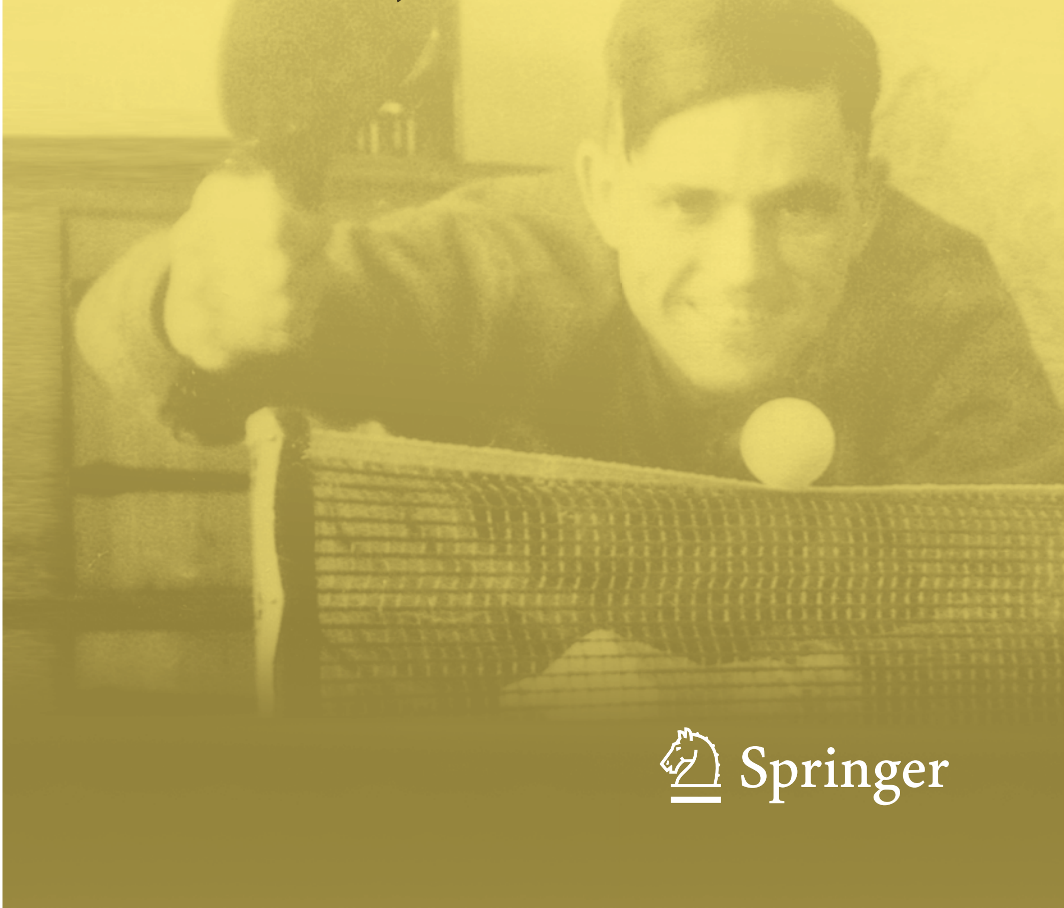
Reinhard Kahle
Michael Rathjen *Editors*

Gentzen's Centenary

The Quest for Consistency



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The Quest for Consistency

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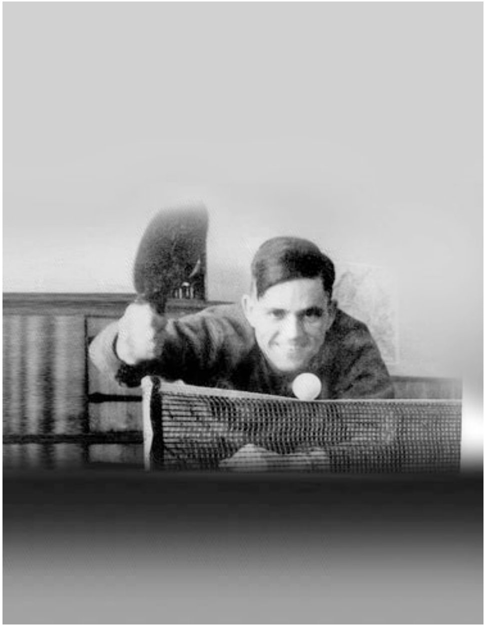
Preface



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This volume is a tribute by several generations of proof theorists to Gerhard Gentzen, one of the greatest logicians ever to whom we owe the most profound investigation of the nature of proofs since Aristotle and Frege. The immediate stimulus for its inception was Gentzen's 100th birthday in 2009 which was celebrated with a conference in Leeds and a workshop in Coimbra at which most of the contributors to this volume spoke.

Gentzen has been described as logic's lost genius[1] whom Gödel sometimes called a better logician than himself.[2] It could be said that Gentzen and Gödel arrived, each in their own exquisite manner, at opposing extremes of a spectrum. Gödel found a very general negative result to the effect that no system embodying a correct

[1]E. Menzler-Trott: *Logic's Lost Genius: The Life of Gerhard Gentzen* (AMS, Providence, 2007).

[2]G. Kreisel: Gödel's excursions into intuitionistic logic, in: *Gödel remembered*, (Bibliopolis, Napoli, 1987) p. 169.

amount of number theory can prove its own consistency by transferring the trick of the "Liar's Paradox" from the context of truth to that of provability. Gentzen, on the other hand, established the positive result that elementary number theory is consistent, using at some crucial point the well-orderedness of a certain ordering called ε_0 that sprang from Cantor's normal form (for presenting ordinals). He also gave a direct proof that the latter principle is not deducible in this theory, thereby providing an entirely new proof of a mathematical incompleteness in number theory.

Gentzen can be rightly considered to be the founding father of modern proof theory. His sequent calculus and natural deduction system beautifully explain the deep symmetries of logic. They underlie modern developments in computer science such as automated theorem proving and type theory. This volume's chapters by leading proof-theorists attest to Gentzen's enduring legacy in mathematical logic and beyond. Their contributions range from philosophical reflections and re-evaluations of Gentzen's original consistency proofs and results in proof theory to some of the most recent developments in this exciting area of modern mathematical logic.

