Gentzen's Anti-Formalist Views

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1 Introduction

In June of 1936 Gentzen gave a lecture at Heinrich Scholz' seminar in Münster. The title of the lecture was "Der Unendlichkeitsbegriff in der Mathematik."[1]

In this lecture, Gentzen presented a generally optimistic view concerning the prospects for the future development of Hilbert's proof-theoretic program to establish the consistency of classical mathematics. At the same time, curiously, he expressed sympathy with a challenge to Hilbert's formalist program that is reminiscent of some of Brouwer's criticisms.

This challenge, which I'll refer to as the *Contentualist Challenge*, was essentially this: even if the consistency of classical mathematics were ultimately to be proved by finitarily acceptable means, this would not be enough to properly found it. Also necessary, in Gentzen's view, was the provision of a way to assign contents to the so-called *ideal propositions*[2] of classical mathematics. Hilbert's so-called direct proof of the consistency of arithmetic was neither designed nor equipped to provide such an assignment. As a result, it was neither designed nor equipped to satisfy conditions the satisfaction of which Gentzen regarded as necessary for the proper foundation of classical mathematics.

[1]The lecture was published in *Semesterberichte Münster*, WS 1936/37: (65–85). It was translated into English by M. E. Szabo as "The Concept of Infinity in Mathematics" and included in [19].

[2]Or what Gentzen generally referred to as *actualist* propositions.

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Gentzen put what he took to be the crucial point this way:

> Even if the consistency were to have been proved, the propositions of actualist mathematics (*die Aussagen der an-sich-Mathematik*)[3] would remain without sense (*sinnlos*) and would therefore, as ever, have to be repudiated (*abzulehnen*). ... The whole question of "sense" (*"Sinnes"*) does not seem ... to be ready for a final settlement. ... The objection against the sense of actualist propositions must in any case not be taken too lightly; it is not entirely without merit. [16, p. 74]

Hilbert's proposed formalist defense of classical mathematics was undertaken for the purpose of justifying the use of ideal elements in our mathematical reasoning. This included, in particular, the use of *actualist* propositions as instruments to aid the conduct of the "logical" parts of mathematical reasoning.

Hilbert did not, however, propose that these so-called ideal propositions be preserved as contentual elements of mathematical thinking. He saw them as useful, perhaps even in some sense "necessary," for the conduct of logical reasoning. He did not, however, take their usefulness to consist in their presumably contentual application in our thinking. Rather, he believed that it is due to their use as ideal elements in our logical thinking—a use which, generally speaking, is similar in both character and motive to the use of such devices as negative and complex numbers in algebra and analysis and points at infinity in projective geometry.

He put the basic point this way:

> [M]athematics contains, first, formulas to which correspond contentual (*inhaltliche*) communications of finitary propositions (mainly numerical equations or inequalities, or more complex communications composed of these) and which we may call the *real propositions* (*realen Aussagen*) of the theory, and, second, formulas that—just like the numerals of contentual number theory—in themselves mean nothing but are merely things governed by our rules and must be regarded as the *ideal material* (*idealen Gebilde*) of our theory. [27, p. 8]

By adjoining the so-called ideal propositions to the real propositions, "we obtain a system of propositions in which all the simple rules of Aristotelian logic hold and all the usual methods of mathematical inference are valid" (*op. cit.*, 9). The development of such a system of "logical" reasoning, Hilbert believed, benefits our logical reasoning in ways that are generally similar to the ways in which the use of ideal elements elsewhere in mathematics benefits other parts of mathematical reasoning. Specifically, it allows us to reason with greater facility to real or contentual conclusions, and it does so without compromising reliability.

Reasoning which makes use of ideal or actualist propositions is not, however, reasoning in the traditional sense. That is, it is not reasoning which proceeds from premises which are judgments having genuine propositional contents to conclusions (ultimate or transitory) which are likewise judgments having propositional contents via inferences that represent judgments of logical relationship between genuine propositions.

[3] "Actualist mathematics" was a term Gentzen commonly used for classical or traditional mathematics.

In the view of the critics of formalism (e.g., Frege and Brouwer), this meant that the reasoning which makes use of the so-called ideal propositions is not, in truth, genuine reasoning at all. Rather, it is only something which has the syntactical facade of genuine reasoning. It lacks the genuine contentful premises and the genuine logical interrelationship of contentual propositions needed for genuine reasoning.

Hilbert and those in his camp (e.g., Bernays) rejected this traditional conception of reasoning. More accurately, they rejected the view that legitimate mathematical reasoning always proceeds according to the traditional contentualist plan. Sometimes, they maintained, it proceeds in decidedly non-contentualist ways for decidedly non-contentualist motives. In their view, this reflected an identifying characteristic of modern scientific thinking generally—namely, that in addition to a descriptive component, it has as well an idealizational component.

> In science we are predominantly if not always concerned with theories that are not completely given to representing reality, but whose significance (*Bedeutung*) consists in the *simplifying idealization* (*vereinfachende Idealisierung*) they offer of reality. This idealization results from the extrapolation by which the concept formations (*Begriffsbildungen*) and basic laws (*Grundsätze*) of the theory go beyond (*überschreitet*) the realm of experiential data (*Erfahrungsdaten*) and intuitive evidence (*anschauliche Evidenz*). [29, pp. 2–3]

As Hilbert and Bernays saw it, the aim of science was not simply or only to describe, but also to idealize and to simplify. Such simplification, however, sometimes called for the use of "formal" rather than contentual methods of reasoning. Accordingly, they believed, contentual interpretation is not necessary for a proper defense of ideal reasoning.

