Formalism

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Formalism is a philosophical theory of the foundations of mathematics that had a spectacular but brief heyday in the 1920s. After a long preparation in the work of several mathematicians and philosophers, it was brought to its mature form and prominence by David Hilbert and co-workers as an answer to both the uncertainties created by antinomies at the basis of mathematics and the criticisms of traditional mathematics posed by intuitionism. In this prominent form it was decisively refuted by Gödel's incompleteness theorems, but aspects of its methods and outlook survived and have come to inform the mathematical mainstream. This article traces the gradual assembly of its components and its rapid downfall.

1 Preliminaries

1.1 Problem of Definition

Formalism, along with logicism and intuitionism, is one of the "classical" (prominent early 20th century) philosophical programs for grounding mathematics, but it is also in many respects the least clearly defined. Logicism and intuitionism both have crisply outlined programs, by Frege and Russell on the one hand, Brouwer on the other. In each case the advantages and disadvantages of the program have been clearly delineated by proponents, critics, and subsequent developments. By contrast, it is much harder to pin down exactly what formalism is, and what formalists stand for. As a result, it is harder to say what clearly belongs to formalist doctrine and what does not. It is also harder to say what count as considerations for and against it, with one very clear exception. It is widely accepted that Gödel's incompleteness theorems of 1931 dealt a severe blow to the hopes of a formalist foundation for mathematics. Yet even here the implications of Gödel's results are not unambiguous. In fact many of the characteristic methods and aspirations of formalism have survived and have even been strengthened by tempering in the Gödelian fire. As a result, while few today espouse formalism in the form it took in its heyday, a generally formalist attitude still lingers in many aspects of mathematics and its philosophy.

1.2 Hilbert

As Frege and Russell stand to logicism and Brouwer stands to intuitionism, so David Hilbert (1862-1943) stands to formalism: as its chief architect and proponent. As Frege and Russell were not the first logicists, so Hilbert was not the first formalist: aspects of Hilbert's formalism were anticipated by Berkeley, and by Peacock and other nineteenth century algebraists. (Detlefsen 2005). Nevertheless, it is around Hilbert that discussion inevitably centers, because his stature and authority as a mathematician lent the position weight, his publications stimulated others, and because it was his energetic search for an adequate modern foundation for mathematics that focussed the energies of his collaborators, most especially Paul Bernays (1888-1977), Wilhelm Ackermann (1896-1962) and to some extent John von Neumann (1903-1957). As admirably recounted by Ewald (1996, 1087-9), Hilbert tended to focus his prodigious mathematical abilities on one area at a time. As a result, his concentration on the foundations of mathematics falls into two clearly distinct periods: the first around 1898–1903, when he worked on his axiomatization of geometry and the foundational role of axiomatic systems; and the second from roughly 1918 until shortly after his retirement in 1930. The latter period coincided with a remarkable flowering of mathematical talent around Hilbert at Göttingen, and must be considered formalism's classical epoch. It was brought to an abrupt end by Gödel's limitative results and by the effects of the National Socialist *Macht-ergreifung*, which emptied Germany in general and Hilbert's Göttingen in particular of many of their most fertile mathematical minds. In the foundations of mathematics, Hilbert's own writings are not as crystalline in their clarity as Frege's, and his successive adjustments of position combine with this to rob us of a definitive statement of formalism from his pen.

1.3 Working Mathematicians

Despite the consensus among mathematicians and philosophers of mathematics alike that Hilbert's program in its fully-fledged form was shown to be unrealizable by

Gödel's results, many of Hilbert's views have survived to inform the views of working mathematicians, especially when they pause from doing mathematics to reflect on the status of what they are doing. While their weekday activities may effectively embody a platonist attitude to the objects of their researches, surprisingly many mathematicians are weekend formalists who happily subscribe to the view that mathematics consists of formal manipulations of essentially meaningless symbols according to strictly prescribed rules, and that it is not truth that matters in mathematics as much as interest, elegance, and application. So whereas formalism is widely (whether wisely is another matter) discounted among philosophers of mathematics as a viable philosophy or foundation for the subject, and is often no longer even mentioned except in passing, it is alive and well among working mathematicians, if in a somewhat inchoate way. So formalism cannot be written off simply as an historical dead end: something about it seems to be right enough to convince thousands of mathematicians that it, or something close to it, is along the right lines.

2 The Old Formalism and its Refutation

2.1 Contentless Manipulation

As mentioned above, formalism did not begin with Hilbert, even in Germany. In the latter part of the 19th century several notable German mathematicians professed a formalist attitude to certain parts of mathematics. In conformity with Kronecker's famous 1886 declaration "*Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk*",[1] Heinrich Eduard Heine (1821-1881), Hermann Hankel (1839-1873), and Carl Johannes Thomae (1840-1921) all understood theories of negative, rational, irrational and complex numbers not as dealing with independently existing entities designated by number terms, but as involving the useful extension of the algebraic operations of addition, multiplication, exponentiation and their inverses so as to enable equations without solution among the natural (positive whole) numbers to have solutions. In this way whereas an expression like '(2 + 5)' unproblematically stands for the number 7, an expression like '(2 – 5)' has sense not by denoting a

[1] Reported in Weber 1893.

number –3 but as part of the whole collection of operations regulated by their characteristic laws such as associativity, commutativity, and so on. Such symbols may be manipulated algebraically in a correct or incorrect manner without having to correspond to their own problematic entities. The rules of manipulation on their own suffice to render the expressions significant.