Acknowledgements The plan for the present book evolved at the *Leeds Symposium on Proof Theory and Constructivism*[3] in 2009 which lasted from 4th July to 15th July. The symposium consisted of three connected events one of which was the *Gentzen Centenary Conference*. We would like to thank the Kurt Gödel Society and the Deutsche Vereinigung für Mathematische Logik und für Grundlagenforschung der Exakten Wissenschaften (DVMLG) for providing funding for the Gentzen Centenary Conference. This conference was followed by a Workshop associated with the Annual Meeting of the European Association of Computer Science Logic, *CSL 2009*, with the title *Gentzen Centenary—The Quest for Consistency*[4] which took place on 12th September in Coimbra, Portugal. This workshop also received support from the Kurt Gödel Society for which we are very grateful, as well as for the support of the Portuguese Science Foundation FCT, which funded the Workshop and the edition of this volume through the projects, *Dialogical Foundation of Semantics* (LOGICCC/0001/2007), within the ESF programme LogICCC, *Hilbert's Legacy in the Philosophy of Mathematics* (PTDC/FIL-FCI/109991/2009), and *The Notion of Mathematical Proof* (PTDC/MHC-FIL/5363/2012).

The editors are especially indebted to Matthias Baaz for his enthusiastic support of both events, for stimulating the book project and for contributing towards giving the present volume its shape by providing information, encouragement, culinary highlights and counsel. Finally, let us thank the referees of the papers collected in this volume for their valuable help.

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[3] http://www.personal.leeds.ac.uk/~matptw/.

[4] http://www.mat.uc.pt/~kahle/gentzen/.

In Memoriam: Grigori Mints, 1939–2014

When this book was about to be sent to the publisher, we received the very sad news that Grigori ("Grisha") Mints had died on 30th May 2014. He was born on 7th June 1939 in Leningrad (now again St. Petersburg).

Grisha was a driving force in proof theory and constructivism and a loyal promoter of Gentzen-style proof theory. He was the pre-eminent expert on Hilbert's epsilon calculus and the leading exponent of the substitution method approach to proof theory, expanding its range of applications to strong subsystems of arithmetic. His discovery of the method of continuous cut elimination for infinitary proofs unearthed the deeper relationship between Gentzen's reduction steps on finitary derivations and infinitary proof theory. In pursuit of his wide ranging research interests, he published three books, ten edited volumes, more than 200 scholarly papers, and thousands of reviews, with the aid of which he also maintained and fostered his world spanning network of intellectual contacts through sometimes difficult years working in the Soviet Union. Vladimir Lifschitz wrote about Grisha[1]:

> ... his true calling was to study formal proofs in the spirit of pure mathematics in the best sense of the word: the main project of Grisha's professional life was to develop a clear, complete understanding of properties of proofs, so that any possible question about them will be easy to answer.

[1] https://philosophy.stanford.edu/news/professor-grigori-grisha-mints.

In this way he can be seen as one of the leading executors of Gentzen's legacy and it seems to be more than adequate to dedicate this volume, celebrating Gerhard Gentzen's centenary, to the memory of Grisha Mints.

Contents

Part III Results

Part IV Developments

Part I
Reflections

Gentzen's Consistency Proof in Context

Reinhard Kahle

1 Introduction

Gentzen's celebrated consistency proof—or proofs, to distinguish the different variations he gave[1]—of Peano Arithmetic in terms of transfinite induction up to the ordinal[2] ε_0 can be considered as the birth of modern proof theory. After the blow which Gödel's incompleteness theorems gave the original Hilbert Programme, Gentzen's result did not just provide *a* consistency proof of formalized Arithmetic, it also opened a new way to deal "positively" with incompleteness phenomena.[3] In addition, Gentzen invented, on the way to his result, *structural proof theory*, understood as the branch of proof theory studying structural (in contrast to mathematical) properties of formal systems [79, 111]. With the introduction of sequent calculus and natural deduction and the corresponding theorems about cut elimination and normalization, respectively,[4] he revolutionized the concept of derivation calculus, fundamental for all further developments of proof theory.

Here, we focus on the aspects of his work related to the quest for consistency proofs of theories with mathematical content. We like to recall the context in which the consistency proofs—one may add: "after Gödel"—have to be put, and what might be their mathematical and/or philosophical rationale. For it, we will look

[1]Cf., e.g., [13, 87, 105], and [114] as well as [97] in this volume.

[2]For the ordinal ε_0 see, for instance, [58] in this volume.

[3]See, for instance, [90] in this volume.

[4]See, for instance, [15] and [87] in this volume.

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back to Hilbert's (original) programme and the immediate lessons one may learn from Gödel's theorems. We then consider consistency proofs for Arithmetic, whose consistency, however, is not really at issue. After discussing the interesting case of Analysis, we finish with a reflection on modern proof theory as it is guided by the quest for consistency in the investigation of stronger and stronger mathematical theories.

2 Hilbert's Programme

Hilbert's programme originates from his own second problem in the famous Paris problem list [45] and, in its mature form, it proposes to carry out consistency proofs of axioms systems for Arithmetic and Analysis "by finitistic methods." Hilbert didn't specify exactly what he meant by "finitistic methods" and in modern formal presentations one identifies these methods—following Tait [104]—with *primitive-recursive Arithmetic*, PRA. From an abstract point of view, the main issue is that the consistency of the base theory, in which the consistency proof should be carried out, is beyond any reasonable doubt; and this should be the case for the finitistic methods, whatever they are concretely.

The idea of Hilbert's programme was somehow already conceived with the question given in 1900, and a first sketch of how a consistency proof could be performed was given by Hilbert in 1904 in his lecture at the International Congress of Mathematicians in Heidelberg [47]. It was, however, only the appearance of Brouwer's *Intuitionism* which forced Hilbert to formulate his programme in precise formal terms.[5] Finitistic Mathematics should play, in this context, the role of the part of Mathematics which is beyond any doubt concerning consistency. It was then the aim to justify the other parts of Mathematics by formal consistency proofs carried out using only finitistic means. It is worth noting that, with the choice of finitistic Mathematics as the base, Hilbert was fully in line with the intuitionistic movement—even on philosophical grounds, and it should not come as a surprise that he himself was occasionally called an intuitionist.[6] One can even find a

[5] For the development of Hilbert's programme(s), cf. e.g., [98].

[6] See Fraenkel [28, p. 154]:

> This is the point of view of HILBERT, who, therefore, picks up himself the methodical starting point of his intuitionist opponents—but for the purpose to deny their thesis; one could almost characterize him as an intuitionist.

(German original: "Dies etwa ist der Standpunkt HILBERTS, der somit den methodischen Ausgangspunkt seiner intuitionistischen Gegner — allerdings zum Zweck der Bestreitung ihrer Thesen — selbst aufnimmt; man könnte ihn geradezu als Intuitionisten bezeichnen.") Van Dalen adds to this citation [112, p. 309]: "Although the inner circle of experts in the area (e.g. Bernays, Weyl, von Neumann, Brouwer) had reached the same conclusion some time before, it was Fraenkel who put it on record." See also footnote 18.

"intuitionistic creed" given by Gentzen in 1938, when he wrote [102, p. 235][7]:

> The most consequential form of delimitation is that represented by the '*intuitionistic*' point of view, . . .