Gentzen suggested a contrary view, giving particular attention to the case of general set theory in this connection. There he speculated in particular that proof-theoretical investigations would confirm that non-denumerable cardinalities are empty appearances (*nur leerer Schein*), that concepts and sentences concerning them are contentless, and that mathematicians ought therefore to avoid making use of them.

> I believe that, for example, in general set theory a careful proof-theoretic investigation will finally show that all powers that go beyond the countable are, in a quite definite sense, only empty appearances and that one should have the good sense to do without them.[4]

In an essay published a year later, he put the point more strongly, describing the question of the content of classical mathematics (or what he called *an-sich* mathematics) as "very important" (*sehr wichtig*) (cf. [18, p. 202]).

[4] The German was:

> Ich glaube, dass z. B. in der allgemeinen Mengenlehre eine sorgfältige beweistheoretische Untersuchung schliesslich die Ansicht bestätigen wird, dass alle über das Abzählbare hinausgehenden Mächtigkeiten in ganz bestimmten Sinne nur leerer Schein sind und man vernünftigerweise auf diese Begriffe wird verzichten müssen. [16, p. 74]

He thus seems to have sympathized with those critics of formalism who, like Frege and Brouwer, emphasized the question of whether formalism can adequately provide for the contentual interpretation of ideal or "actualist" propositions in mathematics.

It is this seeming affinity of Gentzen's views with the traditionalist views of Brouwer and Frege that I find noteworthy. Gentzen, after all, has generally been described, and generally described himself, as an advocate and promoter of Hilbert's ideas in the foundations of mathematics. Hilbert, however, emphasized that the interpretation of ideal reasoning is not necessary either for the conduct of mathematical reasoning or for its proper foundation. This raises the question of how significant the affinities between Gentzen's and Hilbert's views really are. This is the question I want to consider here.

Examination of Gentzen's views reveals distinct sympathy with the traditional conception of reasoning as generally consisting in a finite sequence of judgments arranged according to perceived logical relationships between their contents. He believed the formalization of mathematical reasoning to be a means of preparing it for precise metamathematical investigation, but there is little indication that he saw uninterpreted formal reasoning as playing an important role in mathematical reasoning. In fact, there are counter-indications.

His formalist sympathies thus seem to have been quite limited. He held only a version of what I will call *Representational Formalism*. This is the view that the formal representation of mathematical reasoning is a legitimate and perhaps even a valuable tool for purposes of studying certain of its properties (e.g., its consistency). Whether formal methods have a place in the actual conduct of mathematical reasoning, on the other hand, is another matter, and one which is not settled by the possible usefulness of formalization as a representational tool for metamathematical investigation.

Hilbert too was a Representational Formalist. His formalist convictions went beyond this, however. In addition to believing in the representational utility of formal methods, he believed that they have an important role to play in the actual conduct of mathematical reasoning. He believed, that is, that mathematical reasoning is partially *constituted by* the use of formal, non-contentual methods of reasoning, and he believed as well that the use of such methods has played an important role in making modern mathematics the successful science that it is.

In addition to being a Representational Formalist, then, Hilbert was what I will call a *Conductive Formalist*. Gentzen was not, or at least not so fully as Hilbert. All in all, he seems to have accepted the traditional contentualist view of mathematical reasoning that Hilbert rejected. More specifically, he held that the use of formal methods in mathematical reasoning can only be fully vindicated by providing a contentual interpretation for it.

To the extent that this is correct, Gentzen's formalism was less far-reaching than Hilbert's. This, at any rate, is what I will argue here.

2 The Traditional and Abstract Conceptions of Axiomatization

Gentzen and Hilbert diverged as regards their views of the basic nature of reasoning. Gentzen held a more or less traditional contentualist view of reasoning. Hilbert, on the other hand, rejected the traditional view and emphasized not only the possibility of non-contentual reasoning, but also its importance to mathematics. He did not deny that much mathematical reasoning is contentual. Nor did he deny that contentual reasoning has played an important role, perhaps even a dominant role, in the development of mathematics. He maintained only that there are also non-contentual processes of reasoning, and that these have also been important to the development and success of modern mathematics.

What I am calling the traditional conception of reasoning centered on the idea that an argument is a finite, logically ordered sequence of judgments. The term "judgment" here is used in its traditional sense—that is, to signify an attitude of affirmation taken towards a proposition.

By a "logical ordering" of judgments, I mean an arrangement of the constituent judgments of an argument according to certain perceived relations of broadly logical consequence among them. The traditional conception of proof is a specialization of this view to cases where the constituent judgments making up the proof, or at least certain of them, may have special epistemic qualifications (e.g., being self-evident) and the relations of logical consequence which are taken to relate them are perceived relations of deductive consequence.

The classical source of the traditional view was Aristotle, who presented it as part of a general account of the nature of reasoning in the *Prior Analytics*, Bk. I. What is perhaps the most widely known statement of the view was given in the *Posterior Analytics*, however.

