# NOMINALISTIC ORDINALS, RECURSION ON HIGHER TYPES, AND FINITISM

MARIA HÄMEEN-ANTTILA

**Abstract.** In 1936, Gerhard Gentzen published a proof of consistency for Peano Arithmetic using transfinite induction up to $\varepsilon_0$, which was considered a finitistically acceptable procedure by both Gentzen and Paul Bernays. Gentzen's method of arithmetising ordinals and thus avoiding the Platonistic metaphysics of set theory traces back to the 1920s, when Bernays and David Hilbert used the method for an attempted proof of the Continuum Hypothesis. The idea that recursion on higher types could be used to simulate the limit-building in transfinite recursion seems to originate from Bernays. The main difficulty, which was already discovered in Gabriel Sudan's nearly forgotten paper of 1927, was that measuring transfinite ordinals requires stronger methods than representing them. This paper presents a historical account of the idea of nominalistic ordinals in the context of the Hilbert Programme as well as Gentzen and Bernays' finitary interpretation of transfinite induction.

**§1. Introduction.** The most important event to the development of the Hilbert Programme in the 1930s was Gödel's discovery of the incompleteness theorems. Gödel had uncovered the essential relativity in the concept of formal proof and undermined David Hilbert's aim of providing an absolute consistency proof for all of mathematics. However, these conceptual difficulties were not what troubled Hilbert and his collaborator Paul Bernays the most. The more urgent problem was that no finitistic methods used so far were capable of proving the consistency of arithmetic, let alone of analysis.

As late as in March 1934, Hilbert writes in the introduction to the first volume of the *Grundlagen der Mathematik* [23]:

> Concerning this goal [of a finitary consistency proof], I would like to emphasise that the temporarily arisen opinion that from certain recent results of Gödel follows the infeasibility of my proof theory has been shown to be erroneous.[1]

Despite the bold claim, Hilbert and Bernays had little to say about how to achieve the desired consistency result. Still in 1935, Bernays [7] writes

Published in Bulletin of Symbolic Logic 25 (1): 101–124 (2019).

[1] Im Hinblick auf dieses Ziel möchte ich hervorheben, daß die zeitweilig aufgekommene Meinung, aus gewissen neueren Ergebnissen von Gödel folge die Undurchführbarkeit meiner Beweistheorie, als irrtümlich erwiesen ist.

that the conjecture that the consistency of arithmetic cannot be proven by finitary means has not been refuted. However, before the article went into print, a postscript was added:

> While this paper is being prepared for print, G. Gentzen has presented a proof of consistency of the full number-theoretic formalism, which employs a method that satisfies the fundamental demands of the finitary standpoint. Thereby the aforementioned conjecture about the range of the finitary methods finds its refutation.[2] [7, 216]

Gentzen's work, published in 1936 [16], was his second successful attempt to prove the consistency of arithmetic. His first proof of 1935 had been criticised by Bernays, as well as Gödel and von Neumann, for implicit use of the fan theorem [28, 173–175]. However, on January 15, 1936, he wrote to Bernays that he has devised a new proof in which the methods used are "altogether elementary and certainly finitary." [39, 245] The new approach was based on a reduction of proofs into a form where their correctness can be verified, and the finiteness of this procedure would be proven using transfinite induction, an idea that Gentzen had first considered in late 1931.[3] This time, Bernays enthusiastically approved of the proof.

The methods Gentzen uses in the proof fall under the scope of Primitive Recursive Arithmetic with the exception of transfinite induction over primitive recursive predicates up to $\varepsilon_0$. It was, therefore, exactly the step of transfinite induction that was crucial. The unconventional feature in Gentzen's 1936 proof is that he escapes the set-theoretic connotations of Cantor's ordinals by using arithmetical means to construct the ordinals below $\varepsilon_0$. This helped, in part, to make the method more attractive from the finitary point of view.

The strategy of arithmetically constructing transfinite ordinals had not been much utilised before Gentzen's 1936 work.[4] This way of representing ordinals could be called *nominalistic*; as Bernays notes in his review of Gentzen's proof, it "does not presuppose anything of metamathematics or of set theory." [9, 75] Similar notation systems were used in the reproduction of Gentzen's proof in the second volume of the *Grundlagen der Mathematik* [24] as well as Ackermann's 1940 proof for the consistency

---

[2]Während der Drucklegung dieses Referates ist von G. Gentzen der Nachweis für die Widerspruchsfreiheit des vollen zahlentheoretischen Formalismus erbracht worden, durch eine Methode, die den grundsätzlichen Anforderungen des finiten Standpunktes durchaus entspricht. Damit findet zugleich die erwähnte Vermutung betreffs der Reichweite der finiten Methoden ihre Widerlegung.

[3]Gentzen devoted a whole series of notes (labelled **WTZ** for *Widerspruchsfreiheit transfinite Zahlen*) to this topic, but unfortunately all of them were lost [39, 4].

[4]Church [12] and Kleene [26] first wrote on constructive notation systems two years later.

of arithmetic for Hilbert's $\varepsilon$-calculus [4]. Separating the method of transfinite induction from the Platonistic metaphysics underlying set theory was important from the finitary point of view, and nominalistic ordinals provided a way to understand transfinite sequences as potentially infinite structures instead of actually existing infinities. The potential aspect was crucial to Gentzen's justification of the use of transfinite induction in his proof.

Whereas it has been suggested that the methods used by Gentzen fundamentally transcended those used in the Hilbert school in the 1920s, one can find precursors to Gentzen's work in the pre-Gödelian Hilbert Programme. Transfinite induction was first used by Ackermann in his proof of consistency for second-order PRA in 1924 [2]. Moreover, the core idea of nominalistic ordinals dates back to at least 1924, when Hilbert and Bernays came up with the idea of arithmetising transfinite ordinals for the ulterior purpose of proving the Continuum Hypothesis. The proof involved the conjecture that transfinite recursion on countable transfinite ordinals or the so-called (cumulative) second number class could be reduced to primitive recursion on natural numbers. Whereas the conjecture was partly correct, Hilbert and Bernays did not take into account the fact that representability does not necessarily imply provability. Hilbert's doctoral student Gabriel Sudan showed in 1927 [35] that well-orders on natural numbers were primitive recursive *measurable* only up to $\omega^\omega$, that is, only well-orders below $\omega^\omega$ can be proven well-founded by primitive recursive methods. As Sudan's paper was largely unknown to his contemporaries, however, it would still take over a decade for Gentzen to come up with the idea of a *proof-theoretic ordinal* [19].

This paper will first examine the prehistory of nominalistic ordinals in Hilbert and Bernays' works as well as Sudan's forgotten result. The second half will discuss the use of nominalistic ordinals in Gentzen's and Ackermann's proof and their attempted finitary justification by Bernays and Gentzen. Bernays' and Gentzen's arguments place the potential interpretation of the "nested" infinities of transfinite ordinals at the centre of the debate. These arguments downplay the central role of intuition in finitism, and Gentzen's proof was later criticised by Gödel [20] for this exact reason. Although Gödel's criticism is, from a historical point of view, not conclusive, allowing for different kinds of intuition results in further problems in drawing boundaries for Bernays and Gentzen's extended finitism with respect to stronger constructive views. If one takes a closer look at Gentzen's conception of "finitist," one can see that he would have accepted this consequence: for him, the potential conception of infinite was what separated constructive mathematics from the classical, and the methodical distinctions between intuitionism and Hilbert's conception of finitism were, more or less, debates over details. Bernays, whose arguments about Gentzen's proof are remarkably similar to Gentzen's own, stands in a more difficult position.

**§2. Hilbert and Bernays on transfinite ordinals.** In the Winter Semester of 1924/25, Hilbert gave a lecture course in Göttingen under the title *Über das Unendliche* (not to be confused with the identically titled 1925 Münster lecture). Because these lectures were aimed at a nonprofessional audience, they are particularly clear and easy to follow. Nevertheless, Hilbert gave plenty of thought and attention to this lecture series in particular. In his *Nachlass*, two copies of the lecture notes can be found, both versions containing several handwritten notes in the margins [13, 666]. The lecture notes, almost 140 pages in length, probably covered 14 or 15 lectures. They are divided into five subsections, starting with a general introduction to the concept of infinite, its applications in the natural sciences, and finally, in set theory and logic. Approximately 20 pages are devoted to set theory, a discipline which, says Hilbert, captures the notion of infinity in the most profound way [13, 729].