In his ‘Die Elemente der Functionenlehre’ Heine wrote,

> “To the question what a number is, I answer, if I do not stop at the positive rational numbers, not by a conceptual definition of number, for example the irrationals as limits whose *existence* would be a presupposition. When it comes to definition, I take a purely formal position, in that I call certain tangible signs numbers, so that the existence of these numbers is not in question.” (Heine 1872, 173)

and Hankel writes in his *Theorien der komplexen Zahlensysteme*

> “It is obvious that when b > c there is no number x in the series 1, 2, 3, … which solves the equation [x + b = c]: in that case subtraction is *impossible*. But nothing prevents us in this case from *taking* the difference (c – b) as a sign which solves the problem, and with which we can operate exactly as if it were a numerical number from the series 1, 2, 3, ….” (Hankel 1867, 5).

Thomae’s *Elementare Theorie der analytischen Funktionen einer komplexen Veränderlichen* is particularly candid about this method, which he calls ‘formal arithmetic’. He considered that non-natural numbers could be

> “viewed as pure schemes without content [whose] right to exist [depends on the fact] that the rules of combination abstracted from calculations with integers may be applied to them without contradiction.”

It was Thomae’s fate to have Gottlob Frege as a colleague in Jena. Frege’s criticisms of the formalist position prompted Thomae to extend his introduction in the second edition in justification:

> “The formal conception of numbers sets itself more modest limits than the logical. It does not ask what numbers are and what they are for, but asks rather what we require of numbers in arithmetic. Arithmetic, for the formal conception, is a game with signs, which may be called empty, which is to say that (in the game of calculating) they have no other content than that which is ascribed to them regarding their behaviour in certain rules of combination (rules of the game). A chess player uses his pieces similarly: he attributes certain

> properties to them which condition their behaviour in the game, and the pieces are merely the external signs of this behaviour. There is inded an important difference between chess and arithmetic. The rules of chess are arbitrary; the system of rules for arithmetic is such that by means of simple axioms the numbers may be related to intuitive manifolds and as a consequence perform essential services for us in the knowledge of nature. […] The formal theory lifts us above all metaphysical difficulties; that is the advantage it offers." (Thomae 1898, 1.)

2.2 Frege's Critique

Frege was the old formalism's most trenchant and effective critic. In *Die Grundlagen der Arithmetik* (*Foundations of Arithmetic*), Sections 92-103, entitled "Other Numbers", he takes issue with those who would introduce new numbers simply to provide solutions to equations that were previously insoluble, as had Hankel and others, and as had been standardly practiced and preached by many mathematicians, including Gauss. Frege is unimpressed. Simply introducing new signs to do new things is inadmissible, since they could be introduced to perform contradictory tasks:

> "One might as well say: there are no numbers among those known hitherto that simultaneously satisfy the equations
>
> $$x + 1 = 2 \text{ and } x + 2 = 1;$$
>
> but nothing prevents us from introducing a sign that solves the problem." (Section 96.)

While ordinary numbers would yield a contradiction if they solved both equations, what is to say new numbers would also entail a contradiction? We could introduce them and see what happened. Frege does not admit free creation:

> "Even the mathematician can no more arbitrarily create anything than the geographer: he can only discover what is there, and give it a name." (Ibid.)

Since contradictions do not always show themselves easily, the "try and see" attitude will not suffice. The only way to show a theory consistent is to produce an object that satisfies it: a model.[2] The unwitting irony of these remarks would not emerge until

[2] Ibid., § 95.

1902, when Russell showed Frege that his own system contained a hidden contradiction.

A year after *Grundlagen*, Frege published in 1885 a short essay, "On Formal Theories of Arithmetic", which dealt again with the issues, though it did so without naming adherents to the formalist position. Contrasting formalism with his own logicist view, he criticises the formalists' theory of definition of numbers as either circular in presupposing the consistency of what is defined, which supposes the signs signify something after all, or else as impotent to secure the truth of the propositions that formal manipulations are supposed to underwrite. He also points out that the formalists are not thoroughgoing in their attitude, since they do not affer a formal theory of the positive integers: "usually one does not feel a need to justify the most primitive of numbers." (Frege 1984, 121.)

Russell's contradiction prevented Frege from completing his program of showing how all of arithmetic and analysis is logical in nature. The foundations of analysis were discussed in Part III, "The Real Numbers", of *Grundgesetze der Arithmetik* (*Basic Laws of Arithmetic*), volume II, published in 1903. Russell's Antinomy overshadows this second volume, and prevented the formal continuation, but before Frege introduced his own theory of real numbers he criticised in prose other extant theories, as he had done other theories of natural numbers in *Grundlagen*. The earlier book's wit and light touch are here replaced by protracted, sarcastic and tedious schoolmasterly lecturing of others, most particularly Thomae. Cutting away the redundant verbiage, Frege's criticisms come down to three further points. Firstly, the formalists are excessively cavalier about the distinction between signs and what they signify, ascribing properties of the one to the other and vice versa. Since they *identify* numbers with signs, this is to be expected. Secondly, for this reason, they are unable to distinguish between statements made *within* a formal context and statements made *about* a formal context. For example, when we say that a king and two knights cannot force checkmate, we have stated a well-known theorem of chess. But we have made a statement *about* chess, not a statement *within* chess. Chess positions and chess pieces do not have meanings: they are what they are, but do not state or say anything. (Frege 1903, Section 91.) By contrast, a mathematical statement has a meaning and states something. To suppose that a theory about the signs of arithmetic is a theory about numbers is to confuse statements within the language of arithmetic, *arithmetical statements*, with statements about the language of arithmetic, *meta-*

arithmetical statements (the terminology is modern, not Frege's). Finally the major difference between mathematical theories with content (such as arithmetic and analysis) and mere games is that mathematical theories may be applied outside mathematics: "It is application alone that raises arithmetic up above a game to the rank of a science." (Frege 1903, Section 91.)

