics, with little or no concern for a mathematical analysis of the underlying semantics.[29]

Thus, by the turn of the century a number of different dimensions or axes had emerged along which one might try to characterize work in mathematical logic, depending on the general goals and methods embodied in the work, and the perceived relationship to mathematics. Logicians today are used to distinguishing between semantic and syntactic notions, like truth and provability, or definability in a model and definability in a theory; and both philosophically and mathematically motivated branches of logic are mature, well-developed disciplines. But these distinctions were not as clear around the turn of the century, and a good deal of work was involved in sorting them out. In what follows, we will discuss Hilbert's role in this effort, and we will argue that he was uniquely positioned to bring the various trends and points of view together in a compelling and fruitful way.

3 Hilbert's proof theory

3.1 Hilbert as mathematician

When Hilbert addressed the Second International Congress of Mathematicians in Paris, in 1900, he was 38 years old, and was recognized as one of the foremost mathematicians of his day, rivaled in stature only by Poincaré. He had already achieved a stunning array of successes in a wide range of mathematical fields, and was a brilliant proponent of the new, abstract methods in mathematics.

In a series of papers between 1888 and 1893, Hilbert solved what was known as the "fundamental problem of invariant theory," and proved what are now known as Hilbert's basis theorem and the Nullstellensatz. His elegant but nonconstructive proof of the basis theorem is said to have elicited the comment "this is not mathematics, but theology!" from Paul Gordon, a central figure in the study of invariants.[30] In 1893, he began the *Zahlbericht* [57], a report on number theory that brought together and simplified work in the subject from Gauss to Dedekind and Kronecker, and lay a firm foundation for algebraic number theory. In 1944, Hermann Weyl called it a "jewel of mathematical literature": "Even today, after almost fifty years, a study of this book is indispensable for anybody who wishes to master the theory of algebraic numbers."[31] His *Foundations of Geometry*[32] provided axiomatic developments of Euclidean and non-Euclidean geometry, again providing a unifying framework for developments in the subject since Euclid. His work on integral equations and the foundations of physics, and his solution to Waring's problem, still lay ahead.[33]

[29] Russell and Whitehead were aware of Hilbert's and Veblen's work before writing the *Principia*, and both are mentioned in the preface to Russell's *Principles of Mathematics* [97], first edition, page viii. See here also Scanlan [99] and Awodey and Reck [5].

[30] Quoted in Blumenthal [13], page 394.

[31] Weyl [120], page 626.

[32] *Grundlagen der Geometrie*, Hilbert [58].

[33] Reid [95] is a very readable biography of Hilbert. For further biographical information,

Hilbert's work illustrates many of the themes that we have portrayed as characteristic of the nineteenth century revolution in mathematics: the shift of emphasis from symbolic representation to abstract characterization; the use of infinitary, nonconstructive methods; and the search for foundational unity. Though well aware of the revolutionary character of his work, Hilbert did not take Kroneckerian objections lightly, and was sensitive to constructive and algorithmic issues throughout his career. In 1931, recalling his formative years as a mathematician in Königsberg, he wrote:

> In those days we young mathematicians, *Privatdozenten* and students, played the game of transforming transfinite proofs of mathematical theorems into finite terms, in accordance with Kronecker's paradigm. Kronecker only made the mistake of declaring the transfinite mode of inference to be inadmissible.[34]

In this passage, Hilbert seems to play down the importance of having finitary proofs, in contrast to Kronecker, who held such proofs to be central to mathematics. But Kronecker's influence on the young Hilbert was probably greater than the elder Hilbert wanted to admit, as suggested by Hermann Weyl, in his obituary of Hilbert:

> When one inquires into the dominant influences acting upon Hilbert in his formative years one is puzzled by the peculiarly ambivalent character of his relationship to Kronecker: dependent on him, he rebels against him. Kronecker's work is undoubtedly of paramount importance for Hilbert in his algebraic period. But the old gentleman in Berlin, so it seemed to Hilbert, used his power and authority to stretch mathematics upon the Procrustean bed of arbitrary philosophical principles and to suppress such developments as did not conform: Kronecker insisted that existence theorems should be proved by explicit construction, in terms of integers, while Hilbert was an early champion of Georg Cantor's general set-theoretic ideas.... A late echo of this old feud is the polemic against Brouwer's intuitionism with which the sexagenarian Hilbert opens his first article on "Neubegründung der Mathematik" (1922): Hilbert's slashing blows are aimed at Kronecker's ghost whom he sees rising from the grave. But inescapable ambivalence even here — while he fights him, he follows him: reasoning along strictly intuitionistic lines is found necessary by him to safeguard non-intuitionistic mathematics.[35]

In the heat of the *Grundlagenstreit* of the 1920's, Hilbert grew increasingly critical of Kronecker, Brouwer, and others, whom he saw as restricting the freedom to exercise the new methods that he had done so much to develop. His

see, for example, Weyl [120], Blumenthal [13], and the introductory notes in volume 2 of Ewald [31]. For more on Hilbert's influential 1900 address in Paris, see [50].

[34] Hilbert [66], paragraph 9.

[35] Weyl [120], page 613.

writings and public lectures began to take on a more forceful and polemical tone, resulting in many of the rhetorical flourishes that are now associated with the era: "No one shall be able to drive us from the paradise that Cantor created for us."[36] "[N]o one, though he speak with the tongues of angels, will keep people from negating arbitrary assertions, forming partial judgements, or using the principle of excluded middle."[37] "Forbidding a mathematician to make use of the principle of excluded middle is like forbidding an astronomer his telescope or a boxer the use of his fists."[38] At the same time, as indicated by Weyl above, the metamathematical program that he developed from the late 1910s on can be viewed as a means of grounding mathematics in a way that meets even Kronecker's and Brouwer's strictures. Weyl comments on this tendency in Hilbert's work further when he writes: "With regard to what he accepts as evident in this 'metamathematical' reasoning, Hilbert is more papal than the pope, more exacting than either Kronecker or Brouwer."[39]

We have portrayed Hilbert as a revolutionary figure, a powerful proponent of the new methods in mathematics, yet, at the same time, sensitive to conservative mathematical norms. In the coming pages we will trace the development of his metamathematical program, and argue that one can view it not only as a response to the foundational crisis and the *Grundlagenstreit*, but also as a thoughtful means of diffusing some of the tensions raised by the new methods in mathematics; and, further, as a way of bringing together a number of the aspects of logic discussed in Section 2.3. It is a matter of historical speculation as to whether or not the proof-theoretic program would have become as important to Hilbert as it did had it not been for the foundational challenges of Brouwer, Poincaré, and Weyl. But we would like to emphasize that at least some of the issues that Hilbert's program was designed to address were salient long before the foundational crisis.

3.2 Logic and the Hilbert school, from 1899 to 1922

We have already noted that even Hilbert's early work was foundational in many respects. His *Zahlbericht* aimed as much to buttress the foundations of algebraic number theory as to extend the technical boundaries of the field, and *The Foundations of Geometry* aimed to clarify the most basic geometric notions. Of the twenty-three questions he posed in 1900, three of the ten discussed explicitly in the lecture concern topics that are clearly of a foundational nature: Cantor's continuum problem, the consistency of arithmetic, and a mathematical treatment of the axioms of physics. In addition, the negative solution to the tenth problem, on the solvability of Diophantine equations, required foundational developments in the theory of computability for its ultimate solution. The years to come will, of course, see Hilbert address logical and foundational issues more systematically.

[36] Hilbert [64], page 376.
[37] Hilbert [64], page 379.
[38] Quoted by Weyl in [120], page 639.
[39] Ibid., page 641.

In the last section, we described divergent viewpoints as to the nature and purpose of mathematical logic. We would now like to argue that the development of Hilbert's program, and associated developments in the Hilbert school, helped bring the various viewpoints together, and clarified them significantly. To support this claim, we will trace the development of logic in the Hilbert school, vis-à-vis these viewpoints, in the years preceding the mature presentation of Hilbert's *Beweistheorie*. A much fuller account of these events is provided by Wilfried Sieg in [105] and [106].

In much of Hilbert's early foundational work it is not yet clear which understanding of the nature of logic he has in mind. Consider *The Foundations of Geometry*. Hilbert's axiomatization can be viewed as a foundation for geometric reasoning, a perfection of Euclid's work, which had long been hailed as a model of mathematical rigor. But it differs from the kind of foundation offered by Frege's earlier work on logic and arithmetic, and from that offered by Russell and Whitehead in the *Principia* of 1910–1913, in an important way: in contrast to these other works, *Foundations* does not address the underlying deductive framework explicitly. In particular, while the geometric axioms are stated clearly and precisely in *Foundations*, there is no formal description of the logical language, nor any characterization of the appropriate rules of inference.