One of the central themes in Hilbert's account of set theory is transfinite ordinals. Hilbert's presentation diverges from the standard one in that it involves no set-theoretic definitions for successor and limit ordinals. Instead, he shows that one can employ some very basic arithmetical means to build well-orders of type larger than $\omega$. He first invites the hearer to consider reordering the sequence of natural numbers $1, 2, 3 \dots$ into a new well-founded sequence. Such new well-orders have the same cardinality but not necessarily the same order type: e.g., the sequence $1, 3, 5, \dots 2, 4, 6, \dots$ has order type $\omega \times 2$, twice the type of the usual ordering of natural numbers. Hilbert gives a more complicated example of a well-order of natural numbers of type $\omega \times \omega$. In the table below, $a \prec b$ if either $a$ has fewer (possibly non-distinct) prime factors than $b$, or if $a, b$ have the same number of prime factors, $a < b$:

$$
\begin{array}{l}
1, 2, 3, 5, 7, 11, 13, 17, 19, \dots \\
4, 6, 9, 10, 14, 15, 21, 22, \dots \\
8, 12, 18, 20, 27, 28, 30, \dots \\
16, 24, 36, 40, 54, \dots \\
32, 48, 72, 108, \dots \\
\vdots
\end{array}
$$

As the 1924/25 lectures were intended for a general audience, one might say that Hilbert's presentation served a pedagogical purpose: it is relatively easy to grasp the basic idea, and one can immediately understand the difference between cardinals and ordinals. However, Hilbert also had a more practical application in mind. He mentions [13, 739], as an example of how powerful the theory of transfinite ordinals is, that one could prove the Continuum Hypothesis by mapping the second number class, i.e., the class of countable ordinals, onto the class of real numbers.

In the 1925 lecture in Münster [21], Hilbert attempts to prove this conjecture by means of finitary arithmetic extended with functionals of higher type, aiming to show that there is a one-to-one correspondence between the number-theoretic functions and the ordinals of the second number class. This is achieved by enumerating all primitive recursive functionals by their *height* or their variable-type, where type 0 ranges over numbers, and type $n+1$ over primitive recursive functions whose highest-type argument is of type $n$. Here Hilbert makes the drastic assumption that *any* number-theoretic function can be defined in this way; his first Lemma states that any existential quantifier occurring in the (formalised) proof of the Continuum Hypothesis can be replaced by a recursive definition realising it.

The second lemma states that definitions involving transfinite recursion can be reduced to ordinary recursion. In particular, the set-theoretic definition of the second number class can be replaced by an arithmetical one. In a 1927 lecture delivered in Hamburg, Hilbert mentions that the reference to transfinite ordinals can be avoided "since the numbers of the second number class can, as is well known, represented as well-orderings of the number sequence, i.e., by certain functions of two number variables with values $0, 1$, so that the sentence in question takes the form of a pure functional expression."[5] [22, 75]

The second lemma plays a large part in the finitary justification of Hilbert's proof attempt. Before there was a distinction between primitive recursion and general recursion, "ordinary recursion," at least in the context of the Hilbert Programme, usually referred to the former. Here Hilbert aims to reduce transfinite recursion to recursion on higher types, although the question of eventually reducing the higher-type functions to functions of type 1 is left open.

Paul Bernays, Hilbert's closest collaborator at the time, was at least partly responsible for the idea that definitions using transfinite recursion – such as the definition of an ordinal – could be reduced to ordinary recursion. Hilbert acknowledges Bernays' important role in his Münster lecture, where he thanks Bernays in particular for his help with the Continuum Problem [21, 190]. Similarly, in the Hamburg lecture, Hilbert

---

[5]Dies läßt sich aber vermeiden, da die Zahlen der zweiten Zahlenklasse bekanntermaßen durch Wohlordnungen der Zahlenreihe, d.h. durch gewisse Funktionen zweier Zahlenvariablen mit den Werten $0, 1$, dargestellt werden können, so daß der fragliche Satz die Form eines reinen Funktionssatzes annimmt.

emphasises Bernays' role in the work, suggesting that it should be seen as their joint effort [22, 85].

Among Paul Bernays' notes in ETH Zürich (Hs 973: 97) one finds six pages of notes titled "Zusammenstellung der im Sommer besprochenen Punkte zur Behandlung des Kontinuumproblems." From the content of the notes, it is clear that they are related to Hilbert's proof attempt. This suggests that the notes might have been written between autumn 1924 and summer 1925, before the Münster lecture. "Im Sommer," then, would refer to summer 1924, when Hilbert might have been preparing his Göttingen lecture course. The other option is that they were from 1925–1926, before the Hamburg lecture which took place in summer 1927 – Hilbert's proof attempt was subjected to serious criticism soon after the 1925 lecture, and he, too, gave up on the idea after 1928 (see [31, 59–60]).

Bernays' notes concern two main points: the interpretation of transfinite (i.e., quantified) expressions and transfinite recursion. In the first part, he notes there are three levels of transfiniteness: 1) quantifiers "for all," "there exists," and also the $\pi$-functions, 2) the $\varepsilon$-operator in the sense of "the object such that," and 3) the $\varepsilon$-operator in the sense of choice. We can interpret the $\varepsilon$ in the second way when $\varepsilon$ is substituted into a recursive definition; moreover, this is also possible in the case with a definition of a number of the second number class as a limit of a sequence.

What is more interesting for the purposes of this paper is the second section of the notes titled "II. Wesentliche vermutete, aber noch unbewiesene Sätze." Here we find only one such conjecture, namely:

> 1. Every number of the second number class definable by transfinite induction can also be defined through ordinary induction (as limit-building using sufficiently high variable types).[6]

This is directly followed by two pages of notes under the title "Th. d. Zahlen der 2$^{\text{ten}}$ Zahlkl. als Wohlordnung der gewöhnlichen Zahlenreihe." From what can be gathered from the rather concise notes, Bernays' train of thought seems to run as follows. Let $\prec_f$ denote a recursive well-ordering defined by the function $f$, so that

$$
\begin{aligned}
f(a,b) &= 0 \quad \text{iff } a = 0 \text{ or } b = 0 \\
f(a,b) &= 1 \quad \text{iff } a = b \neq 0 \\
f(a,b) &= 2 \quad \text{iff } a \prec_f b \\
f(a,b) &= 3 \quad \text{iff } b \prec_f a
\end{aligned}
$$

The first clause for $f(0,0) = 0$ states that 0 is not a part of the ordering – Bernays, like Hilbert, starts counting at 1.

[6] Jede durch transfinite Induktion definierbare Zahl d. zweiten Zahlenkl. kann auch durch gewöhnliche Rekursion (als[?] Limesbildung, bei Anwendung von genügend hohen Variablentypen) definiert werden.

Given the functions $f_1, f_2, \dots f_n$ that define such well-orders, we can build a higher-type function $\psi(f_1, f_2, \dots f_n, a, b)$ which defines a well-ordering of type greater than any of the types defined by the functions $f_1, f_2, \dots f_n$. Bernays gives two examples of such functions that correspond to the sum of two ordinals $\alpha + \beta$ and the successor of an ordinal $\alpha'$. Let us consider the first example. Let $f, g$ define well-orders of types $\alpha, \beta$ on the natural numbers. Let

$$\begin{array}{lcll} \varphi(f,g,a,b) & = & f(\frac{a}{2},\frac{b}{2}) & \text{iff } a,b \text{ are even} \\ \varphi(f,g,a,b) & = & g(\frac{a-1}{2},\frac{b-1}{2}) & \text{iff } a,b \text{ are odd} \\ \varphi(f,g,a,b) & = & 2 & \text{iff } a \text{ is odd and } b \text{ is even} \\ \varphi(f,g,a,b) & = & 3 & \text{iff } a \text{ is even and } b \text{ is odd} \end{array}$$

Then the order type of $\prec_\varphi$ is $\alpha + \beta$.

To give a concrete example, consider the two well-orderings $\prec_f, \prec_g$:

$$\begin{array}{ll} \prec_f\colon & 1,3,5,7,\dots 2,4,6,10,\dots \\ \prec_g\colon & 1,4,7,\dots 2,5,8,\dots ,3,6,9,\dots \end{array}$$

Here $\prec_f$ simply orders the odds before the evens; $\prec_g$ is defined so that numbers $n$ such that $n+2$ is divisible by 3 precede the numbers $n$ such that $n+1$ is divisible by 3, which are followed by the numbers divisible by 3, all three sequences in their natural order.