What separated Hilbert from Brouwer and Weyl was the latter's attitude to ban "the other mathematics" from the mathematical discourse. In contrast, he was proposing to justify by his *Beweistheorie* Mathematics in all its extensions on the base of finitistic Mathematics. Here, Hilbert's programme gained a new aspect: besides consistency, one could now also demand *conservativity* of "higher" Mathematics over finitary Mathematics.[8]

Without any doubt, Gödel's second incompleteness theorem put an end to Hilbert's programme in its original formulation.[9] The so-called failure of Hilbert's Programme is advocated at several places, maybe most notable by Kreisel [66, Abstract and p. 352]. But which kind of "failure" was it? Surely, it was the not the one which was feared by the critics of classical mathematics. When Hermann Weyl drew on the picture of a "house built on sand" [118, p. 1] he was afraid of possible inconsistencies which could bring classical mathematics to collapse. Of course, Gödel's theorems suggest on no account that there would be an inconsistency in classical mathematics (or even Arithmetic).[10]

As far as consistency is concerned, one may compare the situation with the classical construction problems in Euclidean Geometry. There is no way to trisect an angle by compass and ruler—but there are other means to do so (for instance, using a marked ruler). Of course, in the context of a consistency proof, using other means than finitistic ones will undermine Hilbert's original philosophical starting point. But Hilbert was, by no means, a philosophical hardliner. The only piece of written evidence which we have about Hilbert's reception of Gödel's result is the cryptic short preface in the first volume of the *Grundlagen der Mathematik* [52], saying that Gödel's result "shows only that—for more advanced consistency proofs—the finitistic standpoint has to be exploited in a manner that is sharper [. . .],"[11] i.e., the philosophical starting point was to change. Bernays and Ackermann provide us with two additional testimonies that Hilbert soon adapted his "meta-mathematical standpoint."

[7]German original [32, p. 6]: "Die folgerichtigste Art der Abgrenzung ist die durch den '*intuitionistischen'* Standpunkt [. . .] gegebene."

[8]We may leave it open here whether Hilbert himself was advocating such a conservativity. The issue of conservativity can be considered, of course, without reference to historic figures.

[9]It is reported in the Schütte school that this was also immediately recognized in Göttingen.

[10]But one may note the puzzling lack of understanding of Russell, expressed in a letter to Leon Henkin of 1 April 1963, cf. [18, p. 89ff].

[11]Hilbert and Bernays [55, p. VII]. German original: "Jenes Ergebnis zeigt in der Tat auch nur, daß man für die weitergehenden Widerspruchsfreiheitsbeweise den finiten Standpunkt in einer schäferen Weise ausnutzen muß, [. . .]."

Based on Bernays's reports, Reid writes about Hilbert's reaction to Gödel's result [92, p. 198]: "At first he was only angry and frustrated, but then he began to try to deal constructively with the problem. Bernays found himself impressed that even now, at the very end of his career, Hilbert was able to make great changes in his program."

Ackermann writes in a letter to Hilbert (August 23rd, 1933)[12]: "I was particularly interested in the new meta-mathematical standpoint which you now adopt and which was provoked by Gödel's work."

Unfortunately, we have no sources which explicate in detail Hilbert's new standpoint, but it goes without saying that Gentzen's work was in line with it.[13] In fact, Bernays starts the section heading of the presentation of Gentzen's proof of the consistency of Arithmetic in [53, Sect. 5.3] with "Transgression of the previous methodological standpoint of proof theory."[14]

Thus, with a more "liberal" philosophical position consistency proofs can still be carried out, addressing Hilbert's initial concerns. And Gentzen's consistency proof was among the first ones which provided such an argument. It was not even the only one, and Gödel gave, as early as 1938, in a talk at *Zilsel's seminar* in Vienna, an interesting overview of possible alternatives to extend Hilbert's original standpoint [38, p. 95][15]:

4. *How then shall we extend?* (Extension is necessary.) Three ways are known up to now:
   1. Higher types of functions (functions of functions of number, etc.)
   2. The modal-logical route (introduction of an absurdity applied to universal sentences and a ⟦notion of⟧ "consequence").
   3. Transfinite induction, that is, inference by induction is added for certain concretely defined ordinal numbers of the second number class.

Gödel himself preferred the first alternative, worked out in [39]; he judged the second one, which is intuitionistic logic of Brouwer and Heyting augmented by a modal-like operator B (for German *beweisbar*), "the worst of the three ways" [38,

[12]German original [1, p.1f]: "Besonders interessiert hat mich der neue meta-mathematische Standpunkt, den Sie jetzt einnehmen und der durch die Gödelsche Arbeit veranlaßt worden ist." The letter was written after Ackermann visited Göttingen, but didn't meet Hilbert and spoke only with Arnold Schmidt, who informed him about "everything" going on in Göttingen.

[13]Detlefsen, [19] in this volume, however, points out that there are some fundamental differences between Gentzen's own philosophical view and Hilbert's view.

[14]In German: "Überschreitung des bisherigen methodischen Standpunkts der Beweistheorie".

[15]German original, [38, p. 94]:

4. *Wie also erweitern?* (Erweiterung nötig.) Drei Wege ⟦sind⟧ bisher bekannt:
   1. Höhere Typen von Funktionen (Funktionen ⟦von⟧ Funktionen von Zahlen, etc.)
   2. Modalitätslogischer Weg (Einführung einer Absurdität auf Allsätze angewendet und eines "Folgerns").
   3. Transfinite Induktion, d.h., es wird der Schluß durch Induktion für gewisse konkret definierte Ordinalzahlen der zweiten Klasse hinzugefügt.

p. 103]; the third one is, of course, Gentzen's way; for a detailed discussion of (this passage from) Gödel talk at Zilsel's seminar, see [24, p. 120f]. Of course, we don't depend on Gödel's choice; what counts is that *there are* extensions of Hilbert's original standpoint which provide a rationale for modern consistency proofs.

With respect to the second aspect of Hilbert's Programme—the supposed conservativity of higher Mathematics over finitistic Mathematics—the "failure" cannot be denied: there is no way to reduce all higher Mathematics to finitistic Mathematics; even more: higher Mathematics may prove finitistic statements which are not provable with pure finitistic methods.[16] But let's draw on a comparison here: nobody will deny that Columbus failed to find the sea route to India; but he didn't sink in the Ocean, he discovered America. In the same way, Hilbert's Programme, aiming for consistency and (maybe) conservativity, didn't sink in inconsistency, but discovered *Non-Conservativity*. Exploring this new phenomena in Mathematics is the driving force of modern proof theory.