Frege's major critical points—the importance of the sign/object distinction; the requirement of consistency; the difference between statement and metastatement; and the importance of application; lack of thoroughgoing application of the program—carried the day in the argument against the earlier formalists. They were however to be consciously noticed and incorporated into the more sophisticated kind of formalism put forward by Hilbert.

3 The New Axiomatics

3.1 Hilbert's *Grundlagen*

In 1899 Hilbert published his *Grundlagen der Geometrie*. This work was radically innovative in a number of ways. It established the basic pattern for axiomatic systems from that time on in modern mathematics. Although the subject matter—Euclidean geometry—was not new, Hilbert's way of treating it was. Axioms in Euclid and in the subsequent tradition were statements considered self-evidently true. In Hilbert this status is put aside. Axioms are simply statements which are laid down or postulated, not because they are seen to be true, but for the sake of investigating what follows logically from them. The choice of axioms is of course not arbitrary: the aim is to find axioms from which the normal theorems of geometry follow. Further, these axioms should be as few and simple as possible, they should contain as few primitive terms as possible, and they should be independent, that is, no one should be derivable from the remainder. Further, where Euclid postulated that certain constructions could be carried out, Hilbert stated the existence of certain geometrical objects.

3.2 Implicit Definition and Contextual Meaning

Hilbert's axiomatization constituted an advance in rigor over Euclid, since it did not depend on having separate suites of definitions, such as "a point is that which has no part"; postulates, such as "To draw a straight line from any point to any point"; and common notions" such as "The whole is greater than the part". In Hilbert, everything is set out in a system of 21 axioms (one was later shown to be redundant). There are three primitive concepts, *point, line* and *plane*, and seven primitive relations: a ternary relaion of *betweenness* linking points, three binary relations of *incidence* and three of *congruence.* Important axioms include Euclid's Parallels Axiom, and the Archimedean Continuity Axiom. Speaking in anticipation of later developments, the last means the system is not one of first order (where only individual points, lines and planes are quantified over) but second-order, where is is necessary to quantify over classes or properties of elements. In the course of his study, Hilbert lays stress on ensuring that the axioms are consistent, by producing a countable arithmetical model for them. Of course this only shows consistency *relative to* arithmetic, not absolute consistency. He showed that any two models are isomorphic, that is, in current terminology, that his axiom system is categorical. He also demonstrates the independence of axioms, again by using models, allowing different interpretations of the primitive terms.

The fact that the words 'point', 'line' and 'plane' are chosen for the three basic kinds of element is a concession to tradition. Their employment is inessential. As early as 1891, Hilbert remarked after hearing a lecture on geometry by Hermann Wiener that "Instead of 'points, lines, planes' we must always be able to say 'tables, chairs, beer mugs'." This distinguishes his approach to axioms from that of his predecessors and contemporaries. It is not required that the primitive terms have a fixed and determinate meaning. Rather, Hilbert regards them as being given meaning by the axioms in which they occur. He describes the axioms as affording an *implicit definition* of the primitive terms they contain, in terms of one another and the various logical components making up the remainder of the axioms.

The most important innovation in Hilbert's approach was, as Bernays put it later, to dissociate the status of axioms from their epistemological status. Axioms are no longer assumed to be true, as guaranteed by self-evidence or intuition. The approach is more liberal, and more experimental. A certain number of axioms are put forward, and their logical interrelations and consequences investigated. The enterprise takes on a hypothetical character rather than the categorical character traditionally assumed. The greater freedom this allows (and Hilbert constantly emphasized the mathematician's creative freedom) comes at a price however, since the loss of intuitive or evident guarantees of truth means the consistency of the axioms can no longer be taken for granted. This turns out to be the crux of the issues facing the new formalism later.

3.3 Dispute with Frege

Hilbert's work prompted a reaction from Frege, who wrote to him objecting to his treatment of axioms, definitions and geometry. Frege's part in their exchange of letters was published by Frege after Hilbert discontinued the correspondence, and when Korselt replied on behalf of Hilbert, Frege criticised him too. The exchange is illuminating both for what it reveals about the issues and for what it tells us about the relative positions of Hilbert and Frege in the German mathematical community.

Frege's view of axiom systems is staunchly Euclidean. Axioms are truths which are intuitively self-evident. Their being individual truths entails their being propositions having a determinate meaning (sense), in all their parts. Their being severally true guarantees their consistency with one another without need of a consistency proof. Definitions on the other hand are stipulations endowing a new sign with meaning (sense) on the basis of the pre-existing meanings of all the terms of the definiens. Hilbert's procedure of taking axioms not to be fully determinate in all their parts, and in considering that they severally define the primitive terms occurring in them, mischaracterizes both axioms and definitions, and unnecessarily blurs the distinction between them. For Frege it also blurs the important epistemological distinction between the truths of geometry, whose validating intuitions are geometric in nature, and so synthetic *a priori*, and the truths of arithmetic, which according to Frege are analytic, following from logic and suitable definitions.