Ordering the even numbers by $\prec_\varphi$, using $f$, we obtain the sequence

$$2,6,10,14,\dots ,4,8,12,16,\dots$$

Reordering the odd numbers by $g$ the sequence becomes

$$3,9,15,\dots 5,11,17,\dots 7,13,19,\dots$$

Finally, by placing odd numbers before even, we obtain $\prec_\varphi$ which has order type of $\prec_f + \prec_g$, that is, $\omega \times 5$:

$$\prec_\varphi\colon \quad 2,6,10,\dots 4,8,12,\dots 3,9,15,\dots 5,11,17,\dots 7,13,19,\dots$$

The first detailed example of how to form well-orders of natural numbers in terms of recursion on higher types, therefore, seems to come from Bernays. Bernays' conjecture would turn out to be false for the whole second number class, but it does hold for the class of recursive ordinals. This still leaves us with plenty of ordinals, but the crucial question is, how complex are these higher-order recursions? Whether or not recursion on higher types could be reduced to recursion on type 0 variables was answered neither by Hilbert nor by Bernays. In the Münster lecture, Hilbert notes that the proof needs further work to comply with the demands of the finitary standpoint [21, 190]; however, he seems confident that a finitist proof could be given. It appears that both Hilbert and Bernays were optimistic – although neither states explicitly that arithmetised ordinals and higher-order recursions should be considered finitary, the introduction of them was seen as relatively unproblematic.

It is now known that building relatively large well-orderings of natural numbers is possible by primitive recursive means. In fact, this is possible for the whole set of constructive ordinals [27]. However, it is one thing to construct well-orders and another to prove their well-foundedness. This distinction between formalisability and provability – in the particular case of nominalistic ordinals, but also more generally – was not yet appreciated by Hilbert and Bernays. Even before Gödel's first incompleteness theorem, a Romanian logician Gabriel Sudan discovered a result that, had it been acknowledged by his contemporaries, should have rung an alarm: the proof of well-foundedness could be carried out in PRA only up to the relatively small ordinal $\omega^\omega$.

**§3. Interlude: The Sudan ordinal $\omega^\omega$.** What Hilbert and Bernays failed to take into account is that any well-ordering *definable* in a certain system need not be *measurable* in the same system. This would become clear in Gentzen's consistency proof which used transfinite induction only up to $\varepsilon_0$. However, this distinction was hinted at in a much earlier paper written by the Romanian logician Gabriel Sudan.

Sudan's article, "Sur le nombre transfini $\omega^\omega$" [35], was published in 1927. It dealt with the question Hilbert had left unanswered in the Münster lecture: can all higher-type functions be reduced to lower-type functions? In this form, the question is more clearly equivalent to the question of whether all recursive functions reduce to primitive recursive ones, a conjecture whose refutation we usually attribute to Wilhelm Ackermann [3]. In his paper, Sudan, too, proves that this is not the case. The two discoveries are independent, and Ackermann does acknowledge Sudan's article [3, 119], which was not yet published by the time that Ackermann's article went into print. He does not seem to make a connection with his own work and only mentions that Sudan's result shares some similarities with his.

Despite the fact that he wrote his dissertation [34] under Hilbert's supervision in 1925, Sudan's article remained virtually unknown to his contemporaries. This could have been partly due to the fact that it was published in a little known Romanian journal, and because the title was too obscure to give an idea of the contents of the article. There was one contemporary review by Fraenkel, which contains a very short and general summary of the article [14]. Sudan's work can actually be found in the bibliography of Rózsa Péter's *Rekursive Funktionen* [33], though no reference to it can be found in the whole text. [11] seems to be the first source to describe Sudan's result in detail.

Another reason for why Sudan's proof was dismissed was that in general, there was no terminology for primitive recursive and general recursive functions at the time. The term "primitive recursive" was coined by Péter in 1935 [32] in order to differentiate between primitive recursive and general recursive functions. As mentioned, in 1925, Hilbert did not actually use the term "primitive recursive," but rather "ordinary recursive" or sometimes simply "recursive." Because Sudan's work seemed to be related to transfinite recursion, and Ackermann wrote in terms of recursion on natural numbers, the connection between the two articles might have not been so obvious for someone who quickly browsed through the text.

In his paper, Sudan not only shows the existence of a non-primitive recursive function, but he also suggests that there is a way of measuring the strength of typed functionals. His first result states that it is not possible to define the ordinal $\omega^\omega$ by any primitive recursive function, i.e., a function of type 1. He also makes the more general conjecture that there exists, for any function of type $\alpha$ for finite or transfinite $\alpha$, the least ordinal not definable by any function of type $\alpha$. However, no more limit ordinals are given, and neither Sudan nor Ackermann were able to pin down the class of general recursive functions as opposed to non-recursive ones.

Sudan, like Ackermann, also constructs a recursive function that is not primitive recursive. The definition is similar to Ackermann's function, except that the proof of non-primitive recursiveness is given by the fact that one can construct the ordinal $\omega^\omega$ with the help of this function. Sudan's original definition is:

$$\begin{aligned} \psi(a,b,0) &= a+b \\ \psi(a,b,n+1) &= t_c(a, \lambda_m \psi(c,m,n), b) \end{aligned}$$

Here $\lambda_n f(n)$ denotes the least ordinal larger than any of $f(0), f(1), \ldots$ through all $f(n)$; $a$ is an ordinal number and $b$ is a natural number. The function $t_c$ is defined as follows:

$$\begin{aligned} t_c(a, f(c), 0) &= a \\ t_c(a, f(c), n+1) &= f(t_c(a, f(c), n)) \end{aligned}$$

Here $f(c)$ is of type 1. The series $\psi(0,1,1), \psi(0,1,2), \psi(0,1,3)\ldots$ create the sequence $\omega, \omega^2, \omega^3, \ldots$ the limit of which is $\lambda_n \psi(0,1,n) = \omega^\omega$, the least ordinal not obtainable by primitive recursion.

Sudan expresses his result in terms of non-constructibility of the ordinal $\omega^\omega$ by primitive recursion. Considering the remark of the last section on the primitive recursive constructibility of any recursive well-order, this use of words might be slightly confusing. What Sudan in fact shows is that there is no recursive function $f$ such that $f$ is a mapping from ordinals below $\omega^\omega$ to an ordinal equal to or greater than $\omega^\omega$. For our purposes, we might as well say that there is no recursive $f : \mathbb{N} \mapsto \omega^\omega$, although such an $f$ can be found for any ordinal less than $\omega^\omega$.

Sudan's result about $\omega^\omega$ comes very close to the notion of proof-theoretic ordinal, an idea which is often traced back to Gentzen's habilitation thesis of 1943 [19]. A proof-theoretic ordinal for a formal system can be characterised as the least ordinal that cannot be proven well-founded within the system. From Sudan's result we gather that for Primitive Recursive Arithmetic, this ordinal is equal to or less than $\omega^\omega$. His conjecture implies that there are similar limits to other formal systems as well. Nevertheless, the importance of this observation would not be understood until Gentzen's result of 1936.

**§4. Nominalistic ordinals in Gentzen's and Ackermann's consistency proofs.** The idea of using transfinite induction to prove the consistency of a system was already present in Ackermann's 1924 consistency proof for second-order primitive recursive arithmetic [2]. Ackermann, as well as Hilbert, originally believed he had proven the consistency of the full system of analysis. The proof does not explicitly use transfinite induction, but rather induction on sequences of indices defined by nested recursion in a process which is quite similar to his 1940 proof (see [41]). Von Neumann [38] expressed some criticism of Ackermann's proof, but it only became clear after Gödel's that consistency of analysis had not been proven. In response, Hilbert and Bernays wrote that Gödel's theorem only established that finitary methods should be "sharpened." Nevertheless, their focus was clearly fixed on obtaining a consistency proof of arithmetic, and hopefully, of analysis, that could be called finitary.

Yet there was no specific idea of how such a proof should proceed. Bernays mentions proof by transfinite induction as a possibility in a lecture of 1934 [8, 91]. This method seems to have been in Gentzen's mind even earlier, although he first attempted a semantic proof which did not, according to Bernays and others, satisfy the requirements of the finitist approach. Unfortunately, we have no access to Gentzen's earlier notes related to transfinite induction, for the series of notes devoted to the theme, **WTZ** (*Widerspruchsfreiheit transfinite Zahlen*), seems to have been either lost or destroyed ([39, 4]).

The general idea of a consistency proof by transfinite induction is this: to prove that the statement $0 = 1$ is not derivable in the system, one first needs to reduce each possible proof into a form where its correctness can be checked. A crucial part of this reduction procedure is to show that it indeed ends at some point. Therefore each reduction step is indexed by an ordinal, which can be shown to decrease with each reduction. The reason why the reduction chain can have a transfinite ordinal is essentially the rule of complete induction, expressed in Gentzen's sequent calculus as

$$\frac{\begin{array}{c}\vdots \\ \Gamma \to \varphi(0)\end{array} \qquad \begin{array}{c}\vdots \\ \varphi(n), \Delta \to \varphi(n+1)\end{array}}{\Gamma, \Delta \to \varphi(t)}\ \mathrm{CI}$$

Here $t$ is any term, with the condition that $n$ does not occur in any of $\Gamma, \Delta, \varphi(0), t$. In the reduction process, the left side of the proof multiplies into a string of subproofs of $\varphi(0) \to \varphi(1), \varphi(1) \to \varphi(2), \varphi(2) \to \varphi(3), \ldots$, depending on the arbitrary number that has to be substituted for $n$ in the proof. The fact that $\varphi$ might have any number of quantifiers can make such a reduction procedure very long, and therefore ordinal numbers all the way up to $\varepsilon_0$ are needed.