> [D]emonstrative knowledge must proceed from premisses which are true, primary, immediate, better known than, prior to, and causative of the conclusion. On these conditions only will the first principles be properly applicable to the fact which is to be proved. Deduction will be possible without these conditions, but not demonstration; for the result will not be knowledge.
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> *Posterior Analytics*, 71b 20–25

Similar views were expressed throughout the modern era (cf. Locke (cf. [35, Bk IV, ch. xvii, §4]) and Reid (cf. [41, Essay VII, Of Reasoning, p. 475]), and also throughout the eighteenth, nineteenth, and early twentieth centuries (cf. [46, ch.I, pt. 3]; [5, pp. 45–46]; [3, §22]; [34, p. 11]; [20, p. 15] and [33, p. 384] for statements from a variety of different types of works).

Towards the end of the nineteenth century, the traditional conception of proof gave way to a conception of proof coming from the then-emerging "abstract" conception of axiomatization. This new conception of axiomatization differed profoundly from its traditional predecessor.

On the traditional conception of axiomatization, axioms were taken to be true propositions chosen out of consideration of supposed special properties of certainty and/or immediacy and/or explanatory power. Relatedly, proofs were taken to be finite sequences of judgments the propositional contents of each element of which were either to be axioms, or to be seen to follow deductively from the contents of previous elements of the sequence. This is what I will call the *t*raditional *v*iew of *p*roof (TVP).

The abstract conception, by contrast, denied that axioms are certain, self-evident, or explanatorily basic truths. It denied, in fact, that they are truths at all, or even that they are propositions.[5] Axiomatization on the abstract plan sought to separate axioms from contents. Hilbert described the basic process he took to effect this dissociation (in the case of geometry) as follows:

> We think (denken) three different systems of things. The things of the first system we call *points* and designate them A, B, C ...... The things of the second system we call *lines* and designate them a, b, c ...... The things of the third system we call *planes* and designate them α, β, γ ......
> We think (denken) the points, lines and planes in certain mutual relations ...
> The exact (genaue) and for mathematical purposes complete (vollständige) specification of these relationships is accomplished by the *axioms of geometry*. [22, ch. 1, §1][6]

In axiomatization, in Hilbert's view, we "think." We do not observe or intuit and then express the contents of our observations or intuitions in the axioms we give. Rather, we "think," with nothing given prior to or in association with this thinking to serve as its contents.[7]

Nor was this thinking taken to have indigenous contents, at least not in any ordinary sense of the term "contents." It was not a thinking as of definite objects standing in definite relations. Rather, the objects and relations of axiomatic thinking were wholly unspecified, and could be *any* objects and relations that satisfy the abstractly thought axioms.

From the abstract point of view, then, axioms were not taken to be propositions but rather, for some, propositional functions or propositional schemata (cf. [45, p. 2]; [31, §20]), and for others (e.g., Hilbert) sentences or sentence-schemata. For

[5]Describing the abstract viewpoint as applied to projective geometry, Whitehead wrote: "The points mentioned in the axioms are not a special determinate class of entities ... they are in fact any entities whatever, which happen to be inter-related in such a manner, that the axioms are true when considered as referring to those entities and their inter-relations. Accordingly—since the class of points is undetermined—the axioms are not propositions at all ... An axiom (in this sense) since it is not a proposition can neither be true or false." [45, p. 1].

[6]That this represented Hilbert's general conception of axiomatization is indicated by the fact that he gave a precisely parallel characterization of the axiomatic method in arithmetic in an essay published the following year (cf. [23, p. 181]).

[7]The separation of thinking from contents represented in this view is more radical than, but still reminiscent of the separation indicated by Kant in the first critique: "I can think (denken) whatever I want, provided only that I do not contradict myself. This suffices for the possibility of the concept, even though I may not be able to answer for there being, in the sum of all possibilities, an object corresponding to it. Indeed, something more is required before I can ascribe to such a concept objective validity, that is, real possibility; the former possibility is merely logical." [32, xxvi, note a].

present purposes, the difference between these alternatives is insignificant. What is important is that axioms were viewed schematically, or hypothetically—any system of objects and relations satisfying them would also satisfy the theorems that follow from them.[8]

The attributes traditionally taken to characterize axioms (e.g., certainty, self-evidentness, explanatory depth, unprovability (in some objective or quasi-objective sense), etc.) do not of course apply to such schemata. Rather, the thinking regarding choice of axioms for abstract theories seems generally to have been that it should be driven by considerations of mutual consistency and of their usefulness as starting points for the efficient deduction of some further body of theorems.[9]

3 The "Decontentualization" of Proof

The core element of the abstract conception of axiomatization was thus a call for the separation—or, perhaps more accurately, call*s* for various separation*s*—of axiomatic thinking from contents. More specifically for my purposes here, it was a family of calls for various separations of the conduct of proof from contentual considerations.

In this connection, it is perhaps useful to distinguish two such separations. One of these is a separation from contents for purposes of conducting the inferential parts of proofs. For convenience, I'll call this Inferential Separation.

The other concerns a separation from contents for purposes of specifying what the constitutive axioms and rules of inference of a would-be formal proof practice are. I'll refer to this as Specificational Separation.

The mature Hilbert, I believe, supported both types of separation. I will now briefly indicate what I take to be essential to each.

3.1 Inferential Separation

In 1882, Pasch had raised the importance of abstracting away from contents for purposes of ensuring that the inferential parts of proofs were genuinely deductive in character.

[8]Cf. [2, pp. 95–96].

[9]J.W. Young put the point this way: "[W]hat is the new point of view? The self-evident truth is entirely banished. There is no such thing. What has taken the place of it? Simply a set of assumptions concerning the science which is to be developed, in the choice of which we have considerable freedom. . . . [T]hey are elected for their fitness to serve, and their fitness is very largely determined by their simplicity, by the ease with which the other propositions may be derived from them." [47, p. 52].

