On RC 102-43-14

Bernd Buldt

April 23, 2003

Technical Report No. CMU-PHIL-141

Philosophy

Methodology

Logic

Carnegie Mellon

Pittsburgh, Pennsylvania 15213

On RC 102-43-14

Bernd Buldt

Introduction

When Carnap's star shone brighter on the philosophical heavens than it does today, he was known for many achievements. But even then his work in infinitary logic was not regarded as one of his outstanding merits. On the contrary, his work in this area has been more or less completely neglected — unduly neglected, I would add, since here he was more advanced than his most advanced contemporaries, and his general philosophical attitudes probably showed most clearly.[1] The present paper, though, focuses not on general philosophical attitudes but on just one piece of documentary evidence: a note to be found among the Carnap Papers, which is probably the earliest document showing his interest and involvement in infinitary logic. Two reasons led me to restrict the scope of the paper. First, this peculiar note has deeply puzzled the relevant community since it first became known some ten years ago. Second, a lot of background has to be supplied to make this puzzlement go away. A separate treatment, therefore, is indicated.

The paper is organized as follows. The first section presents the background and context I think necessary for an appropriate elucidation of the note which has proven so notoriously difficult to understand. This requires that we look at the ω-rule, Hilbert, Bernays, and Gödel, in that order. In the second section, thus prepared, we find out why Carnap's note is so puzzling, and go through a sentence-by-sentence interpretation of this note, that dispels the mysteries surrounding it. A third and final section offers a short summary of the methods employed, the assumptions made, and the conclusions reached.

---

[1] This was why I had chosen to address the Jena conference on which this book is based with a survey on "Carnap's Work in Infinitary Logic" instead of a more fashionable Carnapian topic. However, due to length restrictions the present paper is not an elaboration of the survey presented at the conference (which I hope to publish elsewhere), but a closer look at one particular aspect covered in the survey.

For those consumed with curiosity about what this note says—it has the archive number "RC 102-43-14"—here it is:

> Concerning *Hilbert's new rule of inference.*
> *Me*: It seems to me that it does not yield more or less than the rule of complete induction; therefore, merely a question of expediency.
> *Gödel*: But Hilbert conceives of it differently, more broadly; the condition is meant to be the following: "If . . . is provable with any metamathematical means whatsoever," and not: "If . . . is provable with such and such means of formalized metamathematics." Therefore, *complete induction [is] to be preferred for my system.*[2]

And for those not familiar with what infinitary logics are all about, a brief orientation:

> An infinitary logic (IL) arises from ordinary first-order logic when one or more of its finitary properties are allowed to become infinite, e.g., by admitting infinitely long formulae or infinitely long or branched proof figures. The need to extend first-order logic became pressing in the late 1950's, when it was understood that this logic is unable to express most of the fundamental notions of mathematics and thus does not permit their logical analysis. Because in many cases IL do not suffer from these limitations, they are an essential tool of mathematical logic since then. [Buldt 1998, p. 769]

1 Hilbert, Bernays, Gödel, and the ω-Rule

Carnap's note is about "Hilbert's new rule of inference," the ω-rule, which caused widespread consternation when Hilbert first introduced it, among those concerned with foundational issues in logic and mathematics. This first of the following subsections briefly explains what the ω-rule is

[2] "Zu *Hilberts neuer Schlussregel.* / *Ich*: Mir scheint sie nicht mehr und nicht weniger zu leisten wie die Regel der vollständigen Induktion; daher blosse Zweckmässigkeitsfrage. / *Gödel*: Hilbert meint sie aber anders, umfassender; die Bedingung ist so gemeint: 'Wenn . . . mit beliebigen metamathematischen Mitteln beweisbar ist', nicht so: 'Wenn . . . mit den Mitteln dieser und dieser formalisierten Metamathematik beweisbar ist'. / Also [ist] für mein System *vollständige Induktion vorzuziehen.*" (RC 102-43-14; note, dated 12 July 1931; [Köhler 1991], p. 144 (= [Köhler 2002a], p. 96)) — Note: "RC" refers to the Carnap Papers housed at the University of Pittsburgh Library, with its microfilmed twin at the *Philosophisches Archiv*, University of Konstanz. I am grateful to Dr. Uhlemann, the curator of the *Philosophisches Archiv*, for her constant helpfulness in all matters concerning "her' archive's collections. "BP" refers to the Bernays Papers, housed at the *Wissenschaftshistorische Sammlung* in the library of the *Eidgenössisch-Technische Hochschule* (ETH), Zurich; "GP" refers to the Gödel Papers, housed at the Firestone Library of Princeton University. I thank all these institutions for their permission to quote. All translations are mine, as are the suggested emendations or interpolations in square brackets. Underlining and other indications of emphasis

all about, while the following three subsections discuss the views of, respectively, Hilbert, Bernays and Gödel about this inference rule. This apparent detour builds up, step by step, the background necessary for understanding Carnap's note. For this note recorded a discussion Carnap had with Gödel about the rule, and only by appraising the extent of Gödel's knowledge at the time can we hope to shed light on Carnap's note.

The ω-Rule

The ω-rule is an infinitary rule of inference that has been employed within mathematical logic in a number of different forms, the particular form of its use depending on whether the context is recursion theory, proof theory, or model theory. So strictly speaking we should refer not to "the" ω-rule, but to a whole family of them. Tarski was the first to consider such a rule, in 1926, but it was Hilbert who, hitting on it independently in 1930, put it into the limelight in his last two publications. In its simplest form it reads, for all expressions φ with one free variable:

$$(\omega\text{-rule}) \qquad \forall n \in \mathrm{N}\ [\ \vdash \varphi(\overline{\mathrm{n}})\ ] \ \Rightarrow\ \vdash \forall x \varphi(x).$$

A corollary to Gödel's first incompleteness theorem shows all consistent formal systems of arithmetic to suffer from ω-incompleteness, i.e., there is an expression ψ (different for different formal systems) with one free variable such that:

$$(\omega\text{-incompleteness}) \quad \forall n \in \mathrm{N}\ [\ \vdash \psi(\overline{\mathrm{n}})\ ] \ \ \&\ \ \text{not} \vdash \forall x \psi(x).$$

This is why the ω-rule can be conceived of, though it need not be, as a 'natural' antidote to Gödelian or ω-incompleteness. For this rule obviously removes exactly the kind of incompleteness Gödel's first theorem identified.