At the same time, much of *Foundations* suggests the use of the axiomatic method as a mathematical tool, a way of exploring geometric structures and comparing them with one another. This emphasis is discernible in the introduction:

> Consider three distinct sets of objects. Let the objects of the first set be called points and be denoted by A, B, C, ...; let the objects of the second set be called lines and be denoted by a, b, c, ...; let the objects of the third set be called planes and be denoted by α, β, γ, ...[40]

The structural emphasis becomes even more prominent later in the book when several interpretations of various subsets of the axioms are studied in detail. In his 1900 essay, "The concept of number,"[41] Hilbert provides a similar axiomatic treatment of the real numbers (strongly influenced by Dedekind [24]).

In both *Foundations of Geometry* and "The concept of number" there is an ambiguity as to whether Hilbert views his axioms primarily as providing a semantic characterization or a deductive foundation. In the case of the real numbers, he indicates that the axioms provide a categorical description of the structure in mind: "We therefore recognize the agreement of our number-system with the usual system of real numbers."[42] But with respect to both Euclidean geometry and the real numbers he also alludes to deductive aspects of the axiomatic method; for example, with respect to geometry, he requires that "the system of axioms is adequate to prove all geometric propositions."[43] Support-

[40] Hilbert [58], second page of the introduction.

[41] "Über den Zahlbegriff," Hilbert [59].

[42] Ibid., paragraph 14.

[43] Ibid., paragraph 3.

ing the deductive point of view, he adds that new geometric statements are only valid "if one can derive them from the axioms by means of a finite number of logical inferences."[44] He does not, however, elaborate on the notion of "derivation" or "logical inference."[45]

The ambiguities we have just discussed do not detract from the importance of the work; both *Foundations* and "The concept of number" are masterful, and constitute a landmark in foundations of mathematics. But it does make clear that there were conceptual issues that were still up in the air at the time.

A decisive shift is found in Hilbert's 1904 Heidelberg lecture, "On the foundations of logic and arithmetic,"[46] which addresses the problem of providing a consistency proof for arithmetic. The paper contains the first hint of moving from the search for a semantic consistency proof (giving a model) to a syntactic one, and a discussion of a rudimentary deductive system. But the discussion of the deductive system is sketchy and inadequate, and there is no clear distinction between the mathematical and metamathematical methods. Hilbert does allude to the circularity of justifying the fundamental laws of arithmetic using general mathematical principles, and presents his new approach as a means of avoiding this problem. But the approach left the door open for a strong attack by Poincaré, who emphasized the circularity of using mathematical induction to justify induction itself.[47]

Hilbert would not develop the conceptual wherewithal to formulate an appropriate response to Poincaré's criticism for more than a decade. After 1904, he turned his attention to other matters. But he never turned his back on foundational issues completely, and, indeed, gave lectures on mathematical logic often during this period.[48] Around 1917 he returned to a study of foundations in earnest, and that year witnessed two developments of paramount importance.

The first is a renewed, more detailed exploration of foundational aspects of the axiomatic method. In "Axiomatic thought,"[49] a lecture presented to the Swiss Mathematical Society, Hilbert discussed the unifying methodological role it could play in mathematics and the sciences, providing a "framework of concepts" and a "deepening of the foundations" (*Tieferlegung der Fundamente*) in any field of sustained inquiry. The talk is not a technical one, but Hilbert discusses specific applications of axiomatics in the theory of invariants, the foundations of geometry, number theory, algebraic geometry, classical mechanics, quantum mechanics, electrodynamics, and so on. What is crucial to our account is that, in this lecture, Hilbert explicitly distinguishes between mathematical and foundational uses of the axiomatic method. As mathematical uses, he cites showing that the axioms of geometry are consistent because the Euclidean plane can be interpreted in the arithmetic of the real numbers; or showing that a physical theory of radiation is consistent by interpreting its

[44] Ibid., paragraph 16.
[45] See Awodey and Reck [5] for further discussion.
[46] "Über die Grundlagen der Logik und der Arithmetic," Hilbert [60].
[47] Poincaré [90].
[48] Sieg [105], page 8.
[49] "Axiomatisches Denken," Hilbert [61].

basic concepts in terms of real analysis. He continues:

> The problem of the consistency of the axiom system for the *real numbers* can likewise be reduced by the use of set-theoretic concepts to the same problem for the integers; this is the merit of the theories of the irrational numbers developed by Weierstrass and Dedekind.
>
> In only two cases is this method of reduction to another special domain of knowledge clearly not available, namely, when it is a matter of the axioms of the *integers* themselves, and when it is a matter of the foundation of *set theory*; for here there is no other discipline besides logic which it would be possible to evoke.[50]

Hilbert's observation is that there is an important difference between justifying a mathematical theory using mathematical methods, and justifying the mathematical methods themselves. At this stage, he had not yet developed the metamathematical stance that would underlie his proof-theoretic program; indeed, his ensuing discussion mentions Russell's work and suggests that he has something more like the logicist program in mind. But the important point is that he had begun to grapple with the issue.

The second development of key importance is that Hilbert began to draw a clear line between syntactic and semantic notions in logic. These developments are insightfully chronicled by Sieg [105, 106], which includes a discussion of Hilbert's lecture notes from the winter semester of 1917–1918, prepared with the assistance of Paul Bernays. In these lectures, Hilbert notes that formal languages and theories can be interpreted in a mathematical structure, and, indeed, are usually designed with a particular interpretation in mind; yet he also emphasizes the fact that the corresponding deductive systems can be studied on their own, independent of any semantic interpretation. With respect to the syntactic calculus, Hilbert stresses that the rules of inference should be sufficient to capture all ordinary forms of mathematical argumentation, thus providing a formal model of deductive reasoning in mathematics.

Having separated deductive syntax from its semantics, Hilbert and Bernays were able to explore the relationship between the two. As Sieg points out, the completeness of propositional logic is proved in a footnote to the lecture notes, and a modern formulation (i.e. the assertion that every valid formula is provable, and conversely) appears in Bernays' *Habilitationsschrift* of 1918.[51] Furthermore, the question as to whether or not first-order logic is complete is explicitly presented in Hilbert and Ackermann's logic text of 1928,[52] which is in large part based on the 1917/1918 lecture notes.

Our account so far has emphasized two developments in Hilbert's thought on logic: a new emphasis on syntactic, deductive notions of logical consequence, and a shift to a foundational viewpoint regarding the role of logic with respect to mathematics. This is not to say that Hilbert abandoned the semantic and

[50] Hilbert [61], paragraphs 37–39.

[51] See also the discussion in Zach [123].

[52] Hilbert and Ackermann [68].

mathematical viewpoints; rather, with a more mature understanding of the conceptual issues at hand, by the 1920's Hilbert was able make the necessary distinctions, recognize the uses and merits of the various stances, and study the relationships between them. These developments mark a turning point in logic. Without recognizing a sharp distinction between syntax and semantics, the problem of demonstrating the completeness of first-order deductive systems could not even be posed, much less solved by Gödel in 1929. And without recognizing the distinction between mathematical and foundational roles of logic, Hilbert's metamathematical program of the 1920's, which involved using mathematical methods to prove the consistency of mathematical reasoning, would have been methodologically incoherent.

3.3 The emergence of "Beweistheorie"

In [105] and [106], Sieg goes on to provide an illuminating account of the step-by-step transition of the Hilbert school from the viewpoint of "Axiomatisches Denken" to the full development of the proof-theoretic program. First, there was an attempt to justify classical mathematics on Russell's logicist grounds, ending with a rejection of the problematic axiom of reducibility; then a brief flirtation with constructivism, quickly dismissed in light of the centrality of the law of the excluded middle to mathematics. By 1922, Hilbert had arrived at the program of justifying classical mathematics by means of a consistency proof, presented in "The new grounding of mathematics. First report."[53] Finally, a more mature statement of the program, with a more refined treatment of the metamathematical stance, appeared in "The logical foundations of mathematics" of 1923.[54]

The essence of Hilbert's metamathematical program is well known, involving the formal study of mathematical methods, represented by formal systems of deduction, *using* mathematical methods. Kleene summarizes Hilbert's contribution in that respect:

> To Hilbert is due now, first, the emphasis that strict formalization of a theory involves the total abstraction from the meaning, the result being called a *formal system* or *formalism* (or sometimes called a *formal theory* or *formal mathematics*); and second, his method of making the formal system as a whole the object of a mathematical study called *metamathematics* or *proof theory*.[55]

But Hilbert was also careful to distinguish the portion of mathematics treated as an *object* of study, via formal representation in a deductive system, from the mathematics *used* to study it. This distinction is crucial to understanding Hilbert's program, and it is one we will return to in Section 4.