> [I]f geometry is to be genuinely deductive, the process of inferring (Process des Folgerns) must be everywhere independent of the *sense* (*Sinn*) of geometrical concepts just as it must be independent of figures. It is only *relations* between geometrical concepts that should be taken into account in the propositions and definitions that are dealt with. In the course of a deduction . . . it should *by no means be necessary* to think of the references (Bedeutung) of the geometrical concepts involved. . . . [I]f it is . . . , the gappiness (Lückenhaftigkeit) of the deduction and the inadequacy of the . . . proof is thereby revealed unless it is possible to remove the gaps (Lücke) by modifying the reasoning used. [40, p. 98]

There seem to be both theoretical and practical claims here. On the theoretical side there is a suggestion that an inference in a geometrical proof can properly be known to be deductively valid only if its validity can in principle be known without appealing to the contents of any non-logical term (and, more specifically, any geometrical term) that occurs, whether explicitly or implicitly, in it (i.e., in its premises or its conclusion).[10]

Pasch's practical suggestion, as I see it, ran parallel to this. It suggested as a practical criterion of deductive validity that an inference's validity be practically establishable without appealing to the sense or referent of any non-logical term (specifically, the contents of any geometrical term) occurring (explicitly or implicitly) in it. In other words, it called for the separation of geometrical proof from geometrical contents for purposes of determining the deductive validity of its inferential parts. The suggestion seems to be that persistent failure of conscientious efforts to find such a practical separation of contents from assessments of validity is indication of a failure of rigor in a proof.[11]

Hilbert too endorsed a separation of logical reasoning from contents,[12] though neither the separation he proposed nor his reasons for proposing it were identical to Pasch's.

3.2 Specificational Separation

Pasch's proposed separation of contents from geometrical reasoning seems in significant part to have been a call for rigor. To correctly judge the deductive validity

[10]Pasch did not of course make use of any precise demarcation of logical from non-logical terms. He did, though, have a sense of what the geometrical terms or concepts in a proof were, and he insisted that the validity of a genuinely deductive inference should be knowable without making use of appeals to the senses or referents of any of the geometrical terms that occur in it.

[11]Pasch's call for Inferential Separation of contents from proofs has led some to regard him as the (or at least a) principal founder of the abstract conception of axiomatization (cf. [39, p. 143]; [42, pp. 343–344] and [47, p. 51]). As others (cf. [15, pp. 617–618]) have pointed out, though, correctly in my view, the separation of geometrical reasoning from contents that he proposed is not nearly so radical as that proposed by Hilbert.

[12]"[I]n my theory contentual inference (inhaltliche Schließen) is replaced by manipulation of signs according to rules (äußeres Handeln nach Regeln); in this way the axiomatic method attains that reliability and perfection that it can and must reach if it is to become the basic instrument of all theoretical research." [27, p. 4].

of a geometrical inference did not, in his view, require appeal to the contents of its geometrical terms. To make use of such appeals, therefore, was either to use what one did not recognize was being used, or it was to mistake what is required for deductive validity. Pasch seems to have seen the former—the use of unrecognized information in the inferential parts of proofs—as the more insidious threat and the one protection against which thus required more careful and deliberate efforts.

The use of such information in the conduct of inference constituted a failure of rigor. Pasch's call for abstraction from the meanings of geometrical terms for purposes of conducting the inferential parts of geometrical proofs was intended to provide protection against such failure.

It is not only in the inferential parts of proofs, however, that use of unrecognized information may enter. It may also enter in the identification or specification of axioms and/or rules of inference. It may be avoidance of this type of illicit use of unrecognized information that Hilbert had in mind when he declared that the specification of axioms of an axiomatic system should provide an "exact (genaue) and for mathematical purposes complete (vollständige) specification" [22, ch. 1, §1] of the objects-as-standing-in-relations that constituted what was thought in a given axiomatic "thinking" (denken). Here, I'll focus on the part of the claim concerning exactness and leave the part concerning completeness for another occasion.

What would constitute a specification of axioms that is "exact" in this sense? There is nothing I know of in Hilbert's early writings that clarifies what he had in mind. In the fuller development of his proof theory, however, he came to the view that axioms should be syntactically rather than semantically specified. More accurately, he came to the view that proper specification of axioms consisted in their being *exhibited* (i.e., in their being given in terms of their outward appearances) rather than in their being *expressed* (i.e., in their being given in terms of semantical contents). To put it differently, Hilbert's eventual view seems to have been that only such things as can be identified by their outward appearances, without application of semantic interpretation, are exactly specifiable. Accordingly, only formulae, not propositions, can ultimately satisfy the requirements of exact specification of an axiomatic thinking (denken).

If this is how Hilbert eventually came to understand the requirement that axioms be "exactly" specified, then it represents another point at (or another way in) which at least his mature understanding of axiomatic thinking saw it as involving various types of "decontentualization."

4 Decontentualization and Its Discontents

Weyl described the decontentualized conception of proof of Hilbert's proof theory as representing a radical departure from the views of his predecessors.