3 Consistency Proofs for Arithmetic

Any consistency proof has to rely on some undisputed base. This was clearly stated by Gentzen, for instance in [31, Sect. 2.31][17]:

> Such a consistency proof is once again a *mathematical proof* in which certain inferences and derived concepts must be used. Their reliability (especially their consistency) must already be *presupposed. There can be no 'absolute consistency proof'*. A consistency proof can merely *reduce* the correctness of certain forms of inference to the correctness of other forms of inference.

[16] See, for instance, [75] in this volume.

[17] German original: "Ein solcher Widerspruchsfreiheitsbeweis wäre nun wieder ein *mathematischer Beweis*, in dem gewisse Schlüsse und Begriffsbildungen verwendet würden. Diese müssen als sicher (insbesondere als widerspruchsfrei) bereits *vorausgesetzt* werden. Ein '*absoluter Widerspruchsfreiheitsbeweis*' ist also *nicht möglich*. Ein Widerpruchsfreiheitsbeweis kann lediglich die Richtigkeit gewisser Schlußweisen auf die Richtigkeit anderer Schlußwiesen *zurückführen*. Man wird also verlangen müssen, daß in einem Widerspruchsfreiheitsbeweis nur solche Schlußweisen der Theorie, deren Widerspruchsfreiheit man beweist, als erheblich *sicherer* gelten können."

Similarly in [32]:

> In order to carry out a consistency proof, we naturally already require certain techniques of proof whose reliability must be *presupposed* and can no longer be justified along these lines. An absolute consistency proof, i.e., a proof which is free from presuppositions is of course impossible. [102, p. 237].

German original: "Um einen Widerspruchsfreiheitsbeweis zu führen, braucht man natürlich bereits gewisse mathematische Beweismittel, deren Unbedenklichkeit man *voraussetzen* muß und auf diesem Wege schließlich nicht weiter begründen kann. Ein absoluter, d. h. voraussetzungsloser Widerspruchsfreiheitsbeweis ist selbstverständlich unmöglich."

> It is therefore clear that in a consistency proof we can use only forms of inference that count as considerably *more secure* than the forms of inference of the theory whose consistency is to be proven. [102, p. 138]

Hilbert's original choice for such a base was finitistic Mathematics, and at that time, this was even identified—by name—with intuitionistic Mathematics in the Hilbert school.[18] Now, taking Heyting's intuitionistic formalization of Arithmetic as undisputed base, there was already a consistency proof of classical Arithmetic given by the double negation interpretation, independently found by Gödel [37] and Gentzen [33][19], and even earlier by Kolmogorov [65]. In his paper Gentzen expressed explicitly, [102, Sect. 6.1, p. 66f]:[20]

> If intuitionistic arithmetic is accepted as consistent, then the consistency of classical arithmetic is also guaranteed ...

But Gentzen was not happy with this kind of consistency proof (cf. the neat discussion in [102, p. 10f]), and went on to give his celebrated consistency proof in terms of transfinite induction up to ε_0. This proof starts from a different base, i.e., primitive recursive arithmetic together with transfinite induction up to ε_0.

Here, we dispense with a presentation of Gentzen's result which can be found, if not in Gentzen's original papers, in the standard proof-theoretic literature.[21] Hilbert, of course, was excited about the proof. But Kreisel [68, p. 121] reports also of "familiar jokes (for example, by Tarski whose confidence [in the consistency] was increased by $<\varepsilon$, or by Weyl who was astonished that one should use ε_0-induction to prove the consistency of ordinary, that is ω-induction)."[22]

Tarski's "joke" (or a variation of it) is referred in detail in [102, p. 10]: "Gentzen's proof of the consistency of arithmetic is undoubtedly a very interesting metamathematical result, which may prove very stimulating and fruitful. I cannot say, however, that the consistency of arithmetic is now much more evident to me (at any rate, perhaps, to use the terminology of the differential calculus more evident than by an epsilon) than it was before the proof was given" [109, p. 19]. However, for a "semanticist" like Tarski there cannot be any doubt about the consistency of

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[18] "Concerning the use of the word *intuitionistic* [...], it should be noted that according to Bernays [[11, p. 502]], the prevailing view in the Hilbert school at the beginning of the 1930s equated finitism with intuitionism." [24, p. 117]. See also footnote 6 above.

[19] This paper was submitted in 1933, but withdrawn by Gentzen when he became known about Gödel's paper. An English translation appeared in print in 1969, [102, #2], the German version of the Galley proofs, kept by Paul Bernays, was published only in 1974.

[20] German original [33, p. 131]: "Wenn man die intuitionistische Arithmetik als widerspruchsfrei hinnimmt, so ist [...] auch die Widerspruchsfreiheit der klassischen Arithmetik gesichert."

[21] An informal presentation of the main idea of the proof is given, for instance, by Takeuti in [120, p. 128ff].

[22] A well-known proof theorists presumably *heard* the second joke from Kreisel but confused a "y" with an "i" attributing it—with reference to Kreisel—to "un grand mathématicien français" [35, p. 520, fn. 14]; this confusion is confirmed in [36, pp. 9 and 33] where André Weil is mentioned by name (without reference to Kreisel).

Arithmetic from the very onset—otherwise, even the idea of the *structure of the natural numbers* would be pointless. We mention this, because on the assumption of the existence of a structure, any correctness lemma results in a consistency proof.[23]

Hermann Weyl's joke is equally unfair, as it suppresses the whole issue of Gentzen's proof, i.e., that the induction up to ε_0 is applied to quantifier-free formulas, only.[24]

Universal quantification—which was eliminated by Gentzen in the induction schemata—was at the very bottom of Hilbert's concerns, much more than, for instance, the tertium-non-datur. Hilbert's early outline of a consistency proof in the 1904 Heidelberg talk [47] was criticized by Poincaré with the argument that, for any such consistency proof, Hilbert would have to reason inductively[25]; but justifying induction by induction results in a vicious circle. Only with the separation of Metamathematics—using "weak" induction—from Mathematics proper—allowing for stronger induction—he developed a tool to respond to this critics.[26] Thus, Gentzen's use of quantifier-free inductions, though being transfinite, is fundamentally in line with Hilbert's concern to address Poincaré's objection.[27]

Ackermann gave, shortly after Gentzen, a consistency proof for Arithmetic using Hilbert's ε-*substitution method*, cf. [2], and its discussion in [53, Sect. 2] and [54, Supplement V].[28] From a historic point of view, it is probably more an adaptation of Gentzen's proof to a specific technique favored by Hilbert than a "new" consistency

[23]Smullyan [100, p. 56] illustrates very well this point in connection with Gödel's (first) incompleteness result, stressing that Gödel, by using ω-consistency, makes a much weaker assumption than correctness. The pointlessness of consisteny proofs by semantic methods was well stated by Shoenfield [96, p. 214]:

> The consistency proof for P by means of the standard model [...] does not even increase our understanding of P, since nothing goes into it which we did not put into P in the first place.