Frege's positive characterization of Hilbert's position is illuminating. The conjunction of the axioms with the primitive terms 'point', 'line', 'between' etc. taken as distinct *free variables* gives on open sentence in several first-order variables, so a second-order open sentence. The question of consistency then becomes the question whether this open sentence can be satisfied. Hilbert's position is subtly different from this. Using modern terminology, we could say his view is that his axioms contain *schematic* first-order variables, so that valid inferences from them are schematic inferences after the fashion now familiar in first-order predicate logic, rather than subclauses in a true second-order logical conditional as they would be for Frege. The axioms and their consequences hold not just for a single system of things, the points of space, as Frege would have it, but for *any* system of things that satisfies the axioms. Consistency though would amount to the same thing: there can be a model.

However, this is precisely *not* how Hilbert saw the issue. In correspondence with Frege he writes 29 December 1899 (Simpson's translation):

> You write "From the truth of the axioms follows that they do not contradict one another". It interested me greatly to read this sentence of yours, because in fact for as long as I have been thinking, writing and lecturing about such things, I have always said the very opposite: if arbitrarily chosen axioms together with everything which follows from them do not contradict one another, then they are true, and the things defined through the axioms exist. For me that is the criterion of truth and existence. The proposition 'every equation has a root' is true, or the existence of roots is proved, as soon as the axiom 'every equation has a root' can be added to the other arithmetical axioms without it being possible for a contradiction to arise by any deductions. This view is the key not only for the understanding of my [*Foundations of Geometry*], but also for example my recent [*Über den Zahlbegriff*], where I prove or at least indicate that the system of all real numbers *exists*, while the system of all Cantorean cardinalities or all Alephs – as Cantor himself states in a similar way of thinking but in slightly different words – *does not exist*.

This is the clearest statement by Hilbert of a position which has become notorious: the view that, in mathematics, consistency is existence. It is clear why Frege could not accept Hilbert's view. For Hilbert, non-Euclidean geometry can be treated in just the same axiomatic way as Euclidean geometry, so since all three are consistent (relative to one another), all three are true and their objects exist. But for Frege they cannot all be true because they are mutually inconsistent: if one is true (Euclidean geometry, for Frege), the others are false, and their objects do not exist.

In Hilbert, truth is not absolute in the way it is for Frege. To say that the theorems of a system of geometry are true is for Hilbert to say that they follow logically from the axioms (assuming always the axioms are consistent). Finally, for Hilbert the axioms are subject to different interpretations, which he employs in independence proofs, whereas for Frege they must have a fixed meaning and cannot be reinterpreted. On these matters, while Frege makes his points clearly, it is he rather than Hilbert who is out of step with subsequent mathematical developments. Hilbert's treatment of axiom systems has become orthodoxy.

Hilbert did not continue the correspondence, bring unwilling to publish it, no doubt irritated by Frege's schoolmasterly and patronising tone, and after Frege published his part, the cudgels were taken up by Alwin Korselt, who attempted to mediate between the two positions. The result was another polemical piece by Frege against Korselt, in a much testier tone even than before.

3.4 The Axioms of Real Numbers

In 1900 Hilbert published a short memoir called 'On the Concept of Number'. In this he assembled into an axiom system a number of principles about real numbers which he had mentioned in the *Grundlagen*, characterizing the real numbers axiomatically as an ordered Archimedean field which is maximal, i.e., cannot be embedded in a larger such field. This was in effect the first axiomatization of the reals. He contrasts this axiomatic method with what he calls the *genetic* method, which is the successive introduction of extensions to the natural numbers, such as is found in Dedekind. His preference for the axiomatic method is clearly stated: "Despite the high pedagogic and heuristic value of the genetic method, for the final presentation and the complete logical grounding of our knowledge the axiomatic method deserves the first rank." (*vide* Ewald 1996, 1093.)

4 The Crisis of Content

4.1 Logicism's Waterloo and other Paradoxes

At the same time as Hilbert was proposing his axiomatization, Frege was, so he supposed, crowning his logicism program by showing how to derive the principles of the real numbers from purely logical principles, and establishing the existence of the real numbers by producing a model based on sequences of natural numbers, taken as already established as existing as a matter of logic in the previous volume of *Grundgesetze*. This was the task that Frege set himself in the third part, 'The Real

Numbers', of his monumental *Basic Laws of Arithmetic*. The thrust of Frege's approach unified two strands in previous thinking about the foundations of mathematics. One was his own logicism, which went back to Leibniz, and which he shared, in many respects, with his older contemporary Dedekind and (unknown to him at this stage) his younger contemporary Russell. According to logicism, the principles of mathematics—or as Frege less ambitiously believed, arithmetic and analysis—are logical in nature, and can be demonstrated to follow from logical principles alone. The second strand was the idea, going back to Gauss and Dirichlet, and also shared with Dedekind, that the arithmetic of finite numbers may in some way serve as the basic mathematical theory for grounding "higher" theories such as analysis. In order to vindicate his view, Frege had not only been inspired to create the first comprehensive modern system of logic; he had also been led to introduce a kind of entity called *value-ranges*, a species of abstract object whose existence is demanded by logic, and which includes, as a special case, the extensions of concepts, which Frege called *classes*. Numbers, according to Frege, are particular extensions of concepts, and so are classes in this sense.