Gentzen's proof of 1936, published in *Mathematische Annalen*, is over 70 pages long. The consistency proof itself takes up less than third of the article. The first sections discuss finite and transfinite methods in mathematics and the final sections focus on the finitariness of the step of transfinite induction. It was important to Gentzen to place his result within the context of the Hilbert Programme, although as is suggested in section 5, his picture of the Programme was not quite the same as Hilbert and Bernays'.

The finitary aspect is underlined by the fact that Gentzen, too, employed nominalistic ordinals in his proof. Avoiding the limit clause of standard transfinite recursion was essential to his justification of transfinite sequences as finitarily acceptable. The conventional way to define a limit ordinal $\alpha$ is to take the infinite union of all ordinals $\gamma < \alpha$, an operation that, from a finitist viewpoint, assumes the actual conception of infinity (as a complete totality), as opposed to the potential one (possibility of indefinite iteration). In his letter to Bernays, written on January 15, 1936, Gentzen remarks that the form of notation that he uses for the ordinals is "the most elegant" of the three options he considered. The other possibilities would have been the standard set-theoretic notation and an alternative form of the notation he in fact used in his proof [39, 245].

We will now turn to Gentzen's notation as well as the two alternative notation systems used by Hilbert and Bernays [24] and Ackermann [4].

The system Gentzen uses is based on rational numbers expressed in decimal notation. The *numerus*, i.e., the number on the left side of the decimal point, denotes a *system*. The *mantissa*, i.e., what follows behind the decimal point, denotes an ordinal number in a system, and is expressed in a base-3 notation. Gentzen's idea is to construct a finite sequence of systems, each defining a well-ordering, where each system has a higher order type than the previous one. The limit of these order types is $\varepsilon_0$.

The mantissae of system 0 run through all finite strings of 1's and end with 2:

$$1, 11, 111, 1111, \dots 2$$

The system, then, has order type $\omega + 1$. This corresponds, in a base-10 notation, to running through sums of finite powers of 3 in increasing order, followed by the number 2. In general, system $n + 1$ begins with the number 1. Each row begins with a mantissa $m$ corresponding to an ordinal from the system $n$, in an increasing order, followed by a sequence of numbers of the form $m\underbrace{0\dots0}_{\text{n+1 zeros}}\dots$. Here the suffix " … " runs through all numbers $k$ such that $k \prec_{n+1} m$.

To give an example, system 1 builds a sequence that begins with

$$\begin{array}{l} 1 \\ 11, 1101 \\ 111, 11101, 111011, 11101101 \\ 1111, 111101, 1111011, 111101101, \dots \\ \vdots \\ 2, 201, 2011, 201101, 20111, 2011101 \dots \end{array}$$

System 2 builds upon system 1: every ordinal of system 1 determines a row in system 2, where strings are now separated with two zeros. The order defined in system 2 begins with $1, 11, 11001, 1101, 1101001, 11010011, \dots$

This paper will not go into the particularities of Gentzen's notation system that has already received some attention in the literature. Gentzen's original consistency proof and his ordinal notation are explained in detail in [25]. An even more recent presentation of Gentzen's proof and its interpretation in the ordinary set-theoretic notation can be found in [5].

There are two other variants of Gentzen's consistency proof that use alternative notation systems. The first appears in the reconstruction of Gentzen's proof in the second volume of Hilbert and Bernays' *Grundlagen der Mathematik* in 1939, and the second in Ackermann's proof of consistency for Hilbert's $\varepsilon$-calculus in 1940. We will first look briefly at the notation systems in both proofs before discussing the question of finiteness of these constructions.

The Hilbert-Bernays notation system is based on prime factorisation. The sequence of orders is given by the following definition:

$$a \prec_0 b \quad \text{iff} \quad a < b$$

$$a \prec_{n+1} b \quad \text{iff} \quad \begin{cases} a = 0 \text{ and } b \neq 0 \\ \text{or there is an } n\text{-greatest } i \text{ s.t. } \varphi_i \text{ is a prime} \\ \quad \text{divisor of } b \text{ such that } DIV(\varphi_i, a) < DIV(\varphi_i, b) \end{cases}$$

Here $DIV(x, y) = \max z \leq y$ such that $x^z \mid y$. $\varphi_i$ is read as "the $i$th prime number."

It should be noted that all of the definitions are primitive recursive. E.g., the formal definition of the second line above reads

$$a \prec_{n+1} b \quad \Leftrightarrow \quad (a = 0 \,\&\, b \neq 0) \vee \exists \varphi_i \leq b((\varphi_i \mid b) \& \, \forall \varphi_j \leq max(a, b) \\ (i \prec_n j \supset (\varphi_j \nmid a \,\&\, \varphi_j \nmid b)) \,\&\, DIV(\varphi_i, a) < DIV(\varphi_i, b))$$

where we assume the prime numbers (and the number 1) have been enumerated in their natural order $\varphi_1 = 1, \varphi_2 = 2, \varphi_3 = 3, \varphi_4 = 5, \ldots \varphi_n, \ldots$

The initial segment of the Hilbert-Bernays order $\prec_1$, again best represented in a table, gives an idea of how such a construction actually looks like:

$0$
$1$
$2, 2^2, 2^3, 2^4, \ldots$
$3, 3 \times 2, 3 \times 2^2, 3 \times 2^3, \ldots 3^2, 3^2 \times 2, 3^2 \times 2^2, \ldots 3^k, 3^k \times 2, 3^k \times 2^2, \ldots$
$5, 5 \times 2, 5 \times 2^2, \ldots 5 \times 3, 5 \times 3 \times 2, 5 \times 3 \times 2^2, \ldots 5 \times 3^k, 5 \times 3^k \times 2, \ldots$
$\vdots$
$\varphi_m, \varphi_m \times 2, \ldots \varphi_m \times 3, \ldots\ldots \varphi_m \times \varphi_{m-1}, \varphi_m \times \varphi_{m-1} \times 2, \ldots$
$\vdots$

One can soon see a pattern: the two first rows (let us denote them by 0 and 1) have only one element each.[7] Row 2 runs through all finite powers of 2 and has order type $\omega$. Row 3 starts with the number 3, which is then followed by its product with each of the elements in row 2, in an increasing order, then moves onto $3^2$ and repeats the same pattern, and so on. Each subsequence has order type $\omega$, and with iteration through all finite powers of 3, one obtains an order of type $\omega \times \omega = \omega^2$. The next row begins with the prime number 5, and runs through everything on row 2 as well as row 3 before moving on to $5^2$. This produces an order of type $\omega^3$. The general pattern is that, except for rows 0 and 1, each row $r$ begins with the $r$th prime $\varphi_r$ and the rows grow neatly in powers of $\omega$.

[7]Here Hilbert and Bernays do count zero as the first ordinal.

As the sequence $0, 1, 2, 3, 5, \ldots$ followed by all the other prime numbers is of order type $\omega$, the type of the whole $\prec_1$-order will be exactly $\omega^\omega$.

As one proceeds to the next order $\prec_2$, the prime numbers are reordered by the place of their indices in the order $\prec_1$. Then the first primes will be $\varphi_1, \varphi_2, \varphi_{2^2}, \varphi_{2^3}, \ldots$. As in Gentzen, the order type of the rows grows exponentially, and thereby the order type of $\prec_2$ is $\omega^{\omega^\omega}$. In general, the order type of $\prec_{n+1}$ will be the order type of $\prec_n$ to the power of $\omega$.

The third example of the use of arithmetised ordinals in a consistency proof is from Ackermann's 1940 proof [4]. The proof originated from Bernays' request to give a Gentzen-style proof for Hilbert's $\varepsilon$-calculus. Bernays writes to Ackermann on November 27, 1936 (Hs 975: 100), asking Ackermann whether he "is of the opinion that the method of proving finiteness by transfinite induction can be applied in the consistency proof of your dissertation."[8] It is difficult to say if Bernays failed to see that Ackermann essentially used transfinite induction in his dissertation. That Bernays did not see the similarity between the two proofs is weakly supported by an article of 1935 [8, 89–90], where Bernays mentions the possibility of a consistency proof by transfinite induction, but Ackermann's name is not mentioned.

Bernays and Ackermann exchanged several letters discussing the proof and its significance. Ackermann's side of the correspondence can be found from Bernays' *Nachlass.* He seemed reluctant at first to carry through the proof; in a letter of December 5, 1936, to Bernays (Hs 975: 101), he asks whether there is any value in a new proof once Gentzen's had been published. He adds that Bernays had mentioned once that Gentzen's proof was not entirely satisfying, but that he had assumed that the criticism concerned the first proof of 1935.