[24]For sure, Weyl will have known exactly what's going on here, and probably also classified his remark only as a *joke*.

[25]See [84], cited in [98, p. 7].

[26]See, for instance, [10, p. 203]. This separation might have been suggested by Brouwer to Hilbert in 1909, cf. [112, p. 302]. Sieg [98, p. 27] writes: "Hilbert claims in [[50]], that Poincaré arrived at 'his mistaken conviction by not distinguishing these two methods of induction, which are of entirely different kinds' and feels that '[u]nder these circumstances Poincaré had to reject my theory, which, incidentally, existed at that time only in its completely inadequate early stages'."

[27]It is defensible that Hilbert took Poincaré's critics more serious than, for instance, Brouwer's, cf. [61, 62]; but since Poincaré died already in 1912, Hilbert had lost him as discussion partner at the time his programme was worked out.

[28]This supplement, added to the second edition of [53] and published in 1970, also presents a consistency proof of Kalmár, based on an unpublished manuscript of 1938.

proof.[29] However, the ε-substitution method was recently revived by Mints for the analysis of stronger systems, cf. [4, 76, 78] and [77] in this volume.

Gödel [39] published in 1958 a conceptually different consistency proof, a worked out version of the idea already mentioned at Zilsel's seminar in 1938 (see above) which is based on functionals of higher types, known as *Gödel's* $\mathcal{T}$ (the theory) or the *Dialectica-Interpretation* (the interpretation of Arithmetic in $\mathcal{T}$). This consistency proof is quite different from Gentzen's, and it addresses particularly the *finitistic* aspect of Hilbert's programme, as the functionals of higher types can be considered as fulfilling this aspect.

Even if somebody would not be convinced by any single consistency proof, (s)he should take into account that here conceptually different approaches—intuitionism; transfinite induction; functionals of higher type—all lead to the consistency of Peano Arithmetic. For *Church's thesis* sometimes the argument is put forward that many independent approaches to computability lead to the same class of functions. We have here a similar phenomenon, where the risk—put forward for Church's thesis—of "systematically overlooking something" is even lower, and one gains some kind of independent evidences for the consistency of Arithmetic.

In any case, as the consistency of Arithmetic is not really at issue, for modern proof theory Gentzen's consistency proof must be put in the right perspective. Macintyre writes in this respect [72, p. 2426][30]:

> Much nonsense has been pronounced about Gentzen's work, even by extremely distinguished people. Consistency is not really the main issue at all. He did reveal fine structure in the unprovability of consistency of PA, as a consequence of much deeper general methodology.

4 Analysis

It should be clear that for Hilbert's Programme Arithmetic could have been only an intermediate goal on the way to Analysis. It was, of course, Analysis which Hermann Weyl had in mind when speaking about a "house built on sand," it was

[29] Cf. Bernays in [53, p. VII]:

> Currently, W. Ackermann is developing his earlier consistency proof—by use of a sort of transfinite induction as used by Gentzen—in a way that it obtains validity for the full numbertheoretic formalism.

German original: "Gegenwärtig ist W. ACKERMANN dabei, seinen früheren (...) Widerspruchsfreiheitsbeweis durch Anwendung der transfiniten Induktion in der Art, wie sie von GENTZEN benutzt wird, so auszugestalten, daß er für den vollen zahlentheoretischen Formalismus Gültigkeit erhält."

Von Plato writes in [115, end of I.4.10]: "A second proof of Gentzen's result was given by an unwilling Wilhelm Ackermann, after repeated pleadings on the part of Bernays."

[30] In the continuation of the citation, the mentioned fine structure is illustrated by the result about provably total functions of PA which one can obtain from Gentzen's work.

Analysis which Brouwer tried to "revolutionize" (using Weyl's language) within his intuitionistic philosophy. Analysis uses at its very base the definition of the real numbers, a genuine impredicative concept. It was, first of all, Poincaré who put the use of impredicative concepts into question (though he accepted the real numbers as such).[31] But also Hilbert's own student Weyl was advocating a predicative reconstruction of Mathematics in *Das Kontinuum* [117], being willing to give up a large part of traditional Mathematics. Thus, for Hilbert, a consistency proof of classical Analysis turned now from a "simple question" of his Paris problem list into an issue of defense against an intuitionistic "Putschversuch" (as he expressed it in [49]).

It is known that Gödel started from Analysis when he was still trying to fulfill Hilbert's programme; Wang [116, p. 654] reports: "In the summer of 1930, Gödel began to study the problem of proving the consistency of analysis. [. . .] The problem he set for himself at that time was the relative consistency of analysis to number theory." In this context he encountered the incompleteness results which, in turn, closed this lane of argumentation.

Thus, Gentzen's consistency proof of Arithmetic is now only a first step, and the search for a consistency proof of Analysis was started immediately after. We know that Gentzen was working hard on such a consistency proof even in prison in Prague just before his premature death in 1945,[32] and some remaining notes about this work are currently in the process of publication [115]. But, it is also clear that he didn't reach a final result.

In sharp contrast to intuitionistic Arithmetic, intuitionistic Analysis can hardly be considered as a base to provide a consistency proof for (classical) Analysis which would fit Hilbert's aims. One problem are the additional principles for intuitionistic Analysis proposed by Brouwer, which are inconsistent in the classical setting. This makes it doubtful whether intuitionistic Analysis (in Brouwer's formulation) could be even considered as more reliable than classical Analysis in itself.[33]

Szabo in [102, pp. 12–16] gives a short review of other early consistency results, going beyond Arithmetic, by Fitch, Lorenzen, Takeuti, Schütte, and Ackermann. None of them are accepted as fulfilling Hilbert's requirement on a consistency

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[31] See, for instance, the talk on *transfinite numbers* given by Poincaré in Göttingen in 1909 in the presence of Hilbert, included in [85] and translated by Ewald in [22, 22.G] (reprinted in [62]).

[32] Szabo [102, p. viii] refers to the memories of a friend of Gentzen in the prison: "He once confided in me that he was really quite contented since now he had at last time to think about a consistency proof for analysis. He was in fact fully convinced that he would succeed in carrying out such a proof."