> Before Hilbert constructed his proof theory everyone thought of mathematics as a system of contentual (*inhaltliche*), meaningful (*sinnerfüllte*), and evident (*einsichtige*) truths; this point of view was the common platform of all discussions. . . . Brouwer, like everyone else,

> required of mathematics that its theorems be (in Hilbert's terminology) "real propositions," meaningful truths. [43, p. 22][13]

This may largely have been true, but, as the above remarks concerning the development of the abstract conception of axiomatization indicate, it's not entirely accurate. Pasch's view of proof, with its distinctive understanding of the requirements of inferential rigor, is not adequately captured by it.[14] Neither does it accurately convey the place that abstract views of axiomatization occupied in late nineteenth and early twentieth century understandings of axiomatic method.[15]

Be this as it may, contentualist understandings of mathematical proof were certainly common and influential during the period in which Hilbert developed his proof-theoretic ideas. Since Gentzen's understanding of the nature of proof seems to have been influenced by such views, it seems sensible to briefly survey some of the more influential contentualist views of proof of Gentzen's time.

Among these, Brouwer's are perhaps particularly important because of Gentzen's expressed sympathies with intuitionist views of proof. Brouwer stressed his opposition to non-contentual conceptions of proof in his criticisms of Hilbert's program—particularly his criticisms of Hilbert's idea that to properly found traditional mathematics is essentially to prove its consistency.[16]

In Brouwer's view, to properly found traditional mathematics (or some part of it), it was necessary to establish it not merely as consistent but as truthful or correct. What is true, however, is contentful since it is contentual items only that are capable of being true or false. Proving the syntactical consistency of a theory or inferential

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[13]See [44, p. 640] for a similar statement. See also [7, p. 336]; [8, pp. 490–492] and [10, pp. 2–5] for related ideas and arguments.

[14]Neither are the contributions of others with ideas similar to Pasch's. These contributions were noted by various early twentieth century writers. The following statement by Young is characteristic: "The abstract formulation of mathematics seems to date back to the German mathematician Moritz Pasch. At any rate, he was the first to study in detail the axioms concerning the order of points on a straight line ... But to the Italian Giuseppe Peano belongs the credit of developing this point of view systematically. His idea, which he began to elaborate about 1889, is to put the whole of mathematics on a purely formal basis ... " [47, p. 51].

[15]Here too Young gave a more accurate description: "The point of view of 50 years ago was very largely that the foundations of mathematics were axioms; and by axioms were meant self-evident truths, that is, ideas imposed upon our minds a priori, with which we must necessarily begin any rational development of the subject. So the axioms dominated our mathematical science, as it were, by the divine right of the alleged inconceivability of the opposite. And now, what is the new point of view? The self-evident truth is entirely banished. There is no such thing. What has taken the place of it? Simply a set of assumptions concerning the science which is to be developed, in the choice of which we have considerable freedom." (*op. cit.*, 52).

[16]Strictly speaking, Hilbert required more than a proof of consistency for the proper foundation of classical mathematics. He required as well that its uses of ideal methods be "successful": "[I]f the question of the justification (Berechtigung) of a procedure (Maßnahme) means anything more than proving its consistency, it can only mean determining whether the procedure fulfills its promised purpose. Indeed, success is necessary; here, too, it is the highest tribunal, to which everyone submits." [26, p. 163].

practice could not, therefore, in Brouwer's view, properly found it since it would not establish it as correct (*richtig*).

> [T]he *formalistic critique* ... in essence comes to this: the *language accompanying the mathematical mental activity* is subjected to a mathematical examination. To such an examination the laws of theoretical logic present themselves as operators acting on primitive formulas or axioms, and one sets himself the goal of transforming these axioms in such a way that the linguistic effect of the operators mentioned (which are themselves retained unchanged) can no longer be disturbed by the appearance of the linguistic figure of a contradiction. We need by no means despair of reaching this goal,[17] but nothing of mathematical value will thus be gained: an incorrect theory (*unrichtige Theorie*), even if it cannot be inhibited by any contradiction that would refute it, is none the less incorrect, just as a criminal policy is none the less criminal even if it cannot be inhibited by any court that would curb it. [7, p. 336][18]

As Brouwer saw it, then, the fundamental mistake of the formalist was the failure to appreciate the differences between operations of genuine reasoning and operations on linguistic items. The latter might resemble the former in certain ways but, in the end, these could be only superficial similarities. To fail to recognize this was to fail to see the critical difference between genuine thinking and a mere use of language—a difference featured in what was perhaps the basic element of Brouwer's foundational outlook, the so-called *First Act of Intuitionism*.

> [T]he FIRST ACT OF INTUITIONISM completely separates mathematics from mathematical language, in particular from the phenomena of language which are described by theoretical logic, and recognizes that intuitionist mathematics is an essentially languageless activity of the mind ... [9, pp. 140–141]

Formalism flouted the First Act of Intutionism. More specifically, in the intuitionists' view, it systematically overestimated the importance of language as a vehicle for the conduct of reasoning. Similarly, as they saw it, it overestimated even the importance of mathematical language as a means of representing and studying the properties of mathematical reasoning.

The intuitionists were not the only ones to object to the decontentualizing tendencies of Hilbert's abstractionist outlook. Klein, for example, described it as representing "the death of all science" [33, p. 384].