The concept of number had in the preceding period been subject to an unprecedented development and enlargement by Georg Cantor. In his revolutionary works, Cantor, building on tentative beginnings by Bolzano, had begun to work with the general notion of a class or set, and had established that sets with infinitely many members need not all have the same size (cardinality), or number of elements. In particular the size of the continuum, that of all numbers on a continuous line, is greater than the size of the set of all finite natural numbers. Cantor's second proof of this result in 1891 uses a device now called the method of diagonalization; this was quickly generalized to show that for any size of set, another of greater size can be shown to exist, namely the set of all subsets of the former set (its power set), so that there is no greatest number. The theory of transfinite numbers to which this led was the most radical extension of the domain of arithmetic since its very beginning. However the very generality of the notion of size or cardinality of a set led to that curious result: there could not be a largest set, because if there were, by the diagonalization argument, there would have to be one larger still, contradicting the original assumption that there was a largest. Hence there could be no such set as the set of all things, for it would by definition have the largest cardinality. While this conclusion undercut an infamous attempt by Dedekind to prove that there is at least one infinite set, it did not give Cantor much concern. For theological reasons he was quite happy to accept that there were pluralities of things too numerous to be collected together into a set: he called them "inconsistent totalities".

The same indifference could not apply to Frege, whose logical system required him to quantify over all objects, including all sets, and for whom sets were included among the objects. Bertrand Russell, like Frege working with the idea of all objects, discovered in 1901 by considering Cantor's proof that there is no greatest cardinal number that a similar curious result could be derived concerning sets: according to logical assumptions he shared with Frege about the existence of sets, the set of all sets which are not elements of themselves would have to be an element of itself and also not an element of itself. Russell communicated this result to Frege in 1902, about a year after he had discovered it. Frege, disconcerted, hastily concocted a patched repair to his logical system for the publication of the second volume of *Basic Laws* in 1903, but the repair was unsuccessful,[3] as Frege must soon have realised, since he thenceforth gave up publishing about the foundations of mathematics, and declared that the contradiction showed set theory to be impossible. Russell's Paradox was also independently discovered by Ernst Zermelo at about the same time, but unlike Russell, Zermelo did not think it worth mentioning in a publication.

Russell's Paradox, though the clearest and most damaging, was but one of a cluster of paradoxes which had begun to infest post-Cantorian mathematics, starting with Cesare Burali-Forti's argument in 1897 that there could not be a greatest ordinal number. Cantor's result that there could be no greatest cardinal number followed in 1899. The general atmosphere conveyed by the rash of paradoxes coming to light was that modern mathematics was in a crisis. What had precipitated it was a matter for debate. Uncritical assumptions about the infinite, especially the uncountable infinite, or the assumption of the existence of objects not directly constructed, or the uncritical application of logical principles in an unrestricted context were three not unconected potential sources of the difficulties. All of these potential sources were to be confronted in the "classical" phase of formalism. The paradoxes also dramatically highlighted the importance of ensuring that mathematical theories are consistent.

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[3] This was first shown by Lesniewski: cf. Sobocinski 1949. Lesniewski showed that Frege's repair entails the unacceptable result that there is only one object. But Frege certainly must have realised fairly soon that the repair was also too restrictive to allow him to prove that every natural number has a successor, a crucial theorem of number theory.

4.2 Self-Restriction

Reactions to the paradoxes varied. Russell pressed forward with the attempt to maintain logicism, blocking the paradoxes by stratifying entities into logical types. Expressions of entities of different type could not be substituted for one another on pain of producing ungrammatical nonsense. Russell diagnosed the paradoxes as arising through vicious circles in definition, whose use was strongly criticised by Henri Poincaré. To avoid impredicative definitions, that is, those where the object defined is in the domain of object quantified over in the *definiens*, the types were themselves typed, or ramified, into infinitely many orders. However, this ramification, while it avoided impredicativity, did not allow standard mathematical laws to be derived, so the ramification was effectively neutralized by an axiom of reducibility, according to which every defined function is extensionally equivalent to one of lowest order in the type. The logical system Russell and Whitehead produced, under the influence of Peano and Frege, was the first widely recognised system of mathematical logic. The motivations for its complications were largely philosophical. By contrast, Hilbert's Göttingen colleague Ernst Zermelo produced for mathematical purposes (deriving Cantor's principle that every set can be well ordered from the axiom of choice) a surprisingly straightforward axiomatic version of set theory which retained most of Cantor's results, but by weakening the conditional set existence principles did not allow the formation of the paradoxical Russell set. Mathematicians showed themselves generally unwilling to accept the complications of the type system, and set theory quickly became the framework of choice for the then rapidly developing discipline of topology. Zermelo's achievement was a twofold vindication of the value of working with axiomatic systems as Hilbert had proposed: it largely silenced critics of set theory who had regarded it as a piece of mathematical extravagance, and it apparently avoided inconsistency, though that was (and is) still unproven.

Cantor's extension of arithmetic into the transfinite had been staunchly opposed by Leopold Kronecker, who propounded the principle that all mathematical objects were to be constructed from the finite integers. Kronecker's insistence on constructing mathematical objects was seconded for more philosophical reasons by L. E. J. Brouwer, who first used the terms 'formalism' and 'intuitionism' in 1911. By 1918, Brouwer had rejected the uncountably infinite as well as unrestricted employment of the law of excluded middle, in particular its use in infinite domains. Similar and at the time more influential views were put forward by Hilbert's former student Hermann Weyl in his 1918 book *The Continuum*, developing a logical account of analysis which used only predicative principles, and avoided using the axiom of choice or proofs by *reductio ad absurdum*. Coming from a former Göttingen student, Weyl's book and his 1921 essay 'On the New Crisis in the Foundations of Mathematics' took the challenge of Brouwer's arguments directly to the doors of the Göttingen mathematicians, declaring, "Brouwer, that is the revolution.". It was their response, particularly that of Hilbert and his assistant Paul Bernays, that ushered in the intense but short-lived classical period of formalism.