[33] Here, one can turn Hilbert's programme upside down and use interpretations of new intuitionistic principles to justify them on classical grounds; see, for instance, [27, p. 340]. I also remember a proof theorist, making good use of such principles, but calling them—trained in classical Mathematics and therefore believing in the standard notion of mathematical truth—"totally wrong" (as translation of the German "grob falsch").

proof.[34] But they had, of course, some impact on the development of proof theory. The most stimulating proposal was Takeuti's *Fundamental Conjecture*, saying roughly that cut-elimination holds for second-order logic, cf. [107] and the informal presentation in [120, App. B]. There were soon some proofs of it [86, 93, 103, 106], which, however, rely on set theoretic considerations. Thus, these proofs do not provide additional reliability.[35]

Similar concerns regard other approaches, like Girard's $\mathcal{F}$ [34], where the *candidates*, used in the normalization proof, are subject to the same foundational concerns as the theory itself.[36,37]

Spector [101] introduced *bar recursion* as a concept which could be used to extend the Dialectica interpretation to Analysis.[38] To serve as a consistency proof, however, one would rely on bar recursion/bar induction as valid principle. Avigad and Feferman [8, p. 370f] write in their "Evaluation of Spector's interpretation":

> Spector was careful not to claim that the generalization of bar induction to higher types, which he used to justify bar recursion for continuous functionals, should be accepted on intuitionistic grounds. In fact, he offers the following caveat:
>
>> The author believes that the bar theorem is itself questionable, and that until the bar theorem can be given a suitable foundation, the question whether bar induction is intuitionistic is premature.
>
> The question of whether bar recursion can be justified on constructive grounds was taken up in a seminar on the foundations of analysis led by G. Kreisel at Stanford in the summer of 1963. The seminar's conclusion, summarized by Kreisel in an ensuing report [[69]], was that
>
>> ... the answer is negative by a wide margin, since not even bar recursion of type 2 can be proved consistent [by constructively accepted principles].

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[34]Kreisel, in [67, p. 344], sketches also an extension of "Gödel's old translation" of a system for classical Analysis to a specific intuitionistic reformulation of Analysis, involving the general Comprehension Axiom, which "*provides an intuitionistic consistency proof of classical analysis*". He himself classifies this result as "philosophically [...] not significant at all", except for "*a reduction to intuitionistic methods of proof*"—which he judges a "technical" property. In the Discussion of this proof he reminds the reader to look for alternatives:

> Quite naively, this easy proof in no way reduces the interest of a more detailed proof theoretic reduction [...]; just as Gödel's original intuitionistic consistency proof for classical arithmetic **Z** did not make Gentzen's reduction superfluous.

[35]In a discussion of these proofs, Kreisel writes [67, p. 349, footnote 16]: "[I]n terms of consistency proofs, Tait's argument would only have proved the consistency of classical analysis in third order arithmetic!"

[36]I remember a proof-theorist classifying such a normalization proof as simply "circular."

[37]The worst-case scenario was experienced by Martin-Löf, when he realized that the normalization proof of his first (inconsistent) type theory was carried out in an *inconsistent* metatheory (see Setzer's contribution in this volume [95]).

[38]For a thorough discussion of Spector's proof see [26] in this volume. Oliva and Powell [80], also in this volume, discuss some spin-offs we can get from proof-theoretic analyses in the neighborhood of Spector's approach.

When failing to prove Takeuti's Fundamental Conjecture by more elementary means, proof theory turned naturally to *Subsystems of Analysis* where impressive results were established. Following the two traditions, called *Schütte-style* and *Takeuti-style* proof theory, we are able to give today analyses up to Π^1_2 comprehension, cf. the work of Rathjen [88, 89] and Arai [5–7], respectively.[39] These analyses of subsystems of Analysis in terms of ordinals are the natural extension of Gentzen's consistency proof for Arithmetic. It is particularly rewarding to provide the *proof-theoretic strength* of a theory; with ordinals as measure one is able to compare theories from different formal realms, like set-theoretical ones, type-theoretical ones, or others like Theories of Inductive Definitions[40] and Feferman's Explicit Mathematics. In return, these frameworks can help to carry out parts of the proof-theoretic investigations.[41]

The rationale of ordinal analyses—in comparison with the approaches mentioned above—was recently described by a colleague in the following neat characterization:

> Something that makes specifically ordinal-theoretical proof-theoretical analyses of a theory particularly convincing is that in many cases there is a big difference between the metatheory and the object theory; whereas with normalisation proofs based on Tait-style computability, or Girard-style 'candidates', the meta-theory is (more-or-less) the theory itself together with a uniform reflection principle. Something would be far wrong if one couldn't prove a normalisation theorem for Church's theory of types in such a metatheory; but the extra confidence one gets in the principles formulated therein from a normalisation theorem is tiny.

Let us close this section with the reference to some subprogrammes which grew out of Gentzen-style proof theory and which reach out for Analysis.

In [23], Feferman gives a comprehensive survey on the "viable rationale" of *reductive proof theory*, using examples of "pairs" of frameworks where the first one is reduced to the second one. Whereas Hilbert's original hope about the pair ⟨infinitary, finitary⟩ is limited by Gödel's incompleteness theorems and only exemplified by reductions to PRA [23, 5.1], one can look at other pairs like ⟨uncountable infinitary, countable infinitary⟩ [23, 5.2]; ⟨impredicative, predicative⟩ [23, 5.3]; and ⟨non-constructive, constructive⟩ [23, 5.4].[42]

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[39]See [81, 82, 94, 108] for comprehensive presentations of the background of the respective developments.

[40]See, for instance, [14] and [57] in this volume.

[41]This was exemplified, in particular, by Kripke-Platek set theory, cf. e.g., [56, 81].

[42]In the further course of the discussion, Feferman expresses some doubts about current advances in ordinal analysis with respect to the given rationale [23, p. 80]:

> Even if one succeeds in reducing the system $(\Pi^1_2\text{-CA}) \pm \mathsf{BI}$ to a constructive system (whether evidently so or not), one can hardly expect that doing so will appreciably increase one's belief in its consistency (if one has any doubts about that in the first place) in view of the difficulty of checking the extremely complicated technical work needed for its ordinal analysis.

Another successful subprogramme is *Reverse Mathematics* which looks for the weakest natural subsystem of Analysis which proves a given mathematical theorem, cf. [99].

Finally, we like to mention *Applied Proof Theory*, sometimes also promoted under the name *proof mining*, which aims to extract additional mathematical information from an in-depth analysis of proofs in formal systems, cf. [64].

For all these subprogrammes the consistency issue is clearly secondary. But they all rely on the techniques which were developed to a large extent out of Gentzen's methods used for his consistency proofs.

5 The Quest for Consistency

It was in an informal conversation, years ago, that two distinguished proof theorists repeatedly assured each other that, for modern proof theory, "consistency is not the question." As a matter of fact, the working mathematician considers ZFC, Zermelo–Fraenkel set theory including the axiom of choice, being beyond doubt.[43] Let's have a look at Wiles's proof of Fermat's Last Theorem. As it stands, its formalization seems to require ZFC + some Grothendieck Universes on top [74]. This is an outrageously strong system for a theorem which can be formulated in Peano Arithmetic. But no Mathematician would raise a minimal doubt about Wiles's proofs *because* it makes use of such a strong theory.