---

in the originals are uniformly rendered as italics. I extend thanks to Steve Awodey and André Carus who were so kind as to improve my English.

According to the documentary evidence known to me, Carnap learned about the ω-rule during the summer of 1931. Working at this time on what was to become his *Logical Syntax* and having been one of the first to learn about Gödel's first incompleteness theorem, the ω-rule suggested itself as a means of safeguarding his logicist account of mathematics from the threat of Gödelian incompleteness. In fact, due to his subsequent employment of the ω-rule, it became even known for some time as "Carnap's rule."

Carnap became acquainted with the ω-rule through the publications of Hilbert, the then preeminent, though also controversial, figure in the field of logic and the foundations of mathematics, whose axiomatic and metamathematical research programs influenced Carnap considerably.

> We had a good deal of sympathy with the formalist method of Hilbert . . . and learned much from this school. [Carnap 1963, p. 48]

Confronted with Hilbert's version of the ω-rule, Carnap asked Gödel to comment on it and found him well-prepared, for earlier the same year Gödel had had an exchange of letters on Hilbert's new move with Hilbert's collaborator Bernays.

Hilbert

In the early 1930s the aging Hilbert was still the center of what was then the world's leading mathematics department, and his program for securing the foundations of mathematics by metamathematical (proof-theoretical) investigations had made him an authority in foundational issues. Because his program was partly designed to silence his critics by outdoing them in their constructivism, his stress on finitary considerations and finitary methods of investigation into the foundations of logic and mathematics was well-known.[3]

So it came as a surprise to the relevant community when [Hilbert 1931a] introduced a new "finitary rule of inference" which was nothing but a version of the ω-rule:

> If it has been proved, that, every time $\mathfrak{z}$ is a given numeral, the formula $\mathfrak{A}(\mathfrak{z})$ becomes a correct numerical formula, then $(x)\mathfrak{A}(x)$ may be used as a first formula [in a derivation, i.e., as an axiom].[4]

Let "ω_H-rule" be short for "ω-rule according to Hilbert" (in order to distinguish it from other versions of the ω-rule to be considered); then we can restate it more formally as:

$$(\omega_H\text{-rule}) \qquad \forall n \in \mathrm{N}\ [\ \varphi(\overline{\mathsf{n}}) \text{ is numerically correct }] \ \Rightarrow\ \vdash \forall x \varphi(x).$$

Now, what is important about this rule was already shown by Hilbert in his first paper [1931a], namely:

1. The ω_H-rule can consistently be added to a formal system of arithmetic, like $\mathcal{PA}$, the first-order system of Peano-Arithmetic.[5]
2. The ω_H-rule renders the resulting semi-formal system, $\mathcal{PA}^\omega$, Π_1-complete in the sense of Hilbert (abbreviated as "H-complete for Π_1"); in short:

---

[3] Secondary literature on Hilbert's program is rich and diverse; for the relevant aspects of his finitism *see* [Buldt 2002], pp. 402–415, and the literature cited there.

[4] "Falls nachgewiesen ist, daß die Formel $\mathfrak{A}(\mathfrak{z})$ allemal, wenn $\mathfrak{z}$ eine vorgelegte Ziffer ist, eine richtige numerische Formel wird, so darf die Formel $(x)\mathfrak{A}(x)$ als Ausgangsformel angesetzt werden." ([Hilbert 1931a], p. 491 (= [Hilbert 1935], p. 194)) The paper is based on a lecture Hilbert gave in Hamburg, December 1930, and was received by the journal 21 December 1930. Hilbert proposed the ω-rule (in a slightly different formulation) also in [Hilbert 1931b], p. 121; but this latter paper was read on 17 July 1931, 5 days after Carnap's meeting with Gödel on July 12 (but *see* footnote 21). This strongly suggests that Carnap's discussion with Gödel was triggered by [Hilbert 1931a].

[5] *See* [Hilbert 1931a], p. 491 (= [Hilbert 1935], pp. 194 *seq.*). Hilbert (and Bernays) usually worked with a formal system called "$\mathfrak{Z}$" ([Hilbert/Bernays 1934], p. 380 (= § 7.d.4); but since $\mathfrak{Z}$ is, modulo one equality axiom, the same as the nowadays much more common formalism $\mathcal{PA}$ (and since all results carry over), I use $\mathcal{PA}$ outside of quotations.

$$(\text{H-Com}_{\Pi_1}) \qquad \forall\varphi\in\Pi_1 \,[\text{ consistent } (\mathcal{PA}^{\omega} \cup \{\varphi\}) \;\Rightarrow\; \vdash_{\mathcal{PA}^{\omega}} \varphi \,].$$ [6]

An immediate consequence is the syntactical Π_1-completeness of $\mathcal{PA}^{\omega}$ (assuming the consistency of $\mathcal{PA}^{\omega}$); in short:

$$(\text{SynCom}_{\Pi_1}) \qquad \forall\varphi\in\Pi_1 \,[\, \vdash_{\mathcal{PA}^{\omega}} \varphi \;\text{ or }\; \vdash_{\mathcal{PA}^{\omega}} \neg\varphi \,].$$

This completeness of the semi-formal system $\mathcal{PA}^{\omega}$ would have been enough to escape the original formulation of Gödel's incompleteness result.[7]

Any application of the ω_H-rule requires, for all $n\in$N, a numerical evaluation of $\varphi(\overline{\mathsf{n}})$; that is why the ω_H-rule (like most versions of the ω-rule) is considered an infinitary rule. But in his follow-up paper Hilbert emphasized again that the ω_H-rule is a finitary rule of inference.