Here we would like to point out that aside from the distinction between mathematics and metamathematics, the use of mathematical techniques to study

[53] "Neubegründung der Mathematik. Erste Mitteilung," Hilbert [62].
[54] "Die logischen Grundlagen der Mathematik," Hilbert [63].
[55] Kleene [71], pages 61–62.

mathematical methods of reasoning was in and of itself somewhat radical. To illustrate this point, consider Paolo Mancosu's discussion of the work of Heinrich Behmann, a student of Hilbert's. Mancosu writes of a talk on Russell and Whitehead's *Principia Mathematica*, given by Behmann in the Göttingen colloquium in 1914:

> Behmann began the lecture by stressing the fact that the term "Mathematische Logik" is ambiguous and can be used to characterize two different traditions. The first tradition consists in a general construction of logic by mathematical means ("Mathematik der Logik") and is associated with the names of Boole, Schröder, and partly Peano. The second tradition ("Logik der Mathematik") analyzes the role played by logic in the construction of mathematics. Behmann mentions Bolzano, Frege, and Russell as representative of this direction of work. Although PM belongs squarely in the second tradition, it also accounts for results developed in the first tradition. Thus, for Behmann, PM is the first unified account of these two traditions.[56]

It is likely that this perceived synthesis contributed to the interest in Russell's work that we find in Hilbert during this time.[57] One should note, however, that Russell's synthesis is at best only partial, and, in particular, does not consider deductive systems as syntactic mathematical objects in their own right. Russell, like Frege, never considered a formal deductive system independent of its intended mathematical interpretation, and neither Russell nor Frege recognized the importance of exploring the possibility of multiple interpretations.

Today it is hard to appreciate the novelty of Hilbert's approach. Modern set theorists are fully comfortable with the notion that one may view the Zermelo-Fraenkel axioms as a foundation for mathematics, and yet study various models of these axioms using ordinary mathematical methods. But in 1922 this notion would have seemed at least a little bit odd, a strange partnership between two quite distinct research programs: the first, of providing an axiomatic foundation for mathematics, in the tradition of Frege, Russell-Whitehead, and Zermelo; the second, studying logic as a branch of algebra or as a mathematical tool, in the tradition of Boole and Peirce. It required someone of Hilbert's breadth as a mathematician to realize that combining these two traditions would be fruitful; and it required someone of Hilbert's abilities to carry out the synthesis successfully. In the next section, we will discuss this synthesis in more detail.

[56] Mancosu [84], page 306.

[57] See also the discussion in Mancosu [82].

4 Formalism and Hilbert's program

4.1 Strong formalism

Hilbert's program of justifying abstract mathematics via finitary consistency proofs relies on the implicit assumption that the relevant portions of mathematic are adequately modeled by the formal deductive systems. One might read into this stance the view that classical mathematics *is* nothing more than a system of axioms and rules, in the sense that it is devoid of any content or characteristics that cannot be explained purely in terms of a formal system representing it. Such extreme views were criticized well before the development of Hilbert's *Beweistheorie*. For example, Brouwer writes in his 1912 paper,"Intuitionism and Formalism":

> The viewpoint of the formalist must lead to the conviction that if other symbolic formulas should be substituted for the ones that now represent the fundamental mathematical relations and the mathematical-logic laws, the absence of the sensation of delight, called "consciousness of legitimacy," which might be the result of such substitution would not in the least invalidate its mathematical exactness. To the philosopher or anthropologist, but not to the mathematician, belong the task of investigating why certain systems of symbolic logic rather than others may be effectively projected upon nature. Not to the mathematician, but to the psychologist, belongs the task of explaining why we believe in certain systems of symbolic logic and not in others, in particular why we are averse to the so-called contradictory systems in which the negative as well as the positive of certain propositions are valid.[58]

In light of Hilbert's writings from the 1920's, the formalism critiqued by Brouwer may appear to be a reasonable characterization of Hilbert's later views. For example, in "On the infinite,"[59] Hilbert seems to be quite explicit in denying much of abstract mathematics any "content" or "meaning." First, he distinguishes between formulas which communicate "contentual" finitary propositions, and more general formulas, that refer to the "ideal objects of the theory."

> But since ideal propositions, namely, the formulas insofar as they do not express finitary assertions, do not mean anything in themselves, the logical operations cannot be applied to them in a contentual way, as they are to the finitary propositions. Hence it is necessary to formalize the logical operations and also the mathematical proofs themselves....[60]

He soon adds:

[58] Brouwer [16], page 736.
[59] "Über das Unendliche," Hilbert [64].
[60] Hilbert [64], page 381.

> [W]e will be consistent in our course if we now divest the logical signs, too, of all meaning, just as we did the mathematical ones, and declare that the formulas of the logical calculus do not mean anything in themselves either, but are ideal propositions... [61]

It was rhetoric like this that led Russell to quip, years later:

> The formalists are like a watchmaker who is so absorbed in making his watches look pretty that he has forgotten their purpose of telling the time, and has therefore omitted to insert any works.[62]

When it comes to questions of meaning in mathematics, however, there are a number of factors that weigh against taking Hilbert's rhetoric at face value. The first, and most obvious, is that it belies Hilbert's passionate commitment to the subject. The formalist that Brouwer caricatures holds that mathematics is nothing more than a symbol game, and the task of justifying a particular choice of game is outside the mathematician's scope. But it is clear that, to Hilbert, mathematics is much more than a symbol game, with methods and rules of inference that are by no means arbitrary; the issue of whether or not to allow the use of the law of the excluded middle, for example, is of central *mathematical* importance. He writes, angrily, in response to the foundational challenges:

> What Weyl and Brouwer do amounts in principle to following the erstwhile path of Kronecker: they seek to ground mathematics by throwing overboard all phenomena that make them uneasy and by establishing a dictatorship of prohibitions *à la* Kronecker. But this means to dismember and mutilate our science, and if we follow such reformers, we run the danger of losing a large number of our most valuable treasures.[63]

Hilbert is critical of any attempts to restrict mathematical practice on purely philosophical or ideological grounds, and clearly feels that the task of determining the proper practice of mathematics lies within the mathematical community.

Moreover, Hilbert has too much respect for intuitive thinking, thoughtful reflection, and the role of the mind in mathematical and scientific activity to see mathematics as a mere game with symbols. Thus he observes:

> In our science it is always and only the reflecting mind [*der überlegende Geist*], not the applied force of the formula, that is the condition of a successful result.[64]

Elsewhere, Hilbert makes it clear that such a view is not incompatible with a formal viewpoint, since to proceed axiomatically means "nothing else than to

[61] Ibid.

[62] From the preface to the second edition of Russell [97], page vi.

[63] Hilbert [62], paragraph 10.

[64] Hilbert in a letter to Minkowski, quoted in Stein [110], page 242.

think with consciousness."[65] In 1927 he is even more forceful in indicating that the type of formalism he has in mind does not deny the importance of intuitive thought:

> The formula game that Brouwer so deprecates has, besides its mathematical value, an important general philosophical significance. For this formula is carried out according to certain definite rules, in which the *technique of our thinking* is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds.[66]

In other words, the formal systems under consideration are not *empty* symbol games; we are interested in them precisely because they reflect our intuitive or logical understanding.

How, then, can we account for Hilbert's more extreme proclamations, most salient in the articles "The new grounding," "Logical foundations," and "On the infinite"? It is helpful, in this regard, to keep the historical context in mind. We have already noted that Hilbert's *Beweistheorie*, put forth for the first time in "The new grounding," was conceptually quite novel. In particular, it relied, first, on the observation that one can separate syntax and semantics, and hence study uninterpreted deductive systems from a purely syntactic point of view; and second, on the observation that one can distinguish between the mathematical methods used in the metatheory and the mathematical methods modeled in the formal system under investigation. The force of these two observations is that one may use very weak, restricted, syntactic methods to study formal systems that embody much more powerful and abstract forms of reasoning. In addressing a community of mathematicians not used to making such distinctions, this point is a hard one to make; it is not surprising that Hilbert relies on rhetorical flourishes to drive the point home.