As an expert in set theory, W. Hugh Woodin makes the following "prediction" [119, p. 453][44]:

> In the next ten thousand years, there will be no discovery of an inconsistency in these theories [referring to three equiconsistent theories, including ZFC + "There exist infinitely many Woodin cardinals"].

And Gaisi Takeuti points out that we cannot even imagine any longer the original concerns of Hilbert's times, [120, p. 122]:

> In the current day, axiomatic set theory is fully accepted and it is generally acknowledged that modern mathematics can be carried out in the framework of axiomatic set theory. No contradiction has arisen in axiomatic set theory, and a sense of security that no contradiction will arise in it in the future is supported by intuitive consensus. Under the current secure circumstances one cannot imagine the sense of crises of that earlier time.

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[43]This is, admittedly, in sharp contrast to the early times of axiomatic set theory, where Poincaré, for instance, expressed his doubts about Zermelo's axiomatization of set theory in the following words, cf. [43, p. 540]:

> But even though he has closed his sheepfold carefully, I am not sure that he has not set the wolf to mind the sheep.

[44]Of course, this prediction is embedded in a thorough discussion which gives arguments for this claim. But one may note that Woodin speaks here about the *discovery* not about the *existence* of an inconsistency.

Feferman, taking up explicitly an anti-platonist position, puts the following argument forward for the consistency of standard formal theories [23, p. 72][45]:

> I, for one, have absolutely no doubt that PA and even PA_2 are consistent, and no genuine doubt that ZF is consistent, and there seems to be hardly anyone who seriously entertains such doubts. Some may defend a belief in the consistency of these systems by simply pointing to the fact that no obvious inconsistencies are forthcoming in them, or that these systems have been used heavily for a long time without leading to an inconsistency. [...] My own reason for believing in the consistency of these systems is quite different. Namely, in the case of PA, we have an absolutely clear intuitive model in the natural numbers, which in the case of PA_2 is expanded through the notion of arbitrary subset of the natural numbers. Finally, ZF has an intuitive model in the transfinite iteration of the power set operation taken cumulatively. This has nothing to do with a belief in a platonic reality whose members include the natural numbers and arbitrary sets of natural numbers, and so on. On the contrary, I disbelieve in such entities. But I have as good a conception of what arbitrary subsets of natural numbers are *supposed* to be like as I do of the basic notions of Euclidean geometry, where I am invited to conceive of points, lines and planes as being utterly fine, utterly straight, and utterly flat, resp.

With respect to the standard formal theories, used in Mathematics, one may also cite Kreisel[46]:

> The doubts about the consistency are more doubtful than the consistency itself.

There is even an ironic corollary to Gödel's second incompleteness theorem with respect to "proof obligations": Gödel tells us that we cannot prove the (absolute) consistency of a formal mathematical theory. However, if somebody *believes* that a certain theory is inconsistent, (s)he would be committed to *prove* it, as this would be, of course, always possible. And such a person needs to be reminded of a word of Dedekind from 1887: "In science, what is provable should never be believed without proof."[47] But for one who believes in the consistency of a theory, Dedekind does not apply—thanks Gödel.

Thus, what should we think of these alleged threats of inconsistencies?

One might argue that the history of Mathematics is full of examples which one may consider as inconsistencies.[48] Mathematicians may apply a new concept in a way which results in false theorems. The simple fact that the supposed theorem

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45 The argument for the intuitive model of ZF is compared with the situation for Quine's *New Foundation* where the lack of such an intuitive model gives reason to look for a (relative) consistency proof.

46 Conveyed by Girard in French [35, p. 525]: "Les doutes quant à la cohérence sont plus douteux que la cohérence elle-même."

47 German original: "Was beweisbar ist, soll in der Wissenschaft nicht ohne Beweis geglaubt werden." cited and translated in [20, p. 97].

48 See, for instance, [12]: "Historically speaking, it is of course quite untrue that mathematics is free from contradiction" and later "[Contradictions] occur in the daily work of every mathematician, beginner or master of his craft, as the result of more or less easily detected mistakes, [...]"

is false implies that a proper formalization of the argument will show a formal inconsistency. However, in most cases, the solution was never a problem: either the argumentation was dismissed with invalid, or—a little bit more interesting—some fundamental assumptions about a certain mathematical area were revised which improved our understanding of the this area.

Euler, for instance, in his famous book on Algebra [21], calculated $\sqrt{-1}\sqrt{-4} = \sqrt{4} = 2$, applying the "general law" $\sqrt{a}\sqrt{b} = \sqrt{ab}$.[49] Adding this last "law" to the axioms of the field of complex numbers, of course, leads to an inconsistent theory. Such cases are not of much interest because, typically, the wrong assumption is easy to isolate and to separate from the part which will be kept after a revision.

But there are some interesting examples of inconsistencies in the history of Mathematics which transcend such simple instances and which deserve a closer inspection:

- Cantor's naive set theory;
- Frege's *Grundgesetze der Arithmetik*, and subsequent foundational systems by Curry, Church, Kreisel, and Martin-Löf;
- Reinhardt cardinals over ZFC.

Cantor's naive set theory may be based on an unreflected comprehension principle expressed in Cantor's famous first characterization of the notion of set[50]:

> By a 'set' we understand every collection to a whole M of definite, well-differentiated objects m of our intuition or our thought.

It was soon discovered that this characterization allows for inconsistent set constructions like the set of all cardinals (Cantor 1897, letter to Hilbert [17, letter 156]), the set of all sets (Cantor 1899, letter to Dedekind [17, letter 163]), or the set of all ordinals (Burali-Forti 1897 [113, pp. 104ff]). It is worth noting that Cantor himself did not see any problem here, but took the "paradoxes" just as *reductio-ad-absurdum* arguments of the inexistence of the respective sets; in his correspondence with Hilbert he refines, therefore, his notion of set by distinguishing it as "consistent multiplicities."[51] Thus, for Cantor it was natural that the (in)consistency of a set construction is verified *a*

[49]This example is taken from [20, p. 59].

[50]German original: "Unter einer 'Menge' verstehen wir jede Zusammenfassung M von bestimmten wohlunterschiedenen Objekten m unserer Anschauung oder unseres Denkens (welche die 'Elemente' von M genannt werden) zu einem Ganzen." [16, p. 282]. The translation is from [44, p. 33].