> Finally the important and for our investigation crucial fact should be stressed that all axioms and inference schemes I called transfinite [the ω_H-rule and the quantifier rules] do nevertheless have a strictly finitary character: the instructions contained therein are performable within what is finite.[8]

We will turn to this startling claim in the following section.

---

[6] *See* [Hilbert 1931a], p. 492 (= [Hilbert 1935], p. 195). There are (annoyingly) many different notions of completeness; for a survey of their definition and history, *see* [Buldt 2001].

[7] Contemporary readers may wonder why Hilbert rested content at proving Π_1-completeness and did not immediately show completeness in respect to $\mathcal{TA}$ (True Arithmetic, the set of all sentences of first-order arithmetic true in the standard model of arithmetic). Though this proof is a straightforward induction on the number of quantifiers once one assumes closure under the ω-rule, it requires, if not the arithmetical hierarchy, then at least the prenex normal form for arithmetical sentences; but the latter was established only by [Kuratowski/Tarski 1931], while the first dates back to [Kleene 1943] and [Mostowski 1947] respectively.

[8] "Endlich werde noch die wichtige und für unsere Untersuchung entscheidene Tatsache hervorgehoben, die darin besteht, daß die sämtlichen Axiome und Schlußschemata […], die ich transfinit genannt habe, doch ihrerseits streng finiten Charakter haben: die in ihnen enthaltenen Vorschriften sind im Endlichen ausführbar." ([Hilbert 1931b], p. 121)

Bernays

All this, and more, was known to Gödel when Carnap started asking him about the ω_H-rule during the summer of 1931. For already in January 1931 Gödel was informed of the ω_H-rule and discussed it in an exchange of letters with Bernays, then Hilbert's most important collaborator in foundational issues at Göttingen.[9]

Bernays communicated the ω_H-rule and the accompanying completeness result in his second letter to Gödel, dated 18 January 1931.[10] Besides giving the ω_H-rule a slightly more general form, he more importantly shed light on how the numerical correctness check for the $\varphi(\overline{\mathrm{n}})$'s in the antecedent of the ω_H-rule, and the consistency of $\mathcal{PA}\cup\{\varphi\}$ in H-Com$_{\Pi_1}$, are to be determined according to Hilbert. Bernays wrote:

> If $\mathfrak{A}(x_1,x_2,...,x_n)$ is (according to your terminology) a *recursive* formula, of which it can be shown, by finitary means, that for arbitrarily given number values $x_1=\mathfrak{z}_1$, $x_2=\mathfrak{z}_2$ ... $x_n=\mathfrak{z}_n$ it results in a numerical identity, then the formula $(x_1)(x_2)...(x_n)\mathfrak{A}(x_1...x_n)$ may be used as a first formula (i.e., as an axiom).
> Now Hilbert proves by a simple argument that each formula $(x_1)(x_2)...(x_n)\mathfrak{A}(x_1...x_n)$, where $\mathfrak{A}(x_1...x_n)$ is a recursive formula shown (by a finitary consideration) to be *consistent* with the usual system of number theory, is *provable* in the system extended by the new rule.[11]

Thus, the conditions required for employing the ω_H-rule or for accepting H-Com$_{\Pi_1}$ are finitary demonstrations of correctness and consistency. While these conditions were not really made

---

[9] It is an open secret that the lion's share of the foundational work was done by Bernays; *see* [Zach 1999; 2001] for an attempt to do justice to Bernays' contributions to Hilbert's Program.

[10] The correspondence between Bernays and Gödel relevant here is partly reproduced in [Buldt *et al.* 2002b], pp. 139–146, and is reproduced in its entirety in [Gödel 2003], pp. 78–313.

[11] "Die Hilbertsche Erweiterung besteht nun in folgender Regel: Wenn $\mathfrak{A}(x_1,x_2,..., x_n)$ eine (nach ihrer Bezeichnung) *rekursive* Formel ist, von der sich finit zeigen lässt, dass sie für beliebig gegebene Zahlwerte $x_1=\mathfrak{z}_1$, $x_2=\mathfrak{z}_2$ ... $x_n=\mathfrak{z}_n$ eine numerische Identität ergibt, so darf die Formel $(x_1)(x_2)...(x_n)\mathfrak{A}(x_1...x_n)$ als Ausgangsformel (d. h. als Axiom) benutzt werden. / Hilbert zeigt nun durch eine einfache Überlegung, dass jede Formel $(x_1)(x_2)$ ... $(x_n)\mathfrak{A}(x_1...x_n)$, bei welcher $\mathfrak{A}(x_1...x_n)$ eine rekursive Formel ist und welche (durch eine finite Überlegung) als *widerspruchsfrei* mit dem gewöhnlichen System der Zahlentheorie [...] erwiesen ist, in dem durch die neue Regel erweiterten System [...]

explicit in [Hilbert 1931a, b], once spelled out, they help to explain Hilbert's startling claim that the ω_H-rule is a finitary rule. For at that time Hilbert and Bernays were still convinced that, first, Ackermann had established the consistency of first-order arithmetic, and, second, that he had accomplished this by purely finitary means. Consequently, Ackermann's consistency proof (which was built around a numerical evaluation procedure, the so-called ε-elimination procedure) was taken to furnish the ω_H-rule and H-Com$_{\Pi 1}$ with what was needed, namely a finitary correctness and consistency check. (In the light of Gödel's second incompleteness theorem, however, it was slowly realized that this consistency proof was defective and thus it was never published.)[12] That this is not historical speculation, but how things were seen at Göttingen early in 1931, is evidenced by Bernays, who wrote in the same letter:

> The consistency of the new rule follows from the method of Ackermann (or von Neumann) for demonstrating the consistency of $\mathfrak{Z}$.[13]

Hilbert could therefore emphasize exactly this, namely,

> the important fact that the ω_H-rule does have a strictly finitary character: the instructions contained therein are performable within what is finite.[14]

It was as late as May 1931 that Bernays wrote to Gödel, that and where they had erred in this respect:

---

beweisbar ist." (GP 010015.45, pp. 4–5; [Buldt *et al.* 2002b], p. 139; [Gödel 2003], pp. 82 *seq.*) – "Recursive in Gödel's sense" is now known as "primitive recursive."

[12] *See* [Hilbert/Bernays 1939], §§ 1–3, for details on the ε-calculus, Zach [2002] for a recent assessment of Ackermann's original work, and [Ackermann 1940] as well as [Hilbert/Bernays 1939], suppl. V.B, for Ackermann's rectified proof, using Gentzen's method of transfinite induction.

[13] "Die Widerspruchsfreiheit der neuen Regel folgt aus der Methode des Ackermannschen (oder auch des v. Neumannschen) Nachweises für die Widerspruchsfreiheit von $\mathfrak{Z}$." (GP 010015.45, p. 5; [Buldt *et al.* 2002b], p. 140; [Gödel 2003], pp. 84 *seq.*) – While the letter makes a claim only as to the consistency of the ω_H-rule when added to $\mathfrak{Z}$, I suggest that Hilbert thought Ackermann's work using ε-elimination would also guarantee the finitary character of the ω_H-rule. This is admittedly a novel view and might not find the enthusiastic approval of all Hilbert scholars. Hence, I would like to stress that my interpretation of Carnap's note does not hinge on this reading of Hilbert.

[14] *See* footnote 8 for the exact wording.

> Also concerning Ackermann's proof for the consistency of number theory, I believe I am getting things straightened out now. It seems to me that clearing up the facts consists in the following: Recursions of the type [...] are, in general, *not expressible within* the system $\mathfrak{Z}$.[15]

But Bernays' earlier letter contained more. It is interesting to see, e.g., that he bothered to prove, using Gödel's first incompleteness theorem, that adding the ω_H-rule results in a non-conservative extension. We can learn from it—and I consider this as highly important, for it warns us not to read, anachronistically, modern knowledge into the historical sources—that before Gödel's first incompleteness theorem became known, Hilbert and Bernays were not sure about the actual deductive strength of the ω_H-rule. That is to say, it is by no means obvious, whether adding the ω_H-rule was intended to attain a deductively more powerful formalism, or whether it was to provide a mere point of attack for proof-theoretical investigations.[16]

After the discussion of various such ramifications, Bernays added that it would be desirable to have one inference rule instead of two (the ω_H-rule and the induction axiom) and which would do

---

[15] "Auch betreffs des Ackermannschen Beweises für die Widerspruchsfreiheit der Zahlentheorie glaube ich jetzt ins Klare zu kommen. Es scheint mir die Aufklärung des Sachverhaltes darin zu bestehen, dass Rekursionen vom Typ [...] im allgemeinen *nicht innerhalb* des Systems $\mathfrak{Z}$ *formulierbar* sind." (GP 010015.47, pp. 1–2; [Gödel 2003], pp. 104 *seq.*) – But these observations took time to really sink in, especially on Hilbert's side. Recall that his lecture [1931b], which contains the explicit claim about the finitary character of the ω_H-rule quoted above, was delivered two months after Bernays wrote this letter. Three observations may help to explain this discrepancy between Bernays' letter and Hilbert's lecture. First, according to my understanding of the Hilbert-Bernays relationship, this happened often: Bernays was ahead of Hilbert in accommodating new facts, with Hilbert lagging stubbornly behind (*see* in this connection [Reid 1970], p. 172, describing Hilbert as "slow to understand"). This might very well have increased through the 1930s, when Hilbert began to show visible signs of aging (*see*, e.g., the anecdote reported *ibid.*, pp. 202 *seq.*, and the whole ch. 24, *passim*). Second, we learn in a letter from Bernays to Heinrich Scholz, dated 1 December 1941 (preserved among the Scholz Papers, housed at the *Institut für mathematische Logik*, University of Münster), that he, Bernays, had not been engaged in polishing [Hilbert 1930] and *see*ing it through the press. (Scholz was puzzled about the surprisingly strong anti-Kantian undertones in [Hilbert 1930], which Bernays explained by his non-participation.) That this was true also for [Hilbert 1931b] is made credible by the fact that this last paper from Hilbert's pen featured another terminology than that employed in the papers from 1918–1930, the time of the active collaboration with Bernays—in fact, [Hilbert 1931b] continues Hilbert's old terminology as employed in [Hilbert 1904]. Third, we know that the relationship between Bernays and Hilbert saw, sometimes violent, disagreement over foundational issues ([Reid 1970], p. 173). Taking all this together, I'm inclined to think that a certain alienation grew between Bernays and Hilbert, especially after Gödel's results became known: while Bernays advocated a more flexible framework for finitism (*see* [Bernays 1938]), Hilbert remained unconvinced (*see* his preface to [Hilbert/Bernays 1934]). The difference between Bernays' letter and Hilbert's lecture was then, if not a sign of the alienation that had arisen between the two, a sign of the different stance the two took while trying to cope with Gödel's results.