But we also have to place the three articles mentioned above in the context of the *Grundlagenstreit*, which raged in full force in the early 1920's.[67] It is worth noting that, for example, "Axiomatic thought," written in 1917, makes no mention of the foundational challenges by Brouwer and Poincaré, nor any mention of the paradoxes. But soon after, Brouwer's call to refound contemporary mathematics without use of the law of the excluded middle became more pointed, and in 1920 Weyl, Hilbert's best student, announced, "I now abandon my own attempt and join Brouwer."[68] By 1927, in "Intuitionistic reflections on formalism,"[69] Brouwer is openly hostile and sarcastic towards the Hilbert program. The scale of the confrontation is further illustrated by Hilbert's personal battles with Brouwer, culminating, in 1928, with the removal of Brouwer

[65] Hilbert [62], paragraph 14.
[66] Hilbert [65], page 475. This passage is also quoted in Sieg [102], page 171.
[67] The collection of writings in Mancosu [83] provides a sense of the intensity of the debate.
[68] Weyl [118], page 56 in the original, page 98 in translation; also quoted in [116], page 480.
[69] Brouwer [17].

from his editorship of the *Mathematische Annalen.* Amidst the polemical exchanges, it is unsurprising, though regrettable, to find substantial developments overshadowed by extreme rhetoric. It is interesting to observe that Hilbert's tone grows somewhat softer in his writings of the late 20's and early 30's, and his rhetoric becomes less belligerent.

4.2 A more tenable formalist viewpoint

As a philosophy of mathematics, formalism is usually understood to imply an ontological stance in which abstract mathematical objects are denied any real existence, as well as an epistemological stance in which mathematical knowledge and justification is to be explained solely in terms of formal derivability. But we have seen that Hilbert resisted such a strong identification of mathematical practice with formal deduction. In this section we will argue that the philosophical assumptions needed to justify Hilbert's metamathematical program are far weaker, and are largely independent of strong metaphysical claims.

To start with, Hilbert's strategy of justifying abstract mathematics using consistency proofs only requires the acceptance that informal mathematical proof in various mathematical domains is adequately modeled by formal deductive systems, in the sense that an informal proof of a theorem indicates the existence of a formal derivation of the theorem's formal symbolic representation. With this assumption, proving the formal system consistent using uncontroversial, finitary methods shows that the informal practice in non-contradictory. In other words, a finitary consistency proof provides a precise sense in which one has justified the abstract methods using more concrete ones. Furthermore, as Hilbert pointed out, the consistency of a formal axiomatic system for mathematics implies that any universal arithmetic statement that is derivable in the system is, in fact, true. Thus, a finitary consistency proof serves to transform any proof of a universal number-theoretic statement in the formal system in question into a finitary proof of the same statement. Once again, this serves to justify the abstract methods with respect to the more concrete ones; questions as to what mathematical objects "really" are need not enter the debate.

What is central, then, to Hilbert's metamathematical program is the clear separation of the mathematical framework being investigated from the mathematical framework used to investigate it. To bring out the philosophical significance of such a separation, it is helpful to compare Hilbert's position with that developed by Rudolf Carnap in the late 1920's and early 1930s, in connection with scientific procedure more generally.[70] Carnap's "syntactic" view of the sciences (which was, at least partly, influenced by Hilbert) can serve to illuminate further this central aspect of the view of mathematics provided by Hilbert's formalism.

From Carnap's point of view it is meaningful to speak of various "frameworks" in which science, including mathematics, is carried out. Carnap is careful to distinguish between questions that are "internal" to a given framework

[70] See Carnap [21] and, with respect to the distinction between "internal" and "external" questions, especially Carnap [22].

and questions that are "external" to it. Such a distinction is implicit in Hilbert's approach. On the "internal" level of mathematics, we are free to refer to abstract mathematical objects and structures, including infinitary ones. On the metamathematical level, however, we view mathematics "externally" — and from *that* point of view, mathematics is characterized by the fact that we can represent the language and rules of inference in a concrete, symbolic way. Internal to the framework, one is free to carry out mathematics unfettered by worries about meaning. Externally, one has a vantage point from which to evaluate the framework, in terms of overall usefulness, applicability, and, indeed, consistency. The important thing is to recognize that the two viewpoints are quite different in character, and can be kept separate.

How does Hilbert use this separation? For one thing, he notes that the formalist viewpoint provides a useful perspective from which to try to evaluate classical mathematics, from an external point of view. In particular, it allows one to pose the question of consistency in a clear, precise, and rigorous fashion. But we also see that, for Hilbert, this is not the only criterion that one can appeal to in justifying abstract mathematics externally; Hilbert is equally forceful in appealing to the beauty and elegance of the subject, and its fruitfulness and applicability to the sciences.

More pointedly, Hilbert's goal is to put down intuitionistic challenges on two counts. First, he appeals to pragmatic and aesthetic considerations, which he takes to weigh strongly in favor of a classical framework over an intuitionistic one. Second, he aims to justify the classical framework using intuitionistically acceptable methods. The effectiveness of this twofold line of attack is evidenced by Weyl's comments on Hilbert's 1927 lecture, spoken only seven years after Weyl had taken up the banner of intuitionism:

> [I] am very glad to confirm, there is nothing that separates me from Hilbert in the epistemological appraisal of the new situation thus created. He asserted, first of all, that the passage through ideal propositions is a legitimate formal device when real propositions are proved; this even the strictest intuitionist must acknowledge.[71]

We can extract from these considerations a more reasonable formalist stance, which we will call "weak" formalism, to distinguish it from the "strong" formalism lampooned by Russell and Brouwer. This stance rests simply on the acceptance of the fact that from an external point of view, a characteristic feature of mathematical practice is that it involves a level of rigor and precision that makes mathematical proofs amenable to formalization. Indeed, a large part of what makes a proof "mathematical," from this point of view, is that the assumptions can be stated clearly, the language can be modeled symbolically, and the rules of inference can be described explicitly. Furthermore, the weak formalist holds that this, together with the consistency of the system under consideration, is enough to justify the proof as "mathematics." In contrast to the strong formalist, the weak formalist does not maintain that mathematics has no

[71] Weyl [119], page 483.

further "meaning." But he or she *does* maintain that questions about meaning in mathematics are of an entirely different character from mathematical ones; they are external to the framework, not internal to it.

Hilbert's further observation is that even some external questions can be posed in a manner that is clear and precise enough to make them amenable to mathematical investigation; the question of the consistency of the framework is one such. Other external questions are less amenable to mathematization, such as those involving aesthetic and pragmatic issues. Hilbert does bring these issues into play as well, but he makes no efforts to treat them mathematically. Hilbert's point of view can therefore be summarized as follows: one should keep internal mathematical questions and external questions as to the propriety of the mathematical framework separate; when external questions can be turned into mathematical ones, do so; but for the results to be philosophically informative, one has to distinguish between the mathematical methods that are used externally from the ones that are being studied.

A significant by-product of the separation between internal and external questions is the relegation of issues of meaning to the external evaluation of the framework. Seen as such, Hilbert's formalism is at its core a way of insisting that the ordinary practice of mathematics is not dependent either on empirical facts and knowledge or on particular metaphysical assumptions. With respect to the independence from empirical questions, compare the following remark by Hans Freudenthal about Hilbert's axiomatic treatment of geometry:

> "Consider three distinct sets of objects..." – thus the umbilical cord between reality and geometry has been cut. Geometry has become pure mathematics, and the question whether and how it can be applied to reality is answered just as for any other branch of mathematics.[72]

With respect to the independence from metaphysics, we can, once more, turn to Weyl's 1927 conciliatory lecture. In this lecture he laments the failure of the intuitionist program to provide an immediate phenomenological meaning to mathematics, but seems to accept that the overall evaluation of the appropriateness of a particular mathematical framework must appeal to external standards:

> What "truth" or objectivity can be ascribed to this theoretic construction of the world, which presses far beyond the given, is a profound philosophical question. It is closely connected with the further question: what impels us to take as a basis precisely the particular axiom system developed by Hilbert? Consistency is indeed a necessary but not a sufficient condition for this. For the time being we probably cannot answer this question except by asserting our belief in the reasonableness of history, which brought these structures forth in a living process of intellectual development....[73]

[72]Freudenthal [42], page 111, translated by Erich H. Reck.
[73]Weyl [119], page 484.

The independence of mathematical practice from answering such questions is expressed very nicely by Howard Stein in the following remark:

> [Hilbert's] point is, I think, this rather: that the mathematical *logos* has no responsibility to any imposed *standard* of meaning: not to a Kantian or Brouwerian "intuition," not to finite or effective decidability, not to anyone's metaphysical standards for "ontology"; its *sole* "formal" or "legal" responsibility is to be consistent (of course, it has also what one might call a "moral" or "aesthetic" responsibility: to be useful, or interesting, or beautiful; but to this it cannot be constrained — poetry is not produced through censorship).[74]

We have characterized our "weak formalism" as a fairly minimal stance. One occasionally finds objections to a stronger formalist claim that mathematical proof can be identified with proof in a particular formal system, such as, say, Zermelo-Fraenkel set theory. Indeed, Gödel's incompleteness theorems seem to weigh strongly against such a stance. But the weak formalist only maintains that particular mathematical theories can be represented in formal frameworks, not that *all* such theories can be represented in a single overarching framework. One might also object to the claim that the formalizability of mathematical proof is the *defining* characteristic of mathematical practice, or even that it is the most important one. However, our weak formalist stance only assumes that this is *one* important characteristic, and that, therefore, the modeling of mathematics with formal deductive systems is informative from an external point view. Evaluations may vary as to the weight one places on formal characterizations, as mathematics certainly exhibits other important characteristics as well. But it seems that any satisfactory account of mathematical practice will have to take this formal nature into consideration, and so is likely to be compatible with a weak formalist point of view.