[51]In German: "consistente Vielheiten," letter to Hilbert from May 5th, 1899, [17, letter 160]; as "finished set" ("fertige Menge") already in a letter from December 2nd, 1897, [17, p. 390].

posteriori. Hilbert did not agree with such an approach and demanded an *a priori* justification.[52]

In practical terms, this was done by Zermelo in his axiomatization of set theory [121].[53] On the theoretical side, one finds here one of the motivations for Hilbert to propose consistency proofs *for theories* to ensure the meaningfulness of their mathematical notions.[54]

Frege's aim to give a logicist foundation of Mathematics in his *Grundgesetze der Arithmetik* [29, 30] was destroyed by Russell's Paradox. It is generally assumed that Frege's *Basic Law V* is responsible for the collapse of the system, but one may consider alternatives to resolve the problem.[55] What is of interest for us, as a lesson for the history of logic, is that Frege had some kind of *justification* of his axioms (one might as well call them *meaning explanations*). The problem was, that these were *local* justifications for the single axioms, but their combination turns out to be impossible; but it explains at the same time why we can single out *different* consistent and meaningful subsystems. The fate of Frege's system raises the question to which extent we can trust any philosophical justification programme based on local justifications (or meaning explanations).[56,57] What should provide

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[52]In [47] he writes, [113, p. 131]:

> *G. Cantor* sensed the contradiction just mentioned and expressed this awareness by differentiating between "consistent" and "inconsistent" sets. But, since in my opinion he does not provide a precise criterion for this distinction, I must characterize his conception on this point as one that still leaves latitude for *subjective* judgment and therefore affords no objective certainty.

In German (cited in [17, S. 436]): "G. Cantor hat den genannten Widerspruch empfunden und diesem Empfinden dadurch Ausdruck verliehen, daß er 'konsistente' und 'nichtkonsistente' Mengen unterscheidet. Indem er aber meiner Meinung nach für diese Unterscheidung kein scharfes Kriterium aufstellt, muß ich seine Auffassung über diesen Punkt als eine solche bezeichnen, die dem *subjektiven* Ermessen noch Spielraum läßt und daher keine objektive Sicherheit gewährt." An even stronger statement against Cantor's approach can be found in a lecture note from 1917, [48], cf. [59, 60].

[53]Although this axiomatization has the flaw that its justification is *extrinsic* where philosophers would prefer to have an *intrinsic* one, cf. e.g., the discussion in [73].

[54]One may note that Cantor's criterion for a "finished set" also requires a consistency proof, but somehow locally for the particular construction only. However, as far as we know, Cantor only took note of the criterion in the negative cases, to dismiss a set construction when it was shown to be inconsistent.

[55]For instance, Aczel's *Frege Structures*, [3].

[56]The situation becomes philosophically even more doubtful when such a justification depends, in addition, on the approval of a "Master". In this respect, Lorenzen complained about Brouwer [70]:

> Unfortunately, the explanation which Brouwer himself offers for this phenomenon [that some Mathematicians consider the 'tertium non datur' as unreliable] is an esoteric issue: only one who listened the Master himself understands him.

(German original: "Unglücklicherweise ist die Erklärung, die Brouwer selbst für dieses Phänomen anbietet, eine esoterische Angelegenheit: nur, wer den Meister selber hörte, versteht ihn.")

[57]A complementary view on this issue is given by Setzer [95] in this volume.

the evidence for a consistent combination if not a *global* justification—like a *model*—which, then, could also be used directly?

After Frege, there were four more prominent examples of inconsistent foundational systems: Curry's combinatory logic, Church's original λ-calculus (both subject to the Kleene-Rosser paradox), Kreisel's theory of constructions (subject to the Kreisel-Goodman paradox), and Martin-Löf's first type theory (subject to Girard's paradox). Although these systems represent three quite different approaches, it appears to us that the problems for all arise from the *philosophical* motivation rather than from a formal (logical) inaccuracy in the formalization.[58] This suggests the conclusion that philosophical motivations are apparently more dangerous for formal systems than pure mathematical motivations (as in the case of ZFC, for instance).

With a Reinhardt cardinal in ZFC we have, however, a completely different case of inconsistency. A Reinhardt cardinal is a certain *large cardinal* which was proposed by William Nelson Reinhardt in his doctoral dissertation in 1967, and shown to be inconsistent over ZFC by Kenneth Kunen in 1971. To get a glance of the fate of this cardinal—including its role in the absence of the Axiom of Choice where no inconsistency is known—one may consult [119, Sect. 20.3]; more information can be found in [63, Sect. 23]. In a simplified way, one can say that large cardinals constitute a branch of set theory which tries to settle the Continuum's Hypothesis on the basis of "new axioms."[59] It is a fascinating area which—despite in failing so far to settle ultimately the question of the Continuum's Hypothesis—produced a large amount of interesting results. The inconsistency of the Reinhardt cardinal over ZFC simply puts a bound on what one may add.

What is important for us here is that this inconsistency should not surprise one particularly. Even less should it raise a minimal doubt about the consistency of "ordinary reasoning" in Mathematics. To the contrary, large cardinal axioms are, in some sense, designed to push our axiomatic set theories to its ultimate limit; and the Reinhardt cardinals simply show that we went beyond this limit. As Kanamori puts it [63, p. 324]: "ZFC rallies at last to force a veritable Götterdämmerung for large cardinals!"

As upshot one can say that there is simply no serious threat of inconsistencies in Mathematics, if one doesn't approach intentionally its ultimate limits—or overstretch philosophical demands.

Still, there is an issue of consistency for Analysis—and, *a forteriori*, for set theory: the *impredicative* features might have just not been explored sufficiently to find a possible contradiction. And the reason for it might be that Mathematics uses only a very limited part of the formal theories, a part which resides in an innocent, consistent subsystem; in fact, *Reverse Mathematics* gives us strong evidence for

[58]This claim can be substantiated by the fact that it was not possible for any of the systems to modify it in a way that the original aims of the authors would be preserved.

[59]A thorough discussion of this issue can be found in [25].

such a claim. It was Gentzen himself who expressed the general concern in 1938 as follows, [102, p. 235]:[60]

> Indeed, it seems not entirely unreasonable to me to suppose that *contradictions* might possibly be concealed even in classical *analysis*. The fact that, so for, none have been discovered means very little when we consider that, in practice, mathematicians always work with a comparatively limited part of the logically possible complexities of mathematical constructs.

Thus, after recalling his consistency proof for elementary number theory, he came to the conclusion that "the most important [consistency] proof of all in practice, that for *analysis*, is still outstanding" [102, p. 236].[61]

By pursuing such a consistency proof, modern proof theory developed genuine techniques not only to achieve consistency results but also to analyze the fine structure of formal theories relevant for the mathematical practice.[62] In terms of our comparison above, we may say that pursuing the quest for consistency, Gentzen provided us with the tools to explore and to map the newly discovered land of unlimited mathematical strength.

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[61] German original [32, p. 7]: "doch steht der praktisch vor allem wichtige Beweis für die *Analysis* noch aus."

[62] Cf, e.g., [83] and [91] in this volume.

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