[16] Certain weak versions of the ω-rule do indeed result in conservative extensions and are hence of purely proof-theoretical interest; *see* [López-Escobar 1976] for an example.

the job of both of them. To this end Bernays suggested a more general ω-rule, "ω_B-rule " for short, which no longer came with any restriction on φ and, reduced to the one-variable case, reads:

$$(\omega_B\text{-rule}) \qquad \forall n{\in}\mathrm{N}\,[\,\vdash \varphi(\bar{\mathrm{n}})\,] \;\Rightarrow\; \vdash \forall x\varphi(x).$$ [17]

Although his version of the ω-rule seemed to be much stronger, he had no clue as to whether it already guaranteed closure under the new rule. Hence, he posed this as a question to Gödel.

Gödel

It took even a Gödel some time to digest the news. First of all he of course had to safeguard his major discovery, the incompleteness results, from the threat of completeness that came with the ω-rule(s). So he responded only three months later; his letter is dated 2 April 1931. Two of his insights reported in that letter are relevant for the present context.

First, formal systems of arithmetic enlarged by either the ω_H-rule or the ω_B-rule are not necessarily deductively closed; Gödel's first incompleteness theorem can be extended to cover also such semi-formal systems of arithmetic.

To start with, one can show that also the systems $\mathfrak{Z}^*, \mathfrak{Z}^{**}$ are not deductively closed.[18]

---

[17] "Ist $\mathfrak{A}(x_1 \ldots x_n)$ eine (nicht notwendig rekursive) Formel, in welcher als freie Individuen-Variablen nur $x_1, \ldots, x_n$ auftreten und welche bei der Einsetzung von irgend welchen Zahl-Werten anstelle von $x_1 \ldots, x_n$ in eine solche Formel übergeht, die aus den formalen Axiomen und den bereits abgeleiteten Formeln durch die logischen Regeln ableitbar ist, so darf die Formel $(x_1)(x_2) \ldots (x_n)\mathfrak{A}(x_1 \ldots x_n)$ zum Bereich der abgeleiteten Formeln hinzugenommen werden." (GP 010015.45, p. 11; [Buldt *et al.* 2002b], p. 141; [Gödel 2003], pp. 86–89.)

[18] "Zunächst kann man zeigen, daß auch die Systeme $\mathfrak{Z}^*, \mathfrak{Z}^{**}$ nicht deduktiv abgeschlossen sind [...]." (BP Hs 975 1691a, p. 1; [Gödel 2003], pp. 92 *seq.*) – In the preceding letter, Bernays called "$\mathfrak{Z}^*$" the system $\mathfrak{Z}$ as extended by the ω_H-rule and $\mathfrak{Z}^{**}$ the system extended by the ω_B-rule. The diligent reader may be confused here. For we said above, that the ω-rule can be shown to guarantee $\mathcal{TA}$-completeness, something that *see*ms to be called into question now. The explanation for this apparent conflict is that the proof of $\mathcal{TA}$-completeness assumes closure under the ω-rule, which can be attained only after a transfinite number of its applications; *see* footnote 31 for details.

Second, Gödel filed the complaint that,

> one cannot rest assured with the systems $\mathfrak{Z}^*, \mathfrak{Z}^{**}$ as a satisfying grounding of number theory; first of all because the very complicated and problematic notion of "finitary proof" is presupposed without closer mathematical specification.[19]

Since Ackermann's consistency proof together with the accompanying machinery of ε-elimination was not yet published, no one outside of Hilbert's Göttingen was able to see, as suggested above, why Hilbert thought the application condition for the ω_H-rule—the check that $\varphi(\bar{n})$ is numerically correct for all $n \in N$—could be fulfilled by finitary means, and hence, why Hilbert could claim the finitary character of the ω_H-rule. Lacking this information, it was only natural for Gödel to challenge Hilbert instead with the much more general request to give a comprehensive definition of the notion "finitary proof." (What Gödel requested is more general, for the finitary check demanded by the application condition for the ω_H-rule can be accomplished without defining in advance what else might be finitary as well.)

Be all that as it may, what is important for the present paper is that we find Gödel well-prepared to discuss the ω_H-rule with Carnap.

2. Carnap Meets the ω-Rule

Having collected the necessary background, I will now proceed as follows. The first section describes the general situation in which Carnap's first encounter with the ω-rule took place, and then turns to what I will call the 'natural' reading of note RC 102-43-14. Its goal is to show why this interpretation, though it forces itself on the reader as 'natural,' is most unsatisfactory; hence,

---

[19] "Übrigens glaube ich, dass man sich [...] bei den Systemen $\mathfrak{Z}^*, \mathfrak{Z}^{**}$ als einer befriedigenden Begründung der Zahlentheorie nicht beruhigen kann u.[nd] zw.[ar] vor allem deswegen, weil in ihnen der sehr komplizierte und problematische Begriff 'finiter Beweis' ohne nähere mathem.[atische] Präzisierung vorausgesetzt wird (bei Angabe der Axiomenregel)." (BP Hs 975 1691a, p. 7; [Gödel 2003], pp. 96 *seq.*) – The reservation Gödel uttered about the concept of "finitary proof" was the same the intuitionists of the time were challenged with, namely, to make precise the notion of "constructive proof." Interestingly enough, Gödel attempted in the same letter to specify a general condition any finitary proof must satisfy.

this first section is entitled "Problems." The second section attempts a more satisfying interpretation of RC 102-43-14 and an accompanying entry into Carnap's diary, dated 12 August 1931. It offers a sentence-for-sentence interpretation and is accordingly divided into four subsections. Its goal is to promote an interpretation that makes sense of the complete text of Carnap's notes, and at the same time avoids the problems of the 'natural' reading. If successful, it would present Carnap not as the fool the 'natural' reading implies him to be, but, on the contrary, as a top-notch foundational researcher of his day. To be sure, this is a much more favourable outcome; this second section is thus called "Solutions."