4.3 Clarifying the nature of the infinite

In the introduction to this essay, we claimed that it is impossible to fully appreciate Hilbert's program without looking beyond the narrow concerns of the *Grundlagenstreit.* And, in Section 2, we discussed tensions that emerged from the nineteenth century transformation of mathematics, stemming from divergent views of the subject as a science of abstract characterization and reasoning, on the one hand, and as a science of symbolic representation and calculation, on the other. Let us now see what a weak formalist viewpoint has to offer towards resolving this tension.

In Section 2.2, we quoted Kronecker's insistence that mathematical objects be defined in terms of the integers, for which "one needs above all a suitable, surveyable manner of expression and representation." Cantor summarizes such a viewpoint, as follows:

[74] Stein [110], page 255, emphasis in the original.

> But as for the irrationals, they ought in pure mathematics to receive a purely *formal* meaning, in that they as it were only serve as instruments to fix properties of groups of integers and to ascribe these properties in a simple and uniform manner. According to this opinion, the true material of analysis is exclusively formed from the finite integers, and all the truths which have been found in arithmetic and analysis, or whose discovery is hoped for, ought to be conceived as relations of the finite integers to each other; infinitesimal analysis and with it the theory of functions are held to be legitimate only to the extent that their propositions can be interpreted as provable by laws governing finite integers.[75]

While Cantor disagrees with this viewpoint, he is not entirely dismissive of it. Later in the same work he returns to it in connection with the problem of human understanding of the infinite:

> The finiteness of human *understanding* is often adduced as the reason why only finite numbers can be thought;...But if it turns out that the understanding can also in a determinate sense define and distinguish infinite, that is, *superfinite* numbers, then either the words "finite understanding" must be given an extended meaning (from which that conclusion can then no longer be drawn); or the predicate "infinite" must be in certain respects conceded to human understanding. The latter is in my opinion the only correct course.[76]

Cantor repeatedly suggests that a key factor in our understanding of the infinite is that infinitary objects stand in definite relationships to one another, "bound to each other and to the finite numbers by fixed laws."[77]

Apparently, we are faced with an epistemological dilemma: Cantor holds that, under suitable circumstances, we *can* have knowledge of infinitary mathematical objects, while Kronecker maintains that mathematical knowledge is restricted to objects that have finite representations. We find Hilbert sensitive to both sides of the debate. On the one hand, he is clearly attentive to Kroneckerian demands for "surveyability" and "representability." In "The New Grounding" he writes:

> [A]s a precondition for the application of logical inferences and for the activation of logical operations, something must already be given in representation: certain extra-logical discrete objects, which exist intuitively as immediate experience before all thought. If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation, their difference, their succession (like the objects themselves) must exist for us immediately, intuitively, as something that cannot be reduced

[75] Cantor [20], paragraph 6 of section 4.
[76] Ibid., paragraph 4 of section 5.
[77] Ibid., paragraph 1 of section 6.

> to something else. Because I take this standpoint, the objects of number theory are for me — in direct contrast to Dedekind and Frege — the signs themselves.... [78]

Hilbert returns to this requirement of "surveyability" and finite representability often during the 1920's. On the other hand, he recognizes that the limitation to finitary objects is insufficient for a full development of abstract mathematics.

> [W]e cannot conceive of the whole of mathematics in such a way. Already when we cross over into the higher arithmetic and algebra – for example, if we wish to make assertions about infinitely many numbers or functions – the contentual procedure breaks down.[79]

Hilbert's concern with "higher arithmetic and algebra" is, of course, part of his continued defense of abstract mathematics, in the tradition of Dedekind and Cantor.

For Hilbert, the formal, metamathematical stance provides a means of obtaining the best of both worlds.

> [W]e can achieve an analogous point of view if we move to a higher level of contemplation, from which the axioms, formulae, and proofs of the mathematical theory are themselves the objects of a contentual investigation. But for this purpose the usual contentual ideas of the mathematical theory must be replaced by formulae and rules, and imitated by formalisms.... In this way the contentual thoughts (which of course we can never do wholly without or eliminate) are removed elsewhere — to a higher plane, as it were; and at the same time it becomes possible to draw a sharp and systematic distinction in mathematics between the formulae and the formal proofs on the one hand, and the contentual ideas on the other.[80]

Note that in this passage, Hilbert does not go so far as to say that our thoughts of the infinite are not "contentual." Indeed, he indicates that the content of such thoughts *cannot* be removed, only moved "to a higher plane." Meanwhile, the formal axioms and rules lend concrete representation to the abstract ideas.

For Hilbert, these formal representations provide the key to resolving the epistemological dilemma. They do so by supplying the "surveyable manner of expression" demanded by Kronecker and the "fixed laws" required by Cantor, while, at the same time, supporting the use of abstract infinitary objects in modern mathematics. Thus, within the formal system, one is free to pursue Cantorian set theory; from a metamathematical point of view, formal proofs provide the theory with a concrete content. As a result, mathematicians can contemplate infinitary notions, and still meet Kronecker's epistemic demand for finite representability: we can know a theorem of abstract mathematics because we can grasp the finitely presented proof. This resolution is not exactly what

[78] Hilbert [62], paragraph 25.
[79] Ibid., paragraph 32.
[80] Ibid., paragraph 33.

Kronecker had in mind, since it is the elements of the formal system rather than the mathematical notions themselves that are presented concretely. But such a shift of focus from the objects of the theory to the formal framework is entirely consistent with the metamathematical stance.

It is important to note that this use of formalism does not preclude the possibility that there are other epistemological issues that should be addressed. Our weak formalist may, for example, inquire as to what justifies the particular choice of a mathematical framework, or its use in scientific inquiry. And nothing that Hilbert says denies the validity of such pursuits; indeed, it is here that he appeals to external, pragmatic factors, including fruitfulness and applicability to the sciences. Thus, when we view Hilbert's proof-theoretic program as a response to the particular issues raised by the Cantor-Kronecker debate rather than a comprehensive philosophy of mathematics, the metamathematical stance is seen to resolve a central epistemological problem without placing undue restrictions on the range of broader philosophical options.

5 Proof theory after Hilbert

5.1 Proof theory in the 1930's and 1940's

What became of proof theory, after Hilbert? Of course, Gödel's incompleteness theorems dealt a serious blow to Hilbert's plans of using finitary consistency proofs to rid the world, "once and for all, of the question of the foundations of mathematics as such."[81] By 1930, Heyting had presented a formalization of intuitionistic number theory, and it was not long before Gödel and Gentzen had observed independently that a simple translation can be used to interpret classical first-order arithmetic in its intuitionistic version. This made it clear that intuitionistic reasoning is not identical with finitary reasoning, since it was commonly accepted that classical first-order arithmetic went beyond finitary means. Reflecting on these developments years later, Bernays notes that these results came as a surprise, since they were "against the prevailing views at that time."[82]

Describing the ensuing development of proof theory, Bernays writes:

> An enlarging of the methods of proof theory was therefore suggested: instead of a restriction to finitist methods of reasoning, it was required only that the arguments be of a constructive character, allowing us to deal with more general forms of inference.[83]

In other words, proof theorists began to pursue a "modified Hilbert's program," which involved justifying classical methods on constructive grounds, or relative to constructive methods. Examples of this include Gerhard Gentzen's two consistency proofs for arithmetic using transfinite induction up to ε_0, published in

[81] Hilbert [66], paragraph 39.

[82] Bernays [10], page 502. See also von Neumann [117], especially pages 61–62, where the "intuitionistic" and "finitistic" are used interchangeably.

[83] Ibid.