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[17] At this point Brouwer inserted the following remark in a note: "[T]he unjustified application of the principle of excluded middle to properties of well-constructed mathematical systems can never lead to a contradiction ... "

[18] The passage in the German original is on pp. 2–3. It is perhaps worth noting that on Hilbert's view, consistency meant consistency with real mathematics. Therefore, if incorrectness is defined as proving something that is refutable by real means, then proving consistency in Hilbert's sense *would* eliminate the possibility of incorrectness on one natural understanding of that term. Perhaps on Brouwer's understanding of "unrichtige," "richtige" was intended to imply conservativeness—so that a theory would be incorrect if it proved propositions that are not themselves provable by real means, and not only if it proved propositions that are refutable by real means. Under certain conditions, of course, the two understandings extensionally coincide.

Frege too decried it and he made the contentual nature of genuine proof the focal point of his disagreements with Hilbert and those others (e.g., Heine and Thomae) he saw as advocating non-contentualist views of proof.

> [A]n inference does not consist of signs. We can only say that in the transition from one group of signs to a new group of signs, it may look now and then as though we are presented with an inference. An inference simply does not belong to the realm of signs; rather, it is the pronouncement of a judgment made in accordance with logical laws on the basis of previously passed judgments. Each of the premises is a determinate thought recognized as true; and in the conclusion, too, a determinate thought is recognized as true .... [12, p. 387]

In Frege's view too, then, a proof was a thoroughly contentual affair—specifically, it was a sequence of judgments the propositional contents of which must be judged by the prover to stand in certain logical relationships to one another. Without such logical interrelationship there can be no genuine proof, and unless the premises and conclusions of proofs have propositional contents, there can be no genuine logical relationship between them.

5 Hilbert's Conductive Formalism

As mentioned, Hilbert rejected this traditional contentualist conception of proof (and, more generally, the traditional contentualist view of reasoning). This should not, however, lead us to think that he denied the importance, or even the centrality, of contentual proof to the development of mathematical knowledge. He did not. In fact, he emphasized the importance of contentual reasoning to mathematics and, particularly, its indispensability to metamathematics.

Where he thought the opponents of non-contentual reasoning had gone too far was in their view that mathematical reasoning and proof has and indeed must always be contentual, or that non-contentual reasoning has played only an insignificant role in the historical development of our mathematical knowledge. In Hilbert's view, mathematical proof has often assumed non-contentual forms, and he believed the use of such forms to have been and to continue to be invaluable in our attempts to mitigate various types of complexity and/or inefficiency that commonly limit the usefulness or even the practical applicability of contentual methods of proof.

Hilbert gave various examples intended to illustrate the usefulness of non-contentual methods of reasoning in mathematics. These included the introduction of the imaginary and complex numbers to "simplify the theorems on the existence and number of roots of an equation" [26, p. 166] and the introduction of elements at infinity in projective geometry which "make the system of laws of connection as simple and perspicuous as is possible" (*loc. cit.*) and which induce the symmetries behind the dualities of projective geometry "which are so fruitful (*fruchtbare*)" (*loc. cit.*).

What he regarded as the crowning example, though, is the use of the classical laws of logic to manage what he believed are crippling complexities of non-classical (specifically, finitary) contentual logical reasoning. Classical methods of logical reasoning may not be contentual, but this ought not blind us to the fact that they may be useful, even, in some sense, indispensable to the practical conduct of (at least parts of) our logical reasoning.

Hilbert thus urged addition of the so-called ideal propositions [26, p. 174] to real contentual propositions "in order to maintain the formally simple rules of ordinary Aristotelian logic" (*ibid.*).[19] To make such an addition was, in his view, a natural and motivated application of the method of ideal methods in mathematics, a method which had proved its efficacy and trustworthiness again and again in the history of mathematics.

Hilbert seems also to have seen the application of ideal methods as pervasive both in our scientific and in our everyday reasoning.

> In our theoretical sciences we are accustomed to the use of formal thought processes (*formaler Denkprozesse*) and abstract methods ... [But] already in everyday life (*täglichen Leben*) one uses methods and concept-constructions (*Begriffsbildungen*) which require a high degree of abstraction and which only become plain through unconscious application of the axiomatic method (*nur durch unbewußte Anwendung der axiomatischen Methoden verständlich sind*). Examples include the general process of negation and, especially, the concept of infinity. [28, p. 380]

To try to do without ideal methods in our thinking would thus, in Hilbert's view, seriously impair our effectiveness as thinkers. Opposition to their use was, in Hilbert's view, largely a result of a failure to recognize that language has valuable and legitimate non-descriptive uses. Bernays memorably urged this point in offering his Faustian summary of Hilbert's formalist viewpoint.

> Where concepts fail, a sign appears at just the right time.[20] This is the methodological principle of Hilbert's theory. [1, p. 16]

Hilbert put the point more forcefully, if perhaps less picturesquely. In his view, the use of non-contentual (or, more specifically, symbolico-algebraic) methods in the conduct of our reasoning is indispensable to the fullest practical development of our mathematical knowledge (cf. [24, pp. 162–163]; [26, p. 162]; [27, pp. 7–8]). He saw it as reflecting the importance attached to the use of non-descriptive simplifying idealizations he took to be characteristic of modern science (cf. [29, pp. 2–3]). He believed that we may legitimately take advantage of the benefits of such simplification without sacrificing security in the contentual parts of our thinking.

[19]This addition was to be controlled by consistency, of course. On this point, Hilbert thought he could satisfy even Brouwer and Kronecker. What they did not accept, however, is that controlling for consistency should be enough to establish a putative body of reasoning as genuine reasoning, much less as reliable genuine reasoning.

[20]"Thus even where concepts fail, a word appears at just the right time."