5 The Classical Period

5.1 Preparations

The first outward response to the challenge of Brouwer and Weyl came in the form of two papers published in 1922: Hilbert's 'The New Grounding of Mathematics' and Benays' 'Hilbert's Significance for the Philosophy of Mathematics'. However, as Wilfried Sieg has emphasized, these papers emerged from a richer matrix of work in progress, and not merely as a response to the intuitionist challenge. After a period of over ten years in which Hilbert had concentrated on functional analysis and, under the influence of Hermann Minkowski, on the mathematics of physics, he returned to foundational issues. In 1917 he delivered a lecture course 'Principles of Mathematics', for which Paul Bernays, newly recruited to Göttingen from Zurich, produced lecture notes. Notes from these and subsequent lectures, later reworked by Hilbert's student Wilhelm Ackermann, became the basis for Hilbert and Ackermann's classic 1928 book *Mathematical Logic* (*Grundzüge der mathematischen Logik*), the first modern textbook of the subject. In the lectures, Hilbert, availing himself of the developments since Whitehead and Russell's *Principia mathematica*, gave a modern formulation of mathematical logic in what has become the standard form, separating propositional from predicate calculus, and first-order from higher-order predicate calculus. Metamathematical questions are posed such as whether the various systems of axioms are consistent, independent, complete, and decidable. Although Hilbert soon distanced himself from the foundationally suspect axioms of infinity and reducibility, for the first time he and the Göttingen school had a precise logical instrument with which to approach the revisionary challenge to mathematics posed by intuitionism.

5.2 Hilbert's Maximal Conservatism

Brouwer himself had pointed out that adopting the constructive viewpoint of intuitionism meant foregoing acceptance of such mathematical results as that every real number has an infinite decimal expansion. It soon became clear that the intuitionistic program, at this stage not cast in the form of an alternative logic, would involve a large-scale rejection of many well-established mathematical results as genuinely false. In addition, Brouwer's rejection of completed infinities meant that Cantor's transfinite revolution was to be repudiated wholesale. In time, this threatened loss of contentual mathematics was to cost Brouwer even the support of Weyl.

Short of inconsistency, Hilbert was not prepared to accept restrictions on what mathematics can be accepted. His goal indeed was, as it had been earlier, to provide an epistemologically respectable foundation for *all* mathematics, and that included not just traditional number theory, analysis, and geometry, but also the newly added regions of set theory and transfinite number theory. His program was thus conservative, in the sense of wishing to conserve accepted mathematical results, in contradistinction to the revisionism of Brouwer, Weyl and Poincaré. And his conservatism was *maximal*, in that *any* consistent mathematical theory was acceptable, whether or not the patina of time-honored acceptance clung to it. What was new was the way in which mathematics, including the new mathematics of the infinite, was to be defended. Hilbert decided to break radically with foundational attempts by Dedekind, Frege and Russell, and to beat the intuitionists at their own game.

5.3 Finitism

The sticking point in establishing the consistency of geometry, analysis and number theory had always been the infinite. Any attempt to transmit consistency from finite cases to all cases by a recursive procedure, such as that sketched by Hilbert in 1905, was subject to Poincaré's criticism that the consistency of inductive principles was being assumed, so that a vicious circularity was involved. Hilbert adopted a distinction and a strategy to circumvent this. The distinction was between reasoning *within* some part of mathematics, represented by an axiomatic system, and reasoning *about* the axiomatic system itself, considered as a collection of symbol-combinations. Any mathematical proof, even one using transfinite induction, is itself a finite combination of symbols. Provided the notion of proof can be regimented uniformly, a procedure which advances in mathematical logic since Frege gave reason to think could be done, then provided conceptions of logical derivation and consistency could be formulated which did not depend on the content of a mathematical theory but only on the graphical form of its formulas, as a formula *A* and its negation ~*A* differ only by the presence of the negation symbol, the question of consistency could be tackled by examination of the formulas themselves. A consistency proof for a given mathematical theory, suitably formalized, would show that from the given finite collection of axioms, each a finite combination of symbols, no pair of formulas could be logically derived which differed solely in that one was the negation of the other.

The reasoning about a mathematical system was *metamathematics*. In so far as such reasoning, aimed at establishing consistency of a system, considered only the shapes and relationships of formulas and their constituent signs, not what they are intended to mean or be about, it is concerned only with the *form* or *syntax* of the formulas. The theory of the formulas themselves however is not formal in this way: it has a content; it is about formulas! Poincaré's accusation of circularity could be circumvented provided any inductive principles used in reasoning about formulas are themselves acceptable: the status of formulas within the theory (as suspicious because inductive) now becomes irrelevant, because their meaning is disregarded.