1936 and 1938, respectively;[84] and Gödel's *Dialectica* interpretation of arithmetic in a quantifier-free theory of higher-type functionals, first presented in 1941, though not published until 1958.[85]

As the raging foundational battles began to die down, narrow questions of consistency alone began to seem less pressing, and proof theorists began to justify their work in terms of broader epistemological goals. As early as 1928, Hilbert wrote:

> [E]ven if one were not satisfied with consistency and had further scruples, he would at least have to acknowledge the significance of the consistency proof as a general method of obtaining finitary proofs from proofs of general theorems — say of the character of Fermat's theorem — that are carried out by means of the ε-function.[86]

Here Hilbert is referring to his epsilon-substitution method, the technical means by which he hoped to obtain a consistency proof. In 1930, in "The philosophy of mathematics and Hilbert's proof theory," Bernays writes:

> [T]he current discussion about the foundations of mathematics does not have its origins in a predicament of mathematics itself. Mathematics is in a completely satisfactory state of methodological certainty....
>
> The problematic, the difficulties, and the differences of opinion begin rather at the point where one inquires not simply about the mathematical facts, but rather about the grounds of knowledge and the delimitation of mathematics. These questions of a philosophical nature have received a certain urgency since the transformation the methodological approach to mathematics experienced at the end of the nineteenth century.
>
> The characteristic moments of this transformation are: the advance of the concept of set, which aided the rigorous grounding of the infinitesimal calculus, and further the rise of existential axiomatics, that is, the method of development of a mathematical discipline as the theory of a system of things with determinate operations whose properties constitute the content of the axioms. In addition to this we have, as the result of the two aforementioned moments, the establishment of a closer connection between mathematics and logic.
>
> This development confronted the philosophy of mathematics with a completely new situation and entirely new insights and problems.... The debate concerning the difficulties caused by the role of the infinite in mathematics stands in the foreground in the present stage of this discussion.[87]

[84] Gentzen [45] and [46].

[85] Gödel [49].

[86] Hilbert [65], page 474, also quoted in Delzell [26], page 113.

[87] "Die Philosophie der Mathematik und die Hilbertsche Beweistheorie," Bernays [9], pages 234–235.

This passage is striking: as early as 1930, Bernays declares that mathematics is in a "completely satisfactory state of methodological certainty"! According to Bernays, the goal is not to justify mathematics; rather, it is to come to philosophical terms with the nineteenth century transformation of mathematics, particularly with respect to modern methods of abstract characterization and reasoning, and with respect to modern methods of dealing with the infinite.

Beyond proving consistency, one might look to proof theory for a means of understanding abstract, infinitary methods in concrete, constructive, or even finitary terms. In addition to Hilbert and Bernays, Gentzen hints at this more general program at the end of his first consistency proof,[88] and expands on it in "The concept of infinity in mathematics."[89] Like Weyl, he distinguishes between "actualist" and "constructive" interpretations of the infinite, the former providing the abstract, but epistemologically problematic, basis for set theory. He notes that the set theoretic paradoxes point to the need to delimit the allowable modes of inference in abstract mathematics, and continues:

> I must say that to me the clearest and most consequential delimitation seems to be that given by the principle of interpreting the infinite constructively.
>
> We should nevertheless be reluctant to discard the extensive non-constructive part of analysis which has, among other things, certainly stood the test in a variety of applications in physics. *Hilbert* sees his *proof theory* as a means of resolving these difficulties. The theory is intended to clarify as far as possible the mutual relationship between the two interpretations of the infinite by means of a purely *mathematical investigation.*[90]

Here again we see a shift of focus from establishing consistency to "ascribing a constructive sense" to a priori non-constructive methods, and clarifying the relationship between the two interpretations of the infinite.

5.2 Proof theory in the 1950's

The trend of expanding beyond consistency issues in proof theory continued through the 1950's. Comparing developments in this decade to those of the 1920's and 1930's, Charles Delzell wrote in 1996:

> Back then, the threat of an inconsistency (say, in arithmetic) seemed plausible enough to justify a defensive build-up (though critics could call it "Cold War hysteria"). By 1950, however, the trepidation in foundations had largely died down, and the time was ripe to look for possible peaceful, commercial uses of the formidable ϵ-theorems.[91]

[88] Gentzen [45].
[89] "Der Unendlichkeitsbegriff in der Mathematik," Gentzen [44].
[90] Ibid., page 227.
[91] Delzell [26], page 114.

By 1958, Kreisel was openly critical of Hilbert's initial focus on consistency. To take its place, he proposed

> ... a different general program which does not seem to suffer the defects of the consistency program: *To determine the constructive (recursive) content or the constructive equivalent of the non-constructive concepts and theorems used in mathematics*, particularly arithmetic and analysis.[92]

By finding constructive interpretations of classical methods, according to Kreisel, "one not only understands 'ordinary' mathematics better, but obtains new theorems."[93] In later years, he states this shift of emphasis more forcefully. For example, he writes, with Takeuti, in 1974:

> It would be simply absurd to judge proof-theoretic work mainly, let alone solely, by its relevance to Hilbert's original programme, because this programme is itself of dubious relevance.[94]

And, looking back on his career in 1988:

> Like many others... I was repelled by Hilbert's exaggerated claims for consistency as a sufficient condition for mathematical validity or some kind of existence. But unlike most others I was not only attracted by the logical wit of consistency proofs... but also by the so to speak philosophical question of making explicit the additional knowledge provided by those proofs (over and above consistency itself).[95]

General assessments of the success of Kreisel's various proof-theoretic programs can be found in [87], but few will deny that his influence has been widespread.

An account of the development of proof theory after the 1950's would supplement a discussion of Kreisel's work with a discussion of work by major figures like Solomon Feferman, William Tait, Kurt Schütte, and the Munich school of ordinal analysis. Moreover, we have not discussed a number of important developments in proof theory in the 1930's and 1940's, most notably the work of Herbrand;[96] and we have given short shrift to the related study of intuitionism, and the metamathematics of constructive theories, stemming from the work of Brouwer's student Arend Heyting. But space does not allow us to trace the development of proof theory up through the present day, and so, for the most part, we will leave off our historical account here.[97] Before closing this section, however, we wish to consider one more person who was influential in furthering a formal, metamathematical point of view, namely, Stephen Kleene.

[92] Kreisel [78], page 155. The emphasis is Kreisel's.

[93] Ibid.

[94] Kreisel and Takeuti, Dissertationes Math. 118, page 35. Quoted in Arai [1].

[95] Kreisel [80], page 395.

[96] See Herbrand [55].

[97] See footnote 102 for some papers and surveys that might help fill the gap.

We began this essay with a quote from Kleene's *Introduction to Metamathematics*, the first edition of which appeared in 1952. A generation of logicians was introduced to metamathematics by Kleene's book, in much the same way that the previous generation was introduced to the subject by Hilbert and Bernays' *Grundlagen der Mathematik*.[98] In the introductory chapters, Kleene is thorough in acknowledging the influence of Hilbert and Bernays, and his metamathematical methodology is set forth clearly in Chapter III: the formalization of mathematics, the separation of theory and metatheory, and the use of only finitary methods in the metatheory. There is only one respect in which Kleene distances himself from a narrow construal of the formalist program:

> We note in advance that metamathematics will be found to provide a rigorous mathematical technique for investigating a great variety of foundation problems for mathematics and logic, among which the consistency problem is only one. For example, metamathematical methods are applied now in studies of systematizations of mathematics arising from the logicistic and intuitionistic schools, as well as from Hilbert's.... Our aim in the rest of this book is not to reach a verdict supporting or rejecting the formalistic viewpoint in any preassigned version; but to see what the metamathematical method consists in, and to learn some of the things that have been discovered in pursuing it.[99]

In other words, Kleene resists yoking metamathematics to the narrow goal of assuring consistency, preferring to leave the goals broad and open-ended.

In fact, hints of such a broad view can already be found in Hilbert's "Axiomatic thought," delivered in 1917, just before the eruption of the *Grundlagenstreit*:

> When we consider the matter more closely we soon recognize that the question of the consistency of the integers and of sets is not one that stands alone, but that it belongs to a vast domain of difficult epistemological questions which have a specifically mathematical tint: for example (to characterize this domain of questions briefly) the problem of the *solvability in principle of every metamathematical question*, the problem of the subsequent *checkability* of the results of a mathematical investigation, the question of a *criterion of simplicity* for mathematical proofs, the question of the relationship between *content and formalism* in mathematics and logic, and finally the *decidability* of a mathematical question in a finite number of operations.[100]

In other words, Hilbert foresaw metamathematics as providing a mathematical framework for studying a wide range of philosophical issues. In the next section,

[98] Hilbert and Bernays [69].
[99] Kleene [71], page 59.
[100] Hilbert [61], paragraph 41.

we will consider some of the ways in which contemporary proof theory has borne out his more general hopes.