Gentzen's 1936 proof used a variant of sequent calculus which was not well-known at the time and had no direct link to the methods used by Hilbert and Bernays. Using an axiomatic system might, therefore, make the proof more widely accessible. However, the reformation of the failed proof of 1924 would also yield a symbolic victory for the Hilbert Programme.

Bernays' reply of December 29, 1936, is quoted in [1]. Bernays writes that his suggestion was not meant as criticism towards Gentzen's proof, but that

> it might be that the assignment of the numbers of the second number class based on the proof idea of your dissertation turns out more satisfying; and in any case, I would find it favourable if, by

[8] Es würde mich sehr interessieren von Ihnen zu hören, ob Sie der Meinung sind, daß sich die Methode des Endlichkeitsbeweises durch transfinite Induktion auf den Wf-Beweis Ihrer Dissertation anwenden lasse.

> exploiting the extended methodical standpoint, the proof procedure of your dissertation and, at the same time, the first Hilbertian approach to the consistency proof would be reconstructed.[9] [1, 184]

The letter suggests that Bernays' motivation was to link the proof more directly to the Programme, which was now – Hilbert was 74 years old in 1936 – mostly his responsibility to promote. Ackermann, however, was not particularly interested in the finitist interpretation of the proof. The idea was set aside for almost two years, until Ackermann returned to the topic in a letter of June 29, 1938 (Hs 975: 114).[10] An improvement that could be made to Gentzen's proof, he notes, is giving approximation to the upper bound of the length of the reduction chains:

> I have, in fact, taken a closer look at Gentzen's finiteness proof for his reduction procedure. Here, if I understand right, it is only shown that in each reduction, a transfinite ordinal number is lowered, and thus finiteness is concluded. No explicit specification for the decrease of single reductions needed for a given proof-figure is provided. Now, I would like to ask whether you see such a finiteness proof as fully satisfactory from the point of view of proof theory.[11]

Ackermann adds that his attempts at such an approximations had so far failed. December 5, 1938 (Hs 975: 117), he writes to Bernays that he will use ordinal notation similar to the one in the second volume of the *Grundlagen*. It took him, however, until June, 1939, to figure out a satisfactory solution for the finiteness proof (Hs 975: 119). The proof was published in 1940.

Ackermann's nominalistic system, which is already explained in detail in the letter of December, 1938, is based on binary notation. The series of $\prec_n$-orders are built as follows:

---

[9] Es könnte aber doch sein, daß sich anhand des Beweisgedankens Ihrer Dissertation die Zuordnung zu den Zahlen der zweiten Zahlenklasse etwas angenehmer gestaltet; und jedenfalls würde ich es sehr erfreulich finden, wenn durch die Verwertung des erweiterten methodischen Standpunktes das Beweisverfahren Ihrer Dissertation und zugleich der erste Hilbertsche Ansatz für den Wf.-Beweis rehabilitiert würde.

[10] Unfortunately, no letter drafts of Bernays' on this topic can be found from the archives at ETH Zürich, although the main points of the correspondence can be figured out from Ackermann's letters.

[11] Ich habe mir nämlich den Gentzenschen Endlichkeitsbeweis für sein Reduktionsverfahren mal etwas näher angesehen. Hier wird, wenn ich recht sehe, nur gezeigt, dass sich bei jeder Reduktion eine transfinite Ordnungszahl erniedrigt, und daraus auf die Endlichkeit geschlossen. Eine explizite Angabe der für die Reduzierung einer bestimmten Beweisfigur notwendigen Einzelreduktionen wird nicht gegeben. Ich möchte nun fragen, ob Sie einen derartigen Endlichkeitsbeweis vom Standpunkt der Beweistheorie aus für voll befriedigend ansehen.

Let $a$ be of the form $2^k(2m+1)-1$ and $b$ of the form $2^l(2n+1)-1$. Then

$$a \prec_1 b \quad \text{iff} \quad \begin{cases} k < l \\ \text{or if } k = l, m < n \end{cases}$$

For $n \geq 1$, let $a = 2^{a_1} + 2^{a_2} + ... + 2^{a_i} - 1$ and $b = 2^{b_1} + 2^{b_2} + ... + 2^{b_j} - 1$, where where exponents $a_1, ...a_i, b_1, ...b_j$ are ordered by $\prec_n$ so that $a_1 \prec_n a_2 \prec_n ... \prec_n a_i$ and $b_1 \prec_n b_2 \prec_n ... \prec_n b_j$. Then

$$a \prec_{n+1} b \quad \text{iff} \quad \begin{cases} \text{there is some } k \text{ such that for all } l < k, a_l = b_l \text{ and } a_k \prec_n b_k \\ \text{if } a_k = b_k \text{ for all } 1 \leq k \leq i, i < j \end{cases}$$

Apart from the first order (Gentzen's system 1), the pattern formed by Ackermann's orders is similar to those of Gentzen's. The table below shows how the first sequences of an order $\prec_{n+1}$ unravel:

$$\begin{array}{l}
2^{a_1} - 1 \\
2^{a_2} - 1, 2^{a_2} + 2^{a_1} - 1 \\
2^{a_3} - 1, 2^{a_3} + 2^{a_1} - 1, 2^{a_3} + 2^{a_2} - 1, 2^{a_3} + 2^{a_2} + 2^{a_1} - 1 \\
2^{a_4} - 1, 2^{a_4} + 2^{a_1} - 1, 2^{a_4} + 2^{a_2} - 1, 2^{a_4} + 2^{a_2} + 2^{a_1} - 1, \dots \\
\vdots \\
2^{a_m} - 1, 2^{a_m} + 2^{a_1} - 1, 2^{a_m} + 2^{a_2} - 1, \dots 2^{a_m} + 2^{a_{m-1}} - 1, 2^{a_m} + 2^{a_{m-1}} + 2^{a_1} - 1, \\
\quad 2^{a_m} + 2^{a_{m-1}} + 2^{a_2} - 1, \dots \\
\vdots
\end{array}$$

Nevertheless, as Sudan's result suggests, the methods needed for measuring such orders would have to go well beyond Primitive Recursive Arithmetic, and indeed, even beyond Peano Arithmetic. The fact that one can construct a well-ordering of a given type does not imply that one can as easily prove its well-foundedness, which is needed for the consistency proof. The general strategy is the same in all three presentations. In Bernays' terms (section 2), one first defines a well-order $f_1(a, b)$ over the natural numbers corresponding to the first order $\prec_1$.[12] Then $f_{n+1}(a, b) = g(f_n, a, b)$ where $g$ represents the instructions for constructing, given an ordering function $f$ and two ordinals $a, b$, a new well-order.

**§5. The question of finitariness.** The formal picture was not as clear as Hilbert and Bernays had thought in the 1920s, and therefore it

[12] Gentzen's and Ackermann's definitions involve different definitions for $\prec_1$ and the rest of the $n$-orders, and in this case, two definitions for $f_1, f_2$ are needed in place of $f$.

was not obvious that Gentzen's proof was finitary. In fact, many believed that it was not. The common conception today is that Hilbert's finitistic methods are encompassed within Primitive Recursive Arithmetic, suggesting an upper limit of the Sudan ordinal $\omega^\omega$. However, both Gentzen and Bernays provided several arguments for why the process of transfinite induction on nominalistic ordinals should be seen as finitary.

It should be noted that Ackermann does not mention the terms "finitary" or "finitist" in his consistency proof. In strong contrast to Gentzen's proof, Ackermann's paper is mostly formal in nature. From his letters to Bernays, we can infer that he believed that adding upper bounds for individual reductions, despite the relative complexity of the process, does make the proof more satisfying from the point of view of Hilbertian proof theory. Bernays' motive, on the other hand, was to tie the result more tightly to the Hilbert Programme by employing the method of $\varepsilon$-calculus instead of Gentzen's sequent systems. For some reason or another, Ackermann chose not to mention any of this in his paper.

Defining what "finitary" means, even in the context of the pre-Gödelian Hilbert Programme, is not entirely straightforward, and it is even more difficult to pin down the post-Gödelian conception of finitism. In the 1930s, it became clear that the notion of finitism should either be extended or buried as useless for the purposes of the Hilbert Programme. Naturally, Hilbert and Bernays did not want to accept the latter alternative, but whereas they call for "a sharpening of the finitary standpoint" in the first volume of the *Grundlagen* [23, preface], neither of the two volumes specifies how this should be understood.