Hilbert signals this turn to the sign as a radical break with the past:

> the objects of number theory are for me—in direct contrast to Dedekind and Frege— the signs themselves, whose shape can be generally, and certainly recognized by us […] The solid philosophical attitude that I think is required for the grounding of pure mathematics—as well as for all scientific thought, understanding and communication—is this: *In the beginning was the sign*. (Hilbert 1922, 202; Mancosu 1998, 202)

Formulas are esentially simply finite sequences or strings of primitive symbols, so the kind of reasoning applied to them could be expected to be not essentially more complex than the kind of reasoning applied to finite numbers. Hilbert and Bernays called such reasoning “finitary”. The exact principles and bounds of finitary reasoning were nowhere spelled out, but the expectation was that combinatorial methods involving only finitely many signs could be employed to demonstrate in finitely many steps in the case of a consistent system that no pair of formulas of the respective forms *A* and ~*A* could be deduced (derived) from the axioms. This hope—for hope it was—turned out to be unrealizable.

Formalism’s finitism was not simply an exercise in hair-shirt self-denial. Brouwer’s and Weyl’s criticisms of classical mathematical reasoning stung the formalists into a more extreme response. While intuitionists rejected certain forms of inference, and also uncountable infinities, they were prepared to use countably infinite sequences. Finitism went further in its rejection of infinitary toools, and looked to achieve its results using only finitely many objects in any proof. This was the point of the turn to symbols. It is possible to formulate many a short quantified sentence of first-order logic using just one binary relation, such that these sentences cannot be true except in an infinite domain. The infinite is then “tamed” by any such sentence. If formalized theories of arithmetic, analysis etc. could be shown consistent using finitely many finitely long sentences in finitely many steps, then even the uncountable infinities of real analysis that intuitionism rejected would be “tamed”, and by sterner discipline than the intuitionists themselves admitted. Finitism was thus in part an exercise in one-upmanship.

5.3 Syntacticism and Meaning

Consistency of a formal theory (essentially, a set of formulas, the axioms, with their consequences) can be defined in terms of the lack of any pair of formulas *A* and ~*A* of the theory, both of which derive from the axioms. This characterization depends solely on the graphical fact that the two formulas are exactly alike (type-identical) except that one has an additional sign, the negation sign, at the front. The process of proof or derivation is likewise so set up that the rules apply solely in virtue of the syntactic form of the formulas involved, for example *modus ponens* consists in drawing a conclusion *B* from two premises *A* and $A \rightarrow B$, no matter what the formulas *A* and *B* look like *in concreto.* Likewise other admissible proof steps such as substitution and instantiation can be described in purely syntactic terms, though with somewhat more effort. This metamathematical turn was inmany respects the most radically revolutionary part of formalism: it consisted in treating proofs themselves not (simply) as the vehicles of mathematical derivation, *but as mathematical objects in their own right*. It is ironic indeed that while the general idea of formalization was well understood by the formalists, the implications of the formal nature of proof only became apparent when Gödel showed in detail how to encode these formal steps in arithmetic itself, which was precisely what set up the proof that there could be no finite proof of arithmetic's consistency.

Nevertheless, the oft-repeated charge that according to formalists mathematics is a game with meaningless symbols is simply untrue. The metamathematics that deals with symbols is meaningful, even though it abstracts from whatever meaning the symbols might have. And in the case of an axiomatic system like that for Euclidean geometry, the axioms (provided, as ever, that they are consistent) themselves limit what the symbols can mean. Though in general they do not fix the meanings unambiguously, this very constraining effect gives the symbols a schematic kind of meaning, which it is the task of the mathematician to tease out by her inferences. That is the point of Hilbert's infamous view that the axioms constitute a kind of implicit definition of the primitive signs they contain. While for several reasons the word 'definition' aroused antipathy, the point is that the meaning is as determinate as the axioms constrain it to be, and no more. The "objects" discussed and quantified over in such a theory are considered only from the point of view of the structure of interrelationships that they embody, which is what the axioms describe.

6 Gödel's Bombshell

In their 1928 *Grundzüge der theoretischen Logik*, Hilbert and Ackermann formulated with admirable clarity the interesting metamathematical questions that needed to be answered. Is first-order logic complete, in the sense that all valid statements and inferences can be derived in its logical system? Are basic mathematical theories such as those of arithmetic and analysis, expressed in the language of first- or higher-order predicate logic, consistent? Hilbert had already begun to take steps along the way of showing the consistency of parts of natural number theory and real number theory, in papers in the early 1920s. The aim was to work up to the full systems, including quantifiers for the "transfinite" part, as Hilbert termed it. Ackermann tried unsuccessfully in 1924 to show the consistency of analysis, while Johann von Neumann in 1927 gave a consistency proof for number theory where the principle of induction contains no quantifiers. When Kurt Gödel in his 1930 doctoral dissertation proved the completeness of first-order predicate calculus, it appeared that the ambitious program to show the consistency of mathematics on a finite basis was nearing completion, and that number theory, analysis and set theory would fall in turn. In 1930 Gödel also started out trying to prove the consistency of analysis, but in the process discovered something quite unexpected: that it is possible to encode within arithmetic a true formula which, understood as being about formulas, "says" of itself that it cannot be proved. The formal theory of arithmetic was incomplete.