Problems

The summer of 1931 was the time when, having abandoned the first, ill-fated project *Untersuchungen zur allgemeinen Axiomatik* (Investigations in General Axiomatics), Carnap started writing *Versuch einer Metalogik* (Attempt at a Metalogic), which would finally become his *Logical Syntax of Language*.[20] The first documentary evidence for Carnap's active interest in infinitary logic comes from this context. It is one of the loose sheets that complement entries to his diary, and it carries the date 12 July 1931.[21] Judging from its title, "Gödel Fragen" (Questions to Gödel), and the notes he took, Carnap used this meeting (out of many) he had with Gödel for an inquiry about the views Gödel held at that time on certain technical and philosophical issues. Entries to his diary and other accompanying sheets show that this was a common practice among the two, and indeed some other sheets even carry the same title "Gödel Fragen" (14 March and 9 June 1931).[22] Apparently however, the two continued their exchange beyond the questions Carnap had prepared in advance—as indicated by a horizontal line in the manuscript dividing off

[20] For the abandoned manuscript of the *Untersuchungen*, recently published as [Carnap 2000], *see* [Coffa 1991], ch. 15, [Köhler 1991/2002a], § 3, and [Awodey/Carus 2001]; concerning the *Metalogik* and its transformation into the *Logical Syntax*, *see* [Carnap 1963], pp. 53–56.

[21] Though the sheet carries the date 12 July 1931, [Köhler 1991], p. 144 (= [Köhler 2002a], p. 96), suggests that the exchange actually took place on 30 August 1931 and that the record in question was added later to a previous one on the same page. The reason Köhler adduces is an entry in Carnap's diary, dated 30 August 1931, which reads: "Metalogik gearbeitet. Nachmittags mit Feigl und Gödel im Café. Über Hilberts neue Abhandlung; sehr bedenklich." If pressed for a decision, I would, for various reasons, be more inclined to assume several (at least two) discussions on the ω_H-rule. But since it does not make a difference for the current purpose, I will not try to settle this issue here.

the first section from a second—in order to discuss [Hilbert 1931a], which had just arrived on the library shelves.

It is astonishing that Carnap's note seems not to reflect anything of what was sketched in the preceding section. It simply reads:

> Concerning *Hilbert's new rule of inference.*
> *Me*: It seems to me that it does not yield more or less than the rule of complete induction; therefore, merely a question of expediency.
> *Gödel*: But Hilbert conceives of it differently, more broadly; the condition is meant to be the following: "If ... is provable with any metamathematical means whatsoever," and not: "If ... is provable with such and such means of formalized metamathematics."
> Therefore, *complete induction [is] to be preferred for my system.*[23]

Two problems jump out: Carnap's apparent glaring misunderstanding, and Gödel's missing correction. For the first sentence ("merely a question of expediency") suggests that Carnap considered the ω_H-rule and the rule of induction to be on a par with each other, and hence, that he—and also Gödel, for there is no indication of Gödel setting things straight—apparently did not realize that the ω_H-rule is much more powerful than the rule (or the axiom) of complete induction. How could this possibly be? How could Carnap not realize, on the spot, that the ω-rule must be deductively stronger than complete induction? And how could Gödel leave Carnap with that mistake, for we know that he knew better at that time?

We are thus faced with a situation, where the 'natural' reading of the text and its wording—endorsed by everyone I have discussed it with so far—leads to a seemingly unacceptable conclusion. For this reading of Carnap's note would force us to believe that not only Carnap but in particular Gödel did not realize the difference between the two rules.

---

[22] *See* the material from the Carnap Papers included to [Köhler 1991; 2002a, b].
[23] RC 102-43-14; note, dated 12 July 1931; [Köhler 1991], p. 144 (= [Köhler 2002a], p. 96; *see* footnote 2 for the German text.

In light of what was mentioned above concerning Gödel's correspondence with Bernays, however, this 'natural' reading is out of question, and we thus find ourselves on the horns of a dilemma. For, alternatively, one could hold on to the fact that Gödel knew the difference between the two rules, but that either Gödel failed to let Carnap know as well (the one horn) or that Carnap failed to understand what Gödel might have said in this respect, and hence his notes, too, fail to reflect such knowledge (the other horn).

This last alternative cannot be dismissed off hand, since Carnap seems to have previously been rather slow in grasping the importance of Gödel's first incompleteness theorem. Gödel told Carnap about that theorem as early as 26 August 1930, but there is no indication that Carnap, in contrast to, say, von Neumann only a fortnight later, realized at that time any of its foundational consequences.[24] As unattractive as this alternative is, from what we know, the first alternative is equally unlikely. For during the time Carnap wrote his *Metalogik,* Gödel was always happy to give advice or to help out with his technical expertise (see footnote 22). Therefore, it makes absolutely no sense to assume Gödel would have withheld information from Carnap in this particular situation.

Thus, the 'natural' reading of the text leaves us gored on the one horn, Carnap was slow off the mark—that's it. I regard this a bad enough conclusion to encourage looking for a different reading of the text, thereby saving Carnap from the charge of being slow on the uptake. This will be done (and, I hope, accomplished) in the next section.

Solutions

Trying my hand at a different reading of RC 102-43-14, I will proceed in reverse order, i.e., I will start with the last sentence of this note and work my way up to the first, providing a sentence-for-sentence interpretation, finally turning to Carnap's related diary entry.

---

[24] *See* [Dawson 1985], p. 255, [Dawson 1997], pp. 68–73, and [Köhler 1991; 2002a], § 4.1–3, for a collection of the relevant material.