It is well beyond the scope of this paper to go into the details of how historical finitism should be interpreted. What will be examined here is the case of transfinite induction and its justification. The main purpose of this section is to show that Gentzen was concerned with the finitary justification of his proof and that he was, moreover, not driven by desperation to prove the consistency of arithmetic by any means possible, as long as those means could be made to look remotely constructive. This is not to say that Gentzen's conception of finitism was entirely in line with how Hilbert and Bernays used the word in the 1920s, although as we have seen, there is some continuity from the classical Hilbertian proof theory to Gentzen's consistency proof. In fact, Gentzen had his own conception of finitism, which is reflected by his remarks of finitism and transfinite induction in the 1936 proof and three short lectures he gave on the topic of infinity in 1936-1937 ([15, 17, 18][13]). Gentzen did not differentiate much between the methods used by Hilbert and those of Brouwer, and that the role of intuition was not central to his picture of constructivity. From the point of view of Bernays – whose arguments are very similar to

[13]Page numbering refers to the English translation in [30].

Gentzen's – and the Hilbert progamme, however, Gentzen's conception seems questionable.

After presenting the consistency proof, Gentzen considers the status of the principle of transfinite induction. On the question of whether all ordinals up to and including $\varepsilon_0$ are finitarily accessible, Gentzen states that

> From the way the concept of 'accessibility' was defined it follows that in proving this theorem [of transfinite induction], a 'running through' of all ordinal numbers in ascending magnitude must take place. In dealing with the numbers with the characteristic [i.e., the system number] 0, the following is to be observed: the *infinite* totality of the numbers smaller than 0.2 is transcended by one single idea: the proof *can* be extended *arbitrarily* far into this totality; *hence* it may be considered as completed for the *entire* totality. This 'potential' interpretation of the 'running through' of an infinite totality must be applied throughout the entire proof.[14] [16, 195]

Gentzen's argument seems, at first, circular: is it not the exact difficulty in passing from "for each system $n$, $n$ is accessible" to "for all $n$, system $n$ is accessible" – i.e., from the accessibility of $\omega, \omega^\omega, \omega^{\omega^\omega}, \ldots$ to the accessibility of their limit, $\varepsilon_0$? However, Gentzen is not arguing for any kind of an $\omega$-rule here.[15] Instead, he is trying to justify the treatment of his ordinals as a potential infinity similar to the infinity of natural numbers, which can be thought of as an ever-going process of adding one (see also [15, 348–349]). This is because, for him, a universally quantified statement has a finitist interpretation when the quantifier ranges over a potential infinity and the formula bound by the quantifier is finitistically meaningful.[16] This is not a rule of inference but an informal definition. Indeed, concerning the universal quantifier in the conclusion of the principle of transfinite induction, Gentzen says that it "adds nothing essentially new." [16, 196]

---

[14] Page numbering refers to the English translation in [36].

[15] The $\omega$-rule is an infinitary rule of proof:

$$\frac{\varphi(0) \quad \varphi(1) \quad \varphi(2) \quad \ldots \quad \varphi(n) \quad \ldots}{\forall x \varphi(x)}$$

where $\varphi(m)$ occurs in the premise for each numeral $m$. The complexity of $\varphi$ can be restricted in various ways, e.g., to quantifier-free formulas.

[16] E.g., [16, 163]: "[We] need not associate the idea of a *closed* infinite number of individual propositions with this $\forall$, but can, rather, *interpret* its sense '*finitistically*' as follows: 'If, starting with 1, we substitute for $x$ successive natural numbers then, however far we may progress in the formation of numbers, a true proposition results in each case'."

The lack of formalism is not sloppiness on Gentzen's part. In the same way that Hilbert and Bernays state that finitism is not a precise definition but merely a methodological guideline, Gentzen believed that there was no rigorous definition of the concept of "finitary." According to him, one can only examine the completed proof and the methods used to see that they do not contain transfinite notions [16, 194]. This conception is not quite the same as the "I know it when I see it" slogan sometimes associated with intuitionistic proofs, but it suggests that Gentzen resisted the idea that one could define a single formalised system as corresponding to finitary metamathematics.[17]

In fact, Bernays' arguments echo Gentzen's. He states in a 1938 presentation that

> What concerns us here is not so much to fix the precise limit of where induction is finitary, but rather to clarify to ourselves, from the intuitive point of view, what the legitimacy of the principle of inference consists of and why it represents an appropriate generalisation of ordinary induction.[18] [10, 149]

Bernays then asks us to consider the way in which one "runs through" (*parcourir*, a term directly corresponding to Gentzen's "Durchlaufung") the sequence of systems in Gentzen's construction of the ordinals. Both Gentzen's and Bernays' ideas could be explicated as follows. Let us return to the way in which the $\prec_n$-orders were defined by Hilbert and Bernays. It is completely unproblematic to run through the order $\prec_0$, which is simply the usual ordering of the natural numbers. Thus the order $\prec_0$ is accessible. Assuming that $\prec_n$ is accessible, consider $\prec_{n+1}$. By definition, the order of the numbers in the first column is isomorphic to the order $\prec_n$: thus $\varphi_l \prec_{n+1} \varphi_k$ iff $l \prec_n k$. Now one only needs to prove the termination of each row $m$ at the number $\varphi_m$.

Rows 0 and 1 only have one element each, so the principle is automatically justified. In the case of row 2, we note that the order of exponents corresponds to that of the natural numbers, i.e., $\varphi_2^l \prec_{n+1} \varphi_2^k$ iff $l < k$. Assume all rows up to $m$ have been shown accessible. For the case of row $m+1$, divide each element of the sequence by $\varphi_{m+1}$ as many times as possible to obtain $\omega$ copies of rows 0 to $m$. By inductive hypothesis, each row can be traced back to its initial prime, and thus we can also reach $\varphi_{m+1}$ in the beginning of row $m+1$.

---

[17]Indeed, Gödel originally doubted the formalisability of the totality of finitistic methods when he came up with his incompleteness proof (see [29, 234–236]).

[18]Il s'agit ici pour nous moins de fixer les limites exactes jusqu'où l'induction possède un caractère fini que de nous rendre compte intuitivement, en quoi consiste la légimitité du principe de raisonnement énoncé plus haut, et pourquoi il représente une généralisation appropriée de l'induction ordinaire.

Whereas the train of thought is rather simple, running through the process on larger systems is, as Gentzen states several times, "ziemlich kompliziert," rather complicated. Bernays remarks that these complications arise from the fact that the systems are nested within one another, or that we have "superposed inductions" [10, 150], which Gentzen calls "multiply-nested infinities." Defining exactly how these nested infinities can be perceived of as potential is where Gentzen runs onto difficulties. The potential interpretation is based on the idea that one can run through all the systems simultaenously, as one ordinal in some system uniquely determines a "row" in the next system. He admits that one can visualize the process of running through the first systems, but then the complexity of nestings becomes so great that it is in general "only remotely visualizable." [16, 196]

Consider the case of running through a well-order of type $\omega \times 2$, e.g., the sequence of odd and even numbers mentioned in the previous sections. One can grasp the structure of the order as a composition of two finite sequences $1, 3, 5, 7, \dots$ and $2, 4, 6, 8, \dots$. The idea generalises to the case of $\omega \times \omega = \omega^2$, where one has to run through a finite number of sequences:

$$o_1^1, o_2^1, o_3^1 \dots o_1^2, o_2^2, o_3^2 \dots o_1^3, o_2^3, o_3^3, \dots \dots o_1^n, o_2^n, o_3^n, \dots$$

where $o_i^j$ are defined by any notation system for an order of appropriate type.

It seems possible to generalise further by running through several such sequences simultaneously, here represented in two-dimensional form as a table:

$$\begin{array}{l}
o_1^{1,1}, o_2^{1,1}, o_3^{1,1} \dots o_1^{2,1}, o_2^{2,1}, o_3^{2,1} \dots o_1^{3,1}, o_2^{3,1}, o_3^{3,1}, \dots \dots o_1^{n,1}, o_2^{n,1}, o_3^{n,1}, \dots \\
o_1^{1,2}, o_2^{1,2}, o_3^{1,2} \dots o_1^{2,2}, o_2^{2,2}, o_3^{2,2} \dots o_1^{3,2}, o_2^{3,2}, o_3^{3,2}, \dots \dots o_1^{n,2}, o_2^{n,2}, o_3^{n,2}, \dots \\
o_1^{1,3}, o_2^{1,3}, o_3^{1,3} \dots o_1^{2,3}, o_2^{2,3}, o_3^{2,3} \dots o_1^{3,3}, o_2^{3,3}, o_3^{3,3}, \dots \dots o_1^{n,3}, o_2^{n,3}, o_3^{n,3}, \dots \\
\quad \vdots \\
o_1^{1,m}, o_2^{1,m}, o_3^{1,m} \dots o_1^{2,m}, o_2^{2,m}, o_3^{2,m} \dots o_1^{3,m}, o_2^{3,m}, o_3^{3,m}, \dots \dots o_1^{n,m}, o_2^{n,m}, o_3^{n,m}, \dots
\end{array}$$

To repeat the trick for an order of type greater than $\omega^3$, one needs to be slightly more imaginative. If one tries to replace every ordinal by a finite sequence, the number of sequences in a row will no longer be finite. One possibility to get around this would be to rotate the new sequences slightly to obtain a three-dimensional table. By generalising from this, one obtains, as it were, a discrete space where each point represents a sequence of type $\omega^2$. The crucial feature is that from any ordinal in any single sequence, one can access the first ordinal without hitting a limit point, and that from any point one can reach the origin in a finite number of steps.