This in itself was both unexpected and disappointing, but Gödel's second incompleteness theorem was much more devastating to the formalist program, since it struck at the heart of attempts to show portions of formalized mathematics to be consistent. Gödel showed namely that in any suitable formal system expressively powerful enough to formulate the arithmetic of natural numbers with addition and multiplication, if the system is consistent, then it cannot be proved consistent using the means of the system itself: it contains a formula which can be construed as a statement of its own consistency and this formula is unprovable if and only if the system is consistent. Therefore any proof of consistency of the system can only be made in a system which is proof-theoretically *stronger* than the system whose consistency is in question. The idea of the formalists had been to demonstrate, given some system whose consistency is not straightforwardly provable (such as arithmetic with only addition or only multiplication as an operation), that despite its apparent strength it could be shown by finite formal methods that it is consistent. Gödel's Second Incompleteness Theorem showed to the contrary that no system of sufficient strength, and therefore questionable consistency, could be shown consistent except by the use of a system with greater strength and *more* questionable consistency. The formalist goal was destined forever to recede beyond the capacity of "acceptable" systems to demonstrate. Gödel himself offered a potential loophole to formalists, by suggesting that perhaps there were finitary methods that could not be formalized within a system. However, this loophole was not exploited, and the effect was simply to highlight the unclarity of the concept 'finitary', which has continued to resist clear explication. Other aspects of Gödel's proofs which have remained controversial concern the question in what sense the formula "stating consistency" of the system in the system in fact does state this.

It is usual to portray Gödel's incompleteness theorems as a death-blow to formalism. They certainly closed off the line of giving finitistic consistency proofs for systems with more than minimal expressive power. However they were if anything more deadly to logicism, since logicism claimed that all mathematics could be derived from a given, fixed logic, whereas Gödel showed that any logical system powerful enough to formulate Peano arithmetic—which included in particular second-order predicate logic, set theory, and Russell's type theory—would always be able to express sentences it could be shown were not provable in the system and yet which could be seen by metamathematical reasoning to be true. Logicists aside, most mathematicians were fairly insouciant about this: many had not believed logicism's claims in the first place.

The effect on formalism was more immediate but also ultimately more helpful. Hilbert's dream had proved untenable in its most optimistic form, but interest shifted to investigating the relative strengths of different proof systems, to seeing what methods could be employed beyond the finitary to showing consistency, to investigating the decidability of problems, and in general to further the science of metamathematics. Like a river in spate, formalism was obstructed by the Rock of Gödel, but it soon found a way to flow around it.

7 The Legacy of Formalism

7.1 Proof Theory

Hilbertian metamathematics initiated the treatment of proofs as mathematical objects in their own right, and introduced methods for dealing with them such as structural induction. In the 1930s a number of advances by different logicians and mathematicians, principally Herbrand, Gödel, Tarski and Gentzen, showed that there were a number of perspectives from which proofs could be investigated as mathematical objects. Probably the most important was the development of the sequent calculus of Gentzen, which allowed precise formulations of statements and proofs about what a given system proves. In Gentzen's treatment, the subject-matter of the formulas treated is irrelevant: what matters are the structural principles for manipulating them. Proof theory was to go on to become one of the most important pillars of mathematical logic.

7.2 Consistency Proofs

The first post-Gödelian consistency proof was due to Gentzen (1936), who showed that Peano arithmetic could be proved consistent by allowing transfinite induction up to ε_0, an ordinal number in Cantor's transfinite hierarchy. Later results by Kurt Schütte and Gaisi Takeuti showed that increasingly powerful fragments of mathematics, suitable for formulating all or nearly all of "traditional" mathematics, could be given transfinite consistency proofs. Any sense that the consistency of ordinary mathematics is under threat has long since evaporated.

7.3 Bourbakism

Hilbert's attitude to axiom systems, revolutionary in its day, has become largely unquestioned orthodoxy, and informs the axiomatic approach not just to geometry and arithmetic but all parts of (pure) mathematics. The reformulation of pure mathematics as a plurality of axiomatic theories, carefully graded from the most general (typically: set theory) to the more specific, propagated by the Bourbaki group of mathematicians, effectively took Hilbert's approach to its limit. As to the entities such theories are "about", most commentators adopt a structuralist approach: mathematics is concerned not with any inner or instrinsic nature of objects, but only with those of their characters which consist in their interrelationships as laid down by a given set of axioms. While this stresses the ontology of mathematics more than the formalists did, it is an ontology which is informed by and adapted to the changes in thinking about the axiomatic method which drove formalism. Not all mathematics is done in Bourbaki style, nor is it universally admired or followed, but the organisational work accomplished by the Bourbakist phase is of permanent value to an increasingly sprawling discipline.

8 Conclusion

In the "classical" form it briefly took on in the 1920s, formalism was fairly decisively refuted by Gödel's incompleteness theorems. But these impossibility results spurred those alrady working in proof theory, semantics, decidability and other areas of mathematical logic and the foundations of mathematics to increased activity, so the effect was, after the initial shock and disappointment, overwhelmingly positive and productive. The result has been that, of the "big three" foundational programs of the early 20th century, logicism and intuitionism retain supporters but are definitely special and minority positions, whereas formalism, its aims adjusted after the Gödelian catastrophe, has so infused subsequent mathematical practice that these aims and attitudes barely rate a mention. That must count as a form of success.

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General remarks: There is an extensive secondary literature on formalism. For the "horse's mouth" story the papers of Hilbert are indispensable, but be warned that though attractively written, they are often quite difficult to pin down on exact meaning, and his position does change frequently. Of modern expositions, for historical background and the long perspective it is impossible to beat Detlefsen 2005, while for more detailed accounts of the shifting emphasis and tendencies within formalism as it developed, the works by Sieg, Mancosu, Peckhaus and Ewald are valuable signposts. Sieg is editing the unpublished lectures of Hilbert, so we can expect further detailed elaboration and clarification of the twists and turns of the classical period and the run up to it.

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