Goethe, *Faust* I (Mephistopheles to a student of theology)

The German is: "Denn eben wo Begriffe fehlen, Da stellt ein Wort zur rechten Zeit sich ein." Goethe was not endorsing but criticizing such a practice of course. He presented it as a practice employed by teachers of theology to preserve a facade of contentful thinking where in fact there were only contentually empty words.

Hilbert thus embraced what I am calling *Conductive Formalism*, the view that the use of non-contentual methods of reasoning has been and continues to be important to the effective development of our mathematical knowledge. He accepted as well of course what I call *Representational Formalism*—that is, the view that the formal representation of mathematical reasoning is a tool for facilitating the rigorous and mathematically precise investigation of mathematical reasoning.

As I read him, Gentzen only fully endorsed Representational Formalism. He seems not to have taken the use of non-contentual methods in mathematics to qualify as genuine reasoning. In addition, he seems to have taken the provision of a contentual interpretation for what Hilbert termed “ideal reasoning” (and what he, Gentzen, termed actualist or *an-sich* reasoning) as important to its proper foundation.

In these important respects, then, Gentzen’s views more nearly resembled the anti-formalist views of Brouwer and Frege than the formalist views of Hilbert.

6 Gentzen’s Conductive Contentualism

Gentzen seems in fact to have gone out of his way both to comply with traditional contentualist strictures on reasoning and to make clear his endorsement of them. §9 and §17.3 of [17] provide clear confirmation of this. They are dedicated to establishing compliance with contentualist demands as regards reasoning to actualist conclusions (more accurately, reasoning to contentual conclusions expressed by actualist sentences) in number theory.

Gentzen also took pains to show that his consistency proof for number theory meets all reasonable demands of this type. He was particularly concerned to show that his consistency proof provides for the *finitary interpretation* of the actualist sentences of number theory, and he seems to have seen this as a necessary part of justifying the use of actualist methods in arithmetic.

> The most essential component (*wesentlichste Teil*) of my consistency proof ... consists precisely in its attachment of a finitary sense to actualist propositions (*daß den an-sich Aussagen ein finiter Sinn beigelegt wird*), viz. for any given proposition, if it is proven, a reduction rule (*Reduziervorschrift*) ... can be specified, and this fact represents the finitary sense of the proposition that is obtained precisely through the consistency proof. [17, p. 564]

My point and my claim is not that Gentzen was right to have described his proof as providing finitary senses for actualist sentences. It is rather that *he* seems to have seen it as doing so, and he seems to have seen its doing so as being in some way its most essential feature.

Gentzen thus seems to have affirmed the traditional view that to be fully justified, actualist methods must be contentually interpreted.

> [E]ven if it should be demonstrated that the disputed forms of inference cannot lead to mutually contradictory results, these results would nonetheless be propositions *without sense* (*sinnlose Aussagen*) and their investigation therefore a mere recreation (*eine Spielerei*);

> genuine *knowledge* (*wirkliche Erkenntnisse*) can be gained only by means of the unobjectionable (*unbedenklichen*) intuitionist (or finitist, as the case may be) forms of inference [17, p. 564] (emphases as in text)[21]

Gentzen then went on to consider what value (*Erkenntniswert*) (*loc. cit.*) there might be in uninterpreted actualist reasoning (i.e., in actualist reasoning which, though lacking interpretation, nonetheless qualifies as actualist reasoning). He allowed as how it might have some practical value (*praktischer Wert*) (*ibid.*) and not be entirely useless (*nicht ganz zwecklos*) (*ibid.*) as an instrument of thinking. This was not, however, for him an adequate substitute for its providing a genuine contentual justification for its conclusion.

This too is similar to the things intuitionists said about the value of actualist reasoning. We already noted one such point by Brouwer in his concession that actualist reasoning might be "an efficient ... technique for memorizing mathematical constructions, and for suggesting them to others" [9, p. 140].[22] He even allowed as how it might be contentually reliable over a certain range of cases.

> *Suppose that an intuitionist mathematical construction has been carefully described by means of words, and then, the introspective character of the mathematical construction being ignored for a moment, its linguistic description is considered by itself and submitted to a linguistic application of a principle of classical logic. Is it then always possible to perform a languageless mathematical construction finding its expression in the logico-linguistic figure in question?*
> After careful examination one answers this question in the affirmative (if one allows for the inevitable inadequacy of language as a mode of description) as far as the principles of contradiction and syllogism are concerned; but in the negative (except in special cases) with regard to the principle of excluded third ... [9, p. 140]

What neither Brouwer nor Gentzen was willing to grant, though, and what in the end seems to have constituted their deepest difference with Hilbert, is that actualist reasoning might be an acceptable replacement for contentual reasoning were its syntactical consistency with finitary contentual reasoning to be finitarily proven.

Gentzen's contentualist convictions seem to have stemmed from a view that non-contentual "reasoning" is not genuine reasoning at all, that it is fundamentally a type of game and that it cannot therefore properly be a part of a genuine *science* of mathematics (cf. [17, p. 564]).

This was in fact the common attitude of the late nineteenth and early twentieth centuries. The thinking was that what essentially separates science from a game is applicability. A genuine science *is* (at least potentially) applicable. A game is not. What makes genuine sciences applicable and games not is that the former, in

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[21]Compare this to the remark by Brouwer, quoted earlier, that "even if [actualist reasoning] cannot be inhibited by any contradiction that would refute it, it is none the less incorrect, just as a criminal policy is none the less criminal even if it cannot be inhibited by any court that would curb it," [7, p. 336] brackets added.