"Therefore, *complete induction is to be preferred for my system.*"

If we assume what all available evidence supports, namely, that Carnap referred here to what later became his *Logical Syntax* and bear in mind that Carnap made the rule of complete induction a rule of his 'Language I' but the ω-rule a rule of his 'Language II,' then part of the former bewilderment vanishes into thin air.[25] For there is further evidence, that "my system" most probably does not refer to the *Logical Syntax* as a whole, but in particular to its 'Language I.' But if this is true, then the concluding sentence simply says, that the ω-rule is not suited for a definite logic with a constructive spirit as 'Language I' was intended to be. Two observations support this reading of "my system."

First, 'Language I' was the language of choice for Carnap:

> I had a strong inclination toward a constructivist conception. In my book, *Logical Syntax*, I constructed a language, called "Language I", which fulfilled the essential requirements of constructivism. [Carnap 1963, p. 49]

Second, the larger portion of the technical discussions with Gödel during 1931 seems to relate to designing this language, i.e., it was 'Language I' with which Carnap was mostly concerned at that time. A statement typical for his notes during this time reads, e.g.:

> I want to make do *without sentence variables, predicate variables* (and variables for numerical functions).[26]

---

[25] *See* [Carnap 1934; 1937], §§ 3–14, for 'Language I,' and *ibid.*, §§ 26–34, for 'Language II,' as well as [Carnap 1935] or its later incorporation to the English translation of the *Logical Syntax*, [Carnap 1937], § 34a–i. A fact, often overlooked even in the scholarly literature, is that the ω-rule is not restricted to 'Language II' but made its first appearance already in the context of 'Language I;' *see ibid.*, § 14, condition DC2. Likewise, the ω-rule is not a proper rule of 'Language II' but one of its metatheorems; *see ibid.*, p. 120 (= Thm. 34f.10). Just to keep things simple, I will, for the moment, skim over these details and proceed as if the ω-rule belonged only to 'Language II.' The reason for doing so is that DC2, and hence the ω-rule, appeared in the context of 'Language I' only for expository purposes (a claim, that would take too long to established here).

[26] "Ich möchte *ohne Satzvariable, Prädikatsvariable* und (Zahlfunktionsvariable) auskommen." (note, entitled "Gödel Fragen," dated 9 June 1931; [Köhler 2002b], p. 112) – His final 'Language I' met this demand, allowing only for individual (number) variables, while 'Language II' contained all sorts of variables; *see* [Carnap 1934; 1937], § 4,

Thus, if we have biographical reasons to believe that "my system" refers to Carnap's 'Language I,' then we can conclude that he (and Gödel) clearly understood the difference between the ω-rule and the rule of complete induction. For then this clearly understood difference was the very reason to include the induction rule in, and to ban ω-rules from, his 'Language I.' Seen in this light, the conclusion, "therefore, complete induction for my system," makes perfect sense.

"*Gödel*: But Hilbert conceives of it differently, more broadly; the condition is meant to be the following: 'If ... is provable with any metamathematical means whatsoever,' and not: 'If ... is provable with such and such means of formalized metamathematics.' "

Seen in the light of the preceding section, Gödel's remarks fall into place as well. For what Carnap preserved for later use was Gödel's distinction between "provable with any metamathematical means whatsoever" and "provable with particular formalized metamathematical means," together with the information that Hilbert endorsed the first interpretation. But this answer of Gödel's presupposes that Carnap had asked about the distinction contained therein. It suggests that Carnap had asked Gödel about meaningful conditions for applying the ω-rule, i.e., how Hilbert's requirement for applying the ω_H-rule ("If it has been proved that, every time $\mathfrak{z}$ is a given numeral [...]") can be understood. It suggests he asked what it could possibly mean, that Hilbert characterized the ω_H-rule as a finitary rule. Carnap wanted to know because, according to the last sentence of his minutes, he was pondering whether or not to include an ω-rule to his system. To get clear about this issue, Carnap, always eager to give concepts as precise a meaning as possible, probably even suggested "provable by formalized means" as a possible explanandum for Hilbert's claim. And judging by Gödel's response—"But Hilbert conceives of it differently"—Carnap apparently favoured this reading of "finitary" as "provable by formalized means." (More on this in the following section.)

---

§ 26 respectively. Details of the construction of 'Language II,' among them the important exchange on the notion of analyticity, appear in the exchange with Gödel only in 1932 and later; *see* the material reproduced in [Köhler 1991; 2002a, b].

In his reply Gödel could rely on (part of) the information he had gathered first hand from the correspondence with Bernays. So he could make a authoritative claim—there is no sign that Gödel had wavered between several interpretations ("Gödel thinks it more probable that ...")—as to what Hilbert's stance on this question actually was. Without knowing how tightly Hilbert's claims were connected to Ackermann's consistency proof, though, the conditions for applying the ω_H-rule appeared to Gödel to be far too unspecific to be mathematically useful. This is evidenced by the above-quoted complaint he filed in his reply to Bernays. We can even assume that he did not withhold his opinion from Carnap.

But all this merely supported Carnap's inclination neither to follow Hilbert nor, consequently, to include a version of the ω-rule to his own preferred 'Language I.' Following Gödel's information that Hilbert did not see the ω_H-rule the way he had initially thought, Carnap concluded that there was no room for only vaguely specified "metamathematical means whatsoever" in his definite 'Language I', otherwise, an ω -rule would have corrupted its constructive purity. In the light of the clarification Gödel was able to provide, the following "therefore complete induction" was a fully justified conclusion on Carnap's side.

"*Me*: It seems to me that it does not yield more or less than the rule of complete induction; therefore, merely a question of expediency."

Finally, we have to turn to the first sentence, according to which it is simply "a question of expediency" whether 'to go inductive' or 'to go ω.' I will distinguish two possible scenarios; both have to (seek to) answer the question that forces itself on every reader of Carnap's note, namely: Why is there no record of Gödel correcting Carnap's apparently mistaken opening statement?