This or a similar thought experiment could justify the intuitiveness of the transition from $\omega$ to $\omega^n$ for a natural number $n$. However, as the well-orders grow larger, one can no longer use the same argument for "grasping" an ordinal. What Gentzen suggests by his remark that "nothing new is *basically* added" [16, 196] in moving to transfinite exponentiation is that once one grasps the process of his construction, one can understand the whole construction.

The difference between grasping an object and grasping a process is exactly the difference between concrete and abstract intuition, as Gödel calls them in the 1958 *Dialectica* article [20]. Gödel himself does not explicitly give a limit to concrete intuition of ordinal numbers. In his analysis of Gödel's views on finitism, Tait, too, suggests that the natural limit of graspability of ordinals is $\omega^\omega$ due to the infinite iterations involved in reaching $\omega^\omega$ [37, 105]. Considering Hilbert's and Bernays' descriptions of intuition and evidence, this interpretation is highly plausible.

A concrete example of finitary mathematical reasoning can be found in "Die Philosophie der Mathematik und die Hilbertsche Beweistheorie" of 1930, where Bernays describes the finitist interpretation of exponentiation, which is essentially based on replacing of numbers with finite, graspable sequences [6, 347]. Multiplication $m \times n$ of two numbers $m, n$ (represented here in "stroke notation" as a string of $m, n$ copies of 1's) is to be understood as replacement of each 1 in the representation of $m$ by the number-figure $n$. The generalisation to $m^n$ happens by replacing again each 1 by the operation of multiplication to obtain $m \times m \times \ldots \times m$ with $n$ copies of $m$. Given that $m \times m$ results in a finite, graspable figure, then so does $(m \times m) \times m$, and so on. This level of abstraction only involves finite composition of finite sequences in a similar manner to the construction of finite exponentiation of ordinals. The process, although it cannot necessarily be carried through in actual reality, is based on the grasping of finite sequences and their finite repetitions [6, 348]. These are objects of concrete intuition: not only the construction process but also the construction itself must be visualisable in the sense explained above.

Nevertheless, with respect to transfinite induction, Bernays adopted a position similar to Gentzen's: in the 1938 paper he argues that the conclusion of transfinite induction is justified by "the transition from a progressive process to its metamathematical interpretation."[19] [10, 150] This suggests that he was willing to go beyond Hilbert's original conception of intuition – that is, if Hilbert's conception of intuition was concrete and not abstract in nature. Indeed, in the 1934 lecture, published in French with

[19]Passage d'un processus progressif à sa conception métamathématique.

the title “Quelques points essentielles de la métamathématique” [8, 90–91], Bernays says that the principle of transfinite induction, which could be used in a proof of consistency, is not provable in classical arithmetic, but is intuitionistically valid.

Between 1934 and 1936, the intuitionistically valid principle is thus relabeled as finitistically acceptable. The process-intuition that Gentzen describes in his article bears some resemblance to Brouwer's conception of intuition, which, even though difficult to exactly characterise, allows for a greater degree of abstraction as opposed to Hilbert's object-intuition. If constructive methods in mathematics are seen as a spectrum of more or less strict views, it is not surprising that the way to extend finitism is to borrow from the intuitionists. However, the question that is left open is how much such a loan will cost the Hilbert Programme.

**§6. Conclusion.** The conception of nominalistic ordinals was invented by Bernays and Hilbert to provide a way to treat complex forms of infinities without set theory. In this sense, the use of arithmetised ordinals in Gentzen's proof was nothing new, and even transfinite induction had already been used by Ackermann [2]. Moreover, both Hilbert's attempted proof of the Continuum Hypothesis – which relied on higher-type functionals – and the Ackermann proof involved methods that went beyond PRA. As Zach [40, 41] has noted, even when Hilbert came to know that the Ackermann function was not primitive recursive, he still accepted both proofs as finitary in nature.

It is not entirely clear whether Hilbert or Bernays were aware of the actual strength of the methods they used. Whereas Bernays correctly conjectured that one can build well-orders – albeit only up to the first non-recursive ordinal – by primitive recursive means only, neither of the two took into account that the crucial step of measuring such orders would not go through so easily. This was, Bernays admits in 1938, a special case of the general problem in the early Hilbert Programme:

> The hope that the finitary viewpoint (in its original sense) would suffice for all of proof theory arose from the fact that the problems of proof theory can already be formulated from this point of view. But there is no simple general relationship between the possibility to express and to prove sentences and, therefore, neither between the formulation and the solvability of a problem.[20] [10, 151]

[20]L'espoir que le point de vue finitiste (dans son sens originel) pourrait suffir pour tout la théorie de la démostration, fut suscité par le fait que les problèmes relatifs à cette théorie peuvent être énoncés déjà de ce point de vue. Mais il n'ya pas de relation générale simple entre la possibilité d'énoncer et celle de démontrer une proposition, et par conséquent non plus entre la formulation et la résolution d'un problème.

Thus the problem with well-foundedness and intuition: the construction of the ordinals might itself be entirely finitary, but the operation of deconstructing them in a proper way would get much more complicated.

Gentzen, on the other hand, needed not be as worried as Hilbert and Bernays. Whereas he takes great care to explain how his method is "finitist" or "finitary", his idea of finitist was quite loose. In fact, in the three lectures mentioned above, Gentzen mostly replaces the word "finitary" with "constructive" when talking about his proof. The main division is between two opposing views: the constructive view and the in-itself view (*die an-sich-Auffassung*), a term that is also sometimes used in his notes (see, e.g., the **INH** series in [39]). For Gentzen, the central defining characteristic of the constructive view is the conception of infinity as potential, i.e., an incomplete construction that can always be continued further [15, 346]. In contrast, the in-itself view of mathematics assumes the existence of actual, completed infinities.

Gentzen considers the main distinction between Hilbert's and Brouwer's views to be that "the intuitionists declare all propositions depending on the in-itself conception of the infinite to be *meaningless*, their modes of inference to be an empty game of symbols without any significance." [18, 363] The goal of Hilbert's is, on the other hand, to justify the use of the whole apparatus of classical mathematics by proving its consistency. Gentzen himself sides with Hilbert's more reasonable view.

Gentzen does acknowledge that some intuitionistic concepts, such as that of a real number, are ideal elements from Hilbert's point of view. Nevertheless, he adds, drawing exact boundaries is not very important, as for the sole purpose of consistency proofs, "one applies [the concepts] only in such a way that one always remains aware of their exact constructive sense." [18, 365] This is just a restatement of what Gentzen wrote in the 1936 article: instead of attempting to find a general definition of what is finitary and what is not, one can only look at particular modes of inference and see that they are permissible. The way he defines "permissible" in this case is that no in-itself notions are used [16, 194]. Given that the avoidance of actually infinite notions was the general trait which Gentzen attributed to all constructive views, this already gives a hint that Gentzen was not very concerned of whether his views coincide with what Hilbert exactly meant by finitism.

However, one can and should ask the question, what else can be justified in the same manner as transfinite induction? What prevents us from grasping, e.g., the process of forming intuitionistic choice sequences in the same way that one grasps the process of running through transfinite ordinals? The latter procedure could be called lawful in nature, but with no way to grasp the whole succession of its instances, one has to take the law for granted. Neither Bernays nor Gentzen give any limit to the ordinals that are finitarily acceptable. By focusing on case-by-case evaluations of finitary methods, however, they in effect blur the line between the finitary methods and the intuitionistic ones.

This is, in some sense, a partial return to the old conception held by many – indeed, even by Bernays as late as in 1930 [6] – that at the level of method, there is no great difference between finitism and intuitionism. The conception was abandoned when it was discovered that Peano and Heyting Arithmetic are in fact equal in strength. Approaching such a view again would mean a radical reinterpretation of Hilbert's Programme, although the use of non-primitive recursive methods in the 1920s suggests that the position of Hilbert's finitism in the constructive spectrum was never entirely clear. With the question still left open by Gentzen and Bernays, the price of the resurrection of Hilbert's dream remains undetermined.