[22]Gentzen could not have read this text of course. There are, though, earlier texts which express similar ideas. Cf. [6] and [7] for related, though less firm endorsements of the utility of classical reasoning as a certain type of instrument to guide our thinking.

contrast to the latter, express thoughts or contents and, in doing so, they *describe* the world and so become applicable to it.

Frege expressed these ideas clearly in the second volume of the *Grundgesetze*.

> Why can one make no application (*keine Anwendung machen*) of a position (*Stellung*) of chess figures? Clearly because it expresses no thought (*es keinen Gedanken ausdrückt*). . . . Why can one make applications of arithmetical equalities? Only because they express thoughts (*nur weil sie Gedanken ausdrücken*). How could we possibly apply an equation which expressed nothing, was nothing more than a group of figures (*Figurengruppe*) to be transformed (*umgewandelt*) into another group of figures by certain rules! It is applicability alone (*Anwendbarkeit allein*) that raises (*erhebt*) arithmetic from a game to the rank of science. Is it a good thing (*wohlgetan*), then, to exclude from arithmetic that which is necessary for it to be a science? [11, §91]

Hilbert saw little to justify such thinking. He accepted the idea that mathematics ought to be applicable. He did not however accept the traditional *descriptive* paradigm of application—that application essentially consists in or at least requires description (i.e., expression of a true thought or content). He adhered instead to the Berkeleyan idea that, though the application of reasoning to reality may require that its conclusion be interpretable (i.e., that it admit of interpretation by a true thought or content), the same is not true of the various steps of reasoning that lead to that conclusion.

Actualist sentences were in Hilbert's view instruments of thought and their use was essentially axiomatic in character—that is, it was completely governed by explicit (i.e., syntactically stated) rules of usage. By this he seems to have meant that actualist sentences do not function contentually, and that, accordingly, their justified use does not require semantical interpretation, be it constructive or actualist in character.

As Hilbert's saw it, Brouwer operated with substantially the same scheme of distinctions and made essentially the same mistakes that Frege did. He assumed not only that application requires interpretation but also that the rules according to which the formal operations of ideal reasoning proceed are in some sense *convened* or *chosen*.

This, to Hilbert, was a distorting oversimplification. As he saw it, the rules according to which time-tested ideal reasoning proceeds are laws according to which our reasoning most effectively proceeds. We do not *merely* choose or convene them. Rather, we experiment with various instruments of reasoning in order to test their effectiveness, and we subject them to metamathematical investigation to determine their consistency with the results of real reasoning. Those which survive such testing represent the accumulated experience and prudence of the larger community of mathematical reasoners. The discovery and metamathematical vindication of such laws, in Hilbert's view, deserved to be made the chief focus of foundational investigation.

> The formula game (*Formelspiel*) that Brouwer so dismissively judges (*wegwerfend urteilt*) has, besides its mathematical value, an important general philosophical significance. For this formula game 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. Thinking (*Das Denken*), it so happens, parallels speaking and writing: we form statements and place them one behind another. If any totality of observations and phenomena deserves to be made the object of a serious and thorough investigation (*ernsten und grundlichen Forschung*), it is this one ... [27, pp. 15–16]

The discovery and metamathematical vindication of formal methods of reasoning was thus, in Hilbert's view, far from being a game. It was rather, in a profound sense, the investigation of the laws of human thinking and, as such, deserved to be made a chief focus of foundational research in mathematics.

7 Conclusion

Hilbert and Gentzen were not formalists of the same type. Specifically, Hilbert advocated a version of Conductive Formalism while Gentzen did not. More specifically, Gentzen held a fairly traditional contentualist view of the nature of proof while Hilbert rejected such a view, and, indeed emphasized the importance of the use of non-contentual methods in mathematics to the overall development of mathematical knowledge.

Hilbert was in fact emphatic on this point. His conviction reflected his observation of the fruitful uses that had been made of non-contentual methods of reasoning throughout the history of mathematics. It also reflected his general view of the place of idealization in mathematics and in modern science generally.

According to this view, scientific mathematics not only does not require interpretation, it does not generally invite it. The reason is the characteristic use it makes of simplifying idealizations.

The reasoning that stems from such idealizations is not intended to be interpreted and, generally speaking, it is neither necessary nor desirable that it should be. All that is required is that it be shown not to conflict with the results of the real or contentual (i.e., the non-idealizational) parts of the given science.

Such separation of mathematical reasoning from contentual interpretation was a central element of Hilbert's formalism.

Gentzen, by contrast, was committed both to a contentualist understanding of proof and to a view to the effect that to properly found a body of mathematical reasoning requires providing an interpretation for it. In fact, as noted above, he described as a key virtue of his consistency proof for classical first-order arithmetic that it provides finitary senses for actualist propositions (cf. [17, p. 564]).[23]

[23]Gentzen in fact raised the possibility of the need for a second type of interpretation for classical arithmetic—one which provides *actualist* interpretations of actualist sentences. Cf. [17, p. 565].

If this is right, then Gentzen cannot plausibly be described as having been a formalist of the sort Hilbert was. He was not, in particular, a conductive formalist. He did not emphasize, as Hilbert did, the importance of non-contentual methods as means of conducting mathematical reasoning. He seems not in fact to have seen the use of non-contentual methods as constituting genuine reasoning at all. Still less did he see it as the glory of modern mathematics.

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