6 Proof theory today

6.1 Contemporary perspectives

Today, the phrase "proof theory" means different things to different people. At the end of *The Problems of Philosophy*, Bertrand Russell notes that when the basic concepts and questions driving a line of philosophical inquiry can be stated in a clear and precise fashion, the subject ceases to be called philosophy, and becomes a science.[101] In that sense, proof theory, like other branches of logic that were born of philosophical need, has evolved into a branch of mathematics in its own right. Seen as such, proof theory is the general study of deductive systems (mathematical or otherwise) and their properties, with internally motivated questions and research programs that are evaluated by ordinary mathematical standards. In a branch often described as *structural proof theory*, one studies structural properties of deductive systems, particularly with respect to transformations (or "reductions") of proofs; this is relevant to computational pursuits like automated deduction, where choices of representation are central. In the related branch known as *proof complexity*, one is interested in issues of efficiency, with respect to various measures of proof length; from that point of view deductive systems are thought of as being roughly equivalent when there are, say, polynomial-time computable transformations between them.

In this section, we will focus on what might be called *metamathematical* (or *traditional*) *proof theory*, the branch which is, perhaps, most closely linked to Hilbert's original use of the term. In this branch, formal deductive systems are used to explore the more general notion of mathematical provability, and the particular choice of deductive system is only relevant as a means to this more general end. Of course, the boundaries between the various subdisciplines are not sharp, and ideas and methods flow freely between them. So the metamathematical branch of proof theory should be viewed as embodying one perspective on the subject, one that informs, and is informed by, the others.

In discussing this metamathematical branch, we will try to convey a sense of the subject's goals and methods, its interaction with other branches of proof theory, and its interaction with mathematics, philosophy, and computer science. A full-scale assessment of even just this one branch of proof theory, however, lies well beyond the scope of this paper. Such an assessment would have to address the writings of a number of contemporary logicians and proof-theorists, as well as the writings of a number of philosophers that have been influenced by a proof-theoretic point of view.[102]

[101] Russell [96], page 155.

[102] For a small and scattered sample of views on proof theory, see: articles by Feferman [34, 35, 38, 39] and most of the essays collected in [36]; Girard [48]; Kohlenbach [72]; Kreisel [78, 79, 80] (see also the collection of essays on Kreisel in [87]); Rathjen [92]; Sieg [101, 103, 104, 107]; Simpson [108] and the introduction to [109]; and Tait [111, 112]. See also the recent

In Section 5.1, we saw that, since the 1920's, work in metamathematical proof theory has broadened its focus, from the specific goal of finding consistency proofs to the more general goal of understanding abstract mathematical reasoning in concrete terms. Modern proof theorists generally categorize formal mathematical frameworks as either *classical* or *constructive*. (The word "intuitionistic" is usually used interchangeably with "constructive.") Often the distinction hinges simply on whether or not the underlying logic includes the law of the excluded middle. For example, *Peano arithmetic*, a classical theory, consists of the consequences of a certain set of axioms in classical first-order logic; while *Heyting arithmetic*, a constructive theory, consists of the intuitionistic consequences of the same set of axioms. But in other cases the choice of language and axioms reflects more general methodological differences. Roughly speaking, the axioms of a "classically justified" theory are viewed as characterizing some portion of the mathematical universe, while the axioms of a "constructively justified" theory are viewed as characterizing mathematical constructions. So, for example, the axioms of Zermelo-Fraenkel set theory describe a universe of sets, while the rules of Martin-Löf type theory offer a general framework for reasoning about certain constructions, which have a clear computational interpretation.

For the purpose of studying aspects of classical mathematics, one needs to have appropriate formal frameworks. Metamathematical proof theory has much to offer in this respect, and we now know that a good deal of mathematics can be formalized in theories that are, from a set-theoretic perspective, quite weak. There is a long tradition, from Hilbert and Bernays, Weyl, and Heyting, through Takeuti, to the present day, of representing tracts of mathematics in such theories. More recently, the school of Reverse Mathematics, founded by Harvey Friedman and Stephen Simpson, has calibrated the strength of a large number of central mathematical theorems in terms of five weak subsystems of second-order arithmetic.[103] Similarly, proof theorists have studied fragments of set theory, subsystems and extensions of first-order arithmetic, theories of higher-order arithmetic, and Feferman's theories of "explicit mathematics."

Furthermore, we now have a robust understanding of such formal theories, from a number of perspectives. To start with, we have a clear sense of the foundational stances on which they are based. For example, there are a number of interesting theories that have the same strength as primitive recursive arithmetic, which many take to embody a reasonable interpretation of Hilbert and Bernays' informal notion of finitary proof; and Feferman, among others, has helped clarify the types of theories that are justified on the basis of a "predicative" stance arising from the early work of Weyl. We also have a clear understanding of the relationship between these theories and constructive ones, ranging from Heyting's formalization of first-order arithmetic to theories of intuitionistic inductive definitions and constructive type theories. In fact, one can

collection of essays [54]; and the online discussion [124], including a number of relevant postings by Harvey Friedman. These writings are, for the most part, reflections on proof theory; see [1, 19, 37, 113] and the references cited there for a survey of mathematical developments, providing a sense of the state of the subject today.

[103] See Simpson [109].

often find direct interpretations of the classical theories in their constructive counterparts.[104]

A particular metamathematical goal of contemporary research is to explore various measures of a theory's mathematical "strength." One such measure is its *consistency strength*: a theory T_1 is said to be stronger than T_2, in this sense, if T_1 proves that T_2 is consistent. Another approach is to characterize a theory's *computational strength.* To understand this second notion, suppose a theory T proves a statement of the form $\forall x \exists y R(x, y)$, where x and y range over the natural numbers, and R is a computationally decidable predicate. This tells us that a computer program that, on input x, systematically searches for a y satisfying $R(x, y)$ always succeeds in finding one. Now suppose that f is a function that, on input x, returns a value that is easily computed from the least y satisfying $R(x, y)$.[105] Then f is a computable function, and we can say that the theory, T, proves that f is totally defined on the natural numbers. A simple diagonalization shows that no effectively axiomatized theory can prove the totality of every computable function in this way, so this suggests using the set of computable functions that the theory *can* prove to be total as a measure of its strength. A third, similar, measure, arises naturally in the study of ordinal analysis: one can gauge the strength of a theory by the computable orderings that it proves to be well ordered. In each case, the underlying intuition is that the more infinitary axioms one adds to a theory, the more consequences one obtains "down below." These measures may not be the only way of characterizing a theory's strength, and do not address more general issues of expressiveness, simplicity, naturality, or fruitfulness. But they do provide at least one way of exploring a theory's capabilities, and can be viewed as "clarifying the nature of the infinite" by characterizing the computational and combinatorial consequences of these axioms.

Overall, while we have not entirely fulfilled Gentzen's goal of obtaining a constructive interpretation of the full-fledged set-theoretic conception of classical mathematics, we do have detailed constructive analyses of fragments that are close to mathematical practice. For example, with respect to the theories described above, we typically have exact characterizations of the types of computable functions that one can prove to be total, in terms of recursion along systems of ordinal notations, or in terms of natural computational and type-theoretic constructs. In sum, we now know that a good deal of ordinary mathematics can be formalized in theories that are considerably weaker than set theory; and we have a thorough understanding of such theories from a constructive point of view. In light of these developments, the schism between classical and constructive viewpoints that seemed so divisive in the 1920's now seems rather benign.

Proof theorists are often branded as reactionaries by the larger logic community as a result of the emphasis on studying restricted formal frameworks for mathematics. This stems from the viewpoint that proof theorists advocate the

[104] See, for example, Avigad [4].

[105] For example, $R(x, y)$ may assert that y codes a halting computation of a particular Turing machine on input x, and f may return the result of such a computation.

restriction of mathematical methods, as a means of guaranteeing consistency. But few proof theorists doubt the consistency of Zermelo-Fraenkel set theory, and even fewer maintain that such doubts should influence mathematical practice. The community of proof theorists does take it to be interesting that so much of ordinary mathematics can be carried out in weak theories,[106] but this by no means implies that abstract methods should be barred from mathematics, or that they are not fruitful, or even that they are less desirable than elementary ones. Indeed, some proof theorists, like Kreisel and Friedman, have argued that metamathematical methods may help us discover mathematical theorems whose proofs require stronger methods, like large cardinal hypotheses.