**Acknowledgements.** The research for this article is a part of the GODELIANA project led by Jan von Plato, to whom I remain grateful for his help and support. This project has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 787758). I am also thankful to Annika Kanckos for her many useful comments and suggestions and Gabriel Sandu for his help with the part on Gabriel Sudan.

**Unpublished sources.** Bernays' *Nachlass* is located in Wissenschaftshistorische Sammlungen, Bibliothek, ETH Zürich. The remarks of Paul Bernays are published through an arrangement with Dr Ludwig Bernays. In this article, the following sources were cited.

**Manuscripts and notes:**

**Hs 973: 97:** Mathematische Notizen und Vortragsentwürfe: "Zusammenstellung der im Sommer besprochenen Punkte zur Behandlung des Kontinuumproblems", 6 pages of handwritten notes on 4 loose sheets.

**Correspondence:**

**Hs 975: 100:** Bernays to Ackermann (draft), 27.11.1936.
**Hs 975: 101:** Ackermann to Bernays, 5.12.1936.
**Hs 975: 114:** Ackermann to Bernays, 29.6.1938.
**Hs 975: 119:** Ackermann to Bernays, 10.6.1938.

## REFERENCES

[1] Hans Richard Ackermann, *Aus dem Briefwechsel Wilhelm Ackermanns*, ***History and Philosophy of Logic***, vol. 4 (1983), no. 1–2, pp. 181–202.

[2] Wilhelm Ackermann, *Begründung des "tertium non datur" mittels der Hilbertschen Theorie der Widerspruchsfreiheit*, ***Mathematische Annalen***, vol. 93 (1925), pp. 1–36.

[3] Wilhelm Ackermann, *Zum Hilbertschen Aufbau der reellen Zahlen*, ***Mathematische Annalen***, vol. 99 (1928), no. 1, pp. 118–133.

[4] Wilhelm Ackermann, *Zur Widerspruchsfreiheit der Zahlentheorie*, ***Mathematische Annalen***, vol. 117 (1940), pp. 162–194.

[5] Luca Bellotti, *Decoding Gentzen's notation*, ***History and Philosophy of Logic***, vol. 39 (2018), no. 3, pp. 270–288.

[6] Paul Bernays, *Die Philosophie der Mathematik und die Hilbertsche Beweistheorie*, ***Blätter für Deutsche Philosophie***, vol. 4 (1930), pp. 326–367.

[7] ———, *Hilberts Untersuchungen über die Grundlagen der Arithmetik*, ***David hilbert – gesammelte abhandlungen***, Springer-Verlag, 1935, pp. 196–217.

[8] ———, *Quelques points essentielles de la métamathématique*, ***L'Enseignement Mathématique***, vol. 34 (1935), pp. 70–94.

[9] ———, *Review of die Widerspruchsfreiheit der reinen Zahlentheorie by Gerhard Gentzen*, ***The Journal of Symbolic Logic***, vol. 1 (1936), no. 2, p. 75.

[10] ———, *Sur les questions méthodologiques actuelles de la théorie hilbertienne de la démonstration*, ***Les entretiens de Zürich sur le fondements et la méthode des sciences mathématiques, 6-9 décembre 1938*** (Ferdinand Gonseth, editor), Leemann & Co, 1941, pp. 144–152.

[11] Cristian Calude, Solomon Marcus, and Ionel Tevy, *The first example of a recursive function which is not primitive recursive*, ***Historia Mathematica***, vol. 6 (1979), pp. 380–384.

[12] Alonzo Church, *The constructive second number class*, ***Bulletin of the American Mathematical Society***, vol. 44 (1938), pp. 224–232.

[13] William Ewald and Wilfried Sieg (Eds.), ***David Hilbert's lectures on the foundations of arithmetic and logic 1917–1933***, Springer-Verlag, 2013.

[14] Abraham Fraenkel, *Review of Gabriel Sudan: Sur le nombre transfini $\omega^\omega$*, ***Jahrbuch über die Fortschritte der Mathematik***, vol. 53 (1927), p. 171.

[15] Gerhard Gentzen, *Der Unendlichkeitsbegriff in der Mathematik*, ***Semester-Berichte Münster***, vol. W/S 1936/37 (1936), pp. 65–80.

[16] ———, *Die Widerspruchsfreiheit der reinen Zahlentheorie*, ***Mathematische Annalen***, vol. 112 (1936), pp. 493–565.

[17] ———, *Unendlichkeitsbegriff und Widerspruchsfreiheit der Mathematik*, ***Actualites scientifiques et industrielles***, vol. 535 (1937), pp. 201–205.

[18] ———, *Die gegenwärtige Lage in der mathematischen Grundlagenforschung*, ***Deutsche Mathematik***, vol. 3 (1938), pp. 255–268.

[19] ———, *Beweisbarkeit und Unbeweisbarkeit von Anfangsfällen der transfiniten Induktion in der reinen Zahlentheorie*, ***Mathematische Annalen***, vol. 119 (1943), pp. 140–161.

[20] Kurt Gödel, *Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes*, ***Dialectica***, vol. 12 (1958), pp. 280–287.

[21] David Hilbert, *Über das Unendliche*, ***Mathematische Annalen***, vol. 95 (1926), pp. 161–190.

[22] ———, *Die grundlagen der mathematik*, ***Abhandlungen aus dem Mathematischen Seminar der Universität Hamburg***, vol. 6 (1928), no. 1, pp. 65–85.

[23] David Hilbert and Paul Bernays, ***Grundlagen der Mathematik. I***, Die Grundlehren der mathematischen Wissenschaften, vol. 40, Springer-Verlag, 1934.

[24] ———, ***Grundlagen der Mathematik. II***, Die Grundlehren der mathematischen Wissenschaften, vol. 50, Springer-Verlag, 1939.

[25] Anna Horská, ***Where is the Gödel-point hiding: Gentzen's consistency proof of 1936 and his representation of constructive ordinals***, Springer International Publishing, 2014.

[26] S.C. Kleene, *On notation for ordinal numbers*, ***The Journal of Symbolic Logic***, vol. 3 (1938), pp. 150–155.

[27] S.C. Kleene, *On the forms of the predicates in the theory of constructive ordinals (second paper)*, ***American Journal of Mathematics***, vol. 77 (1955), no. 3, pp. 405–428.

[28] Georg Kreisel, *Gödel's excursions into intuitionistic logic*, ***Gödel remembered***, Bibliopolis, 1987, pp. 65–186.

[29] Paolo Mancosu, ***The adventure of reason: Interplay between philosophy of mathematics and mathematical logic, 1900-1940***, Oxford University Press, 2010.

[30] Eckhart Menzler-Trott, ***Logic's lost genius***, American Mathematical Society, 2007.

[31] Gregory H. Moore, *Hilbert on the infinite: The role of set theory in the evolution of Hilbert's thought*, ***Historia Mathematica***, vol. 29 (2002), no. 1, pp. 40–64.

[32] Rózsa Péter, *Über den Zusammenhang der verschiedenen Begriffe der rekursiven Funktion*, ***Mathematische Annalen***, vol. 110 (1935), no. 1, pp. 612–632.

[33] ———, ***Rekursive Funktionen***, Akadémiai Kiadó, 1951.

[34] Gabriel Sudan, *Über die geordneten Mengen*, ***Buletinul de ştiinţe matematice pure şi aplicate***, vol. 28 (1925), pp. 3–23.

[35] ———, *Sur le nombre transfini* $\omega^{\omega}$, ***Bulletin Mathematique de la Société Roumaine des Sciences***, vol. 30 (1927), pp. 11–30.

[36] M.E. Szabo (Ed.), ***Collected papers of Gerhard Gentzen***, Studies in logic and the foundations of mathematics, North-Holland, 1969.

[37] W. W. Tait, Gödel on intuition and on Hilbert's finitism, Lecture Notes in Logic, pp. 88–108, Lecture Notes in Logic, Cambridge University Press, 2010, pp. 88–108.

[38] J. von Neumann, *Zur Hilbertschen Beweistheorie*, ***Mathematische Zeitschrift***, vol. 26 (1927), pp. 1–46.

[39] Jan von Plato, ***Saved from the cellar: Gerhard Gentzen's shorthand notes on logic and foundations of mathematics***, Springer, 2017.

[40] Richard Zach, *Numbers and functions in Hilbert's finitism*, ***Taiwanese Journal for Philosophy and History of Science***, vol. 10 (1998), pp. 33–60.

[41] ———, *The practice of finitism: Epsilon calculus and consistency proofs in Hilbert's Program*, ***Synthese***, vol. 137 (2003), no. 1, pp. 211–259.

*E-mail*: maria.hameen-anttila@helsinki.fi

UNIVERSITY OF HELSINKI
DEPARTMENT OF PHILOSOPHY, HISTORY AND ART STUDIES
PL 24
00014 HELSINKI, FINLAND