6.2 Proof theory and constructive mathematics

At this point, it is worthwhile to consider the relationship between proof theory and constructive mathematics, since the two subjects share many of the same concerns. A constructivist's perspective on the tension between abstract and concrete views of mathematics is nicely illustrated by the following quotation, from the introduction to Errett Bishop's *Foundations of Constructive Analysis* (1967):

> It appears then that there are certain mathematical statements that are merely evocative, which make assertions without empirical validity. There are also mathematical statements of immediate empirical validity, which say that certain performable operations will produce certain observable results, for instance the theorem that every positive integer is the sum of four squares. Mathematics is a mixture of the real and the ideal, sometimes one, sometimes the other, often so presented that it is hard to tell which is which. The realistic component of mathematics — the desire for pragmatic interpretation — supplies the control which determines the course of development and keeps mathematics from lapsing into meaningless formalism. The idealistic component permits simplifications and opens possibilities which would otherwise be closed. The methods of proof and the objects of investigation have been idealized to form a game, but the actual conduct of the game is ultimately motivated by pragmatic

[106] Indeed, Harvey Friedman has conjectured that every number-theoretic theorem appearing in the Annals of Mathematics from 1950 to the present day can be proved in elementary arithmetic, a fragment of first-order arithmetic so weak that it cannot prove the totality of iterated exponentiation (see "FOM: grand conjectures," April 16, 1999, on the online forum [124]). Some might judge this conjecture whimsical in its generality; but it is rendered somewhat more plausible by the fact that a good deal of algebra and analysis can be carried out in conservative extensions of elementary arithmetic (see, for example, [74, 109] or the lists of references in [3, 40]). Friedman's conjecture emphasizes the importance of distinguishing between proofs that are hard and proofs that require strong ontological or set-theoretic assumptions. For that reason, it seems to us that an interesting metamathematical question is as to whether Fermat's last theorem can be proved in elementary arithmetic, and, if so, how long such a proof must be.

considerations.[107]

The views expressed here resonate with the weak formalist stance that we presented in Section 4.2. In particular, the last sentence acknowledges the formal character of mathematics, without denying the importance of external, pragmatic considerations.

Bishop's work has arguably had as much effect on the tenor and tone of modern constructivism as Brouwer's. Bishop's *Foundations*, and later his book *Constructive Analysis* (co-written with his student Bridges), show that one can refashion parts of classical mathematics constructively, by restricting general definitions and restating them in constructive terms, modifying statements of classical theorems to render them constructively valid, and exercising additional care to ensure that proofs are direct, i.e. avoid the use of the law of the excluded middle.

On the surface, there are crucial differences between Bishop's program and metamathematical proof theory. Bishop-style constructivism involves reworking classical mathematics, in constructive terms; the Hilbert-style metamathematical approach is to maintain a classical mathematical framework, and use constructive methods in the metatheory. But in practice, the divisions are not sharp, and in many ways the two approaches have grown closer to each other. On the proof-theoretic side, formalizing classical mathematics in restricted theories typically involves adapting mathematics accordingly, for example, focusing on mathematical objects that can be coded as sets of natural numbers. Ideas from constructive mathematics are often useful in this regard. On the constructive side, in recent years there has been a good deal of interest in studying the metamathematical properties of formal systems that suffice to carry out the constructive development of mathematics, and endowing these systems with explicit computational semantics.[108] So now the two approaches share much in common, in trying to bridge the gap between abstract mathematics and concrete computation: both are concerned with developing mathematics using methods for which we have a constructive or computational understanding, in such a way that general classical intuitions are maintained.

6.3 Contemporary goals

We have characterized a general goal of metamathematical proof theory as that of modeling classical mathematical reasoning with formal systems, and studying those formal systems from a constructive point of view. Of course, various research programs harbor more specific goals, with orientations towards mathematics, philosophy, and computer science. In this section, we would like to complete our discussion by mentioning some of these more specific goals and their relation to Hilbert's original hopes.

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[107] Bishop [11], page viii.

[108] For more information on constructivism, see, for example, Bishop and Bridges [12], Beeson [7], Troelstra and van Dalen [114], or the recent survey, Bridges and Reeves [15].

A good deal of work in proof theory is devoted to foundational purposes, in the sense of providing a formal representation and analysis of the basic concepts and assumptions used in mathematics. Such formalizations bring mathematical benefits on their own, unifying disparate branches of the subject, clarifying concepts, and raising new mathematical questions.[109] Note, however, that in contrast to set theory, where the emphasis is on finding a framework broad and powerful enough to characterize as much of mathematics as possible in one fell swoop, proof theorists are typically more interested in studying *local* foundations for mathematics, i.e. systems that minimally suffice to capture various fragments of mathematical practice. For example, a proof theorist might be interested in determining the weakest theory in which one can develop a workable theory of real analysis, in the hopes that interesting constructive or computational information can be extracted from such a formal study. We might define "recursive analysis" to be the fragment of real analysis that can be carried out in RCA_0, the weakest of the five major subsystems of second-order arithmetic considered by the Reverse Mathematics school; a proof theorist is then interested in knowing which theorems of analysis can be carried out in this theory. The metamathematical analysis yields additional information: for example, anything provable in RCA_0 remains true when second-order quantifiers are assumed to range over computable sets; and any "computational" consequence of RCA_0 is witnessed by a primitive recursive function.

As Hilbert observed repeatedly, the formalization of mathematics also makes independence results possible. The Gödel-Cohen proofs of the independence of the axiom of choice and the continuum hypothesis from the axioms of Zermelo-Fraenkel set theory are a striking example, but there are others, both lower down as well as higher up in the lineup of mathematical theories. For example, recursive counterexamples to theorems in analysis tell us that these theorems are independent from RCA_0. Recently Razborov [93], building on work by Razborov and Rudich [94], considered the issue of the independence of lower bounds in circuit complexity from weak fragments of arithmetic. The possibility of obtaining natural combinatorial statements that are independent of interesting mathematical theories is also quite striking. The independence of the Paris-Harrington theorem from Peano arithmetic, and Friedman's proof of the independence of a finitary version of Kruskal's theorem from theories of predicative mathematics, are well known. The program of obtaining natural combinatorial independences from much stronger theories is actively being pursued by Friedman.[110]

There is continuing work towards characterizing the strength of formal theories in terms of the traditional measures described above. Powerful computational schemata are used to characterize the strength of increasingly strong theories; in the other direction, more restrictive classes of functions, like the polynomial-time computable ones, have been characterized in terms of weak fragments of arithmetic. Modern ordinal analysis draws heavily on intuitions

[109] See Feferman [32] for a nice discussion of the various benefits of formalization.

[110] See, for example, Friedman, "Boolean relation theory", March 10, 2000 posting to [124].

of the infinite that arise from the set-theoretic study of large cardinals, and we now seem to be close to having an ordinal characterization of the strength of full second-order arithmetic, which formed the backdrop for Hilbert and Bernay's formalization of analysis.[111] Other proof-theorists are pursuing Kreisel's goal of extracting useful mathematical information from classical proofs; for example, Ulrich Kohlenbach has applied proof-theoretic methods to extract bounds on rates of convergence of algorithms arising in the study of numerical approximations.[112]

We have already noted that the formal modeling of mathematical practice can be philosophically informative, by clarifying the types of reasoning that are justified under various philosophical stances and allowing us to explore the strengths and limitations of the corresponding methods. In recent years, many philosophers have turned their attention to issues that complement traditional foundational ones, seeking accounts of the growth and development of mathematical concepts, the structure of mathematical theories, mathematical explanation, and the interaction between pure and applied branches of the subject.[113] Issues like these can be fruitfully explored from a number of perspectives, and it is to be expected that formal metamathematical methods will add clarity and insight.

Finally, let us consider some aspects of proof theory that are more closely related to computer science. Some of the most vibrant areas of proof theory today address computational concerns: metamathematical analyses of type theory are central to the theory of functional programming languages, and the methods of structural proof theory are central in automated deduction, logic programming, and the design of expert systems.[114] As hardware and software becomes more complex, there has been increasing interest in formal methods of verification, yielding further possibilities for proof-theoretic applications. There are also striking relationships between the field of computational complexity and the field of proof complexity.[115] More broadly, a general interest in the storage, manipulation, and communication of mathematical knowledge requires a solid foundational understanding of mathematical language and proof.

Mathematics is a vital human pursuit, one we put great stock in; metamathematical proof-theory is nothing more than a sustained, rigorous study of this vast field of inquiry, with a particular focus. The general goal of the subject remains that of studying the nature of mathematical proof in various domains, in a way that is philosophically insightful, mathematically interesting, and computationally informative. What the various branches of the subject have in common is a focus on symbolic representations of mathematical language, inference, and proof. These concerns did not originate with Hilbert, but his *Beweistheorie* did

[111] For surveys of recent developments in ordinal analysis, see, for example, Pohlers [89] and Rathjen [92].

[112] See, for example, [74, 72], for an overview of the methods, and [73, 75] for applications.

[113] See, for example, Breger and Grosholz [51].

[114] See, for example, Constable [23], Jäger and Stärk [70], and Troelstra and Schwichtenberg [113].

[115] See Beame and Pitassi [6], Buss [18], Krajíček [77], Pudlák [91], or Urquhart [115] for a general overviews.

play an important part in making them central to modern mathematical logic. In that sense, Hilbert's program has been an enormous success.

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