The First Scenario. This reading assumes that Carnap committed the embarrassing mistake not to have realized, on the spot, that the ω-rule is more powerful than the rule of induction. For, while induction does not decide the undecidable Gödel sentence for $\mathcal{PA}$, the ω-rule does, and is hence a stronger rule. (This is the result of comparing the ω-rule with ω-incompleteness as given in § 1.1 above, which Carnap must also have noticed.) If this were so, i.e., if Carnap did not recognize Π_1-completeness to follow from the ω_H-rule, well, then Carnap would surely have been set right by Gödel; for Gödel knew better, as we have seen above. Moreover, this being the easy lesson we just stated, we can likewise assume that it did not require record afterwards—it simply stuck. Thus, Carnap's note does not reflect the answer we expect from Gödel because the oversight that was corrected was far too trivial to require a written record for later perusal.

This reading of the text requires as an auxiliary hypothesis that Carnap considered worthy of being recorded in his minutes only what he expected to be of later use for himself, but which, if unrecorded, might get lost. This seems to me a plausible and innocuous enough hypothesis to be entertained. This scenario has, however, at least three weak spots. First, it leaves unexplained why Carnap took down at all the first sentence of his minutes. For, if it was the easy lesson that immediately stuck, as this scenario assumes, then why should Carnap have preserved his former error at all? Second, it leaves the first and second sentence completely unrelated to each other, where we may expect a thread underlying the recorded discussion; thus, this scenario does not provide a coherent meaning to the whole note. Third, it forces us to believe that Carnap was unable to add up two and two. For on the one hand Carnap had known about the Π_1-incompleteness of $\mathcal{PA}$ for approximately one year, while on the other hand, he read in Hilbert's paper a proof of the Π_1-completeness of $\mathcal{PA}^\omega$. Carnap was slow, perhaps, but certainly not blind.

The Second Scenario. The second reading avoids these weak spots and does so by considering the opening sentence in the light of Gödel's subsequent answer. It results from entertaining the hypothesis of the first scenario, about what Carnap did not think necessary to include to his notes; it supplies some of the context Carnap had no reason to include to his short and personal minutes.

Gödel's answer drew on the distinction between "provable with all metamathematical means" and "provable with particular formalized means." That is why I assumed above, first, that

Carnap requested from Gödel some clarification of what Hilbert could possibly have had in mind, when he stated that the correctness of $\varphi(\overline{\mathrm{n}})$ can finitarily be proven for all $n \in \mathrm{N}$; and second, that Carnap at first preferred the reading "provable with formalized means."

Further, we know that, by 1934—and there is no reason to assume otherwise for the summer of 1931—in order to make more precise Hilbert's notion of "provable with finitary means" Carnap's best guess was to equate it with "provable with definite means," which in turn he specified as "provable within 'Language I'." In addition, Carnap not only knew Gödel's arithmetization technique very well, but even contributed a modest further development for his own purposes.[27] Thus, taking these considerations together, Carnap's initial understanding of the ω_H-rule must have been something like:

$$(\omega_C\text{-rule}) \qquad \vdash_{\mathcal{L}1} \forall x\, [\, \mathrm{Pr}_{\mathcal{L}1}(\ulcorner \varphi(\dot{x}) \urcorner)\,] \;\Rightarrow\; \vdash_{\mathcal{L}1} \forall x \varphi(x)$$

(the index "$\mathcal{L}1$" refers to the formal system of 'Language I'). I do not suggest, of course, that, during the discussions with Gödel, Carnap wrote down the ω_C-rule exactly as given above on the coffee house table. Rather, the peculiar formulation of the ω_C-rule is intended to make explicit what Carnap's best guess could have been, while striving to make precise Hilbert's claim that the ω_H-rule is finitary.[28] What I do assume, however, is that Carnap thought of formalizing the antecedent of the ω-rule; for otherwise, as indicated above, the reference to "provable with

[27] *See* [Carnap 1937] pp. 129, 173, for equating "finitary" with "definite;" [Carnap 1934; 1937], § 15 for the characterization of 'Language I' as definite, and *ibid.*, §§ 18–24, for his knowledge of the arithmetization technique.
[28] The conclusions I draw do not depend on the exact wording of the ω_C-rule (which goes under the name "formalized" or "arithmetized ω-rule"); it suffices that it reflects the 'spirit' of Carnap's assessment of the ω_H-rule. For the arguments to follow, I do not even need to assume that he actually tried his hands at formalizing the ω_H-rule. The reason to state the ω_C-rule is solely to give the following discussion a firmer basis by providing a specific example. I freely admit, therefore, that Carnap's best attempt to formalize the ω_H-rule would, most probably, have been:

$$(\omega_{C^*}\text{-rule}) \qquad \forall n \in \mathrm{N}\, [\, \vdash_{\mathcal{L}1} \mathrm{Pr}_{\mathcal{L}1}(\ulcorner \varphi(\overline{\mathrm{n}}) \urcorner)\,] \;\Rightarrow\; \vdash_{\mathcal{L}1} \forall x \varphi(x).$$

For the trick of working with the functional expression "$\ulcorner \varphi(\dot{x}) \urcorner$" was introduced by Bernays only eight years later ("$\mathfrak{B}(\{x\})$" in his notation); *see* [Hilbert/Bernays 1939], pp. 322–326. But the ω_C-rule as given in the main text, with the universal quantification performed within the formal system, better reflects what Carnap would have aimed at; he preferred strictly formal procedures that remain completely within the formalism (*see* [Carnap 1934; 1937], § 22).

formalized means" would make no sense. (One may recall here that any unformalized version of the ω_H-rule would not have been even worth consideration—because of its infinitary character—for inclusion to his 'Language I.')

Now we are prepared to give specific meaning to Carnap's conjecture that the ω_H-rule did not "yield more or less than the rule of complete induction." In order to compare the relative strength of both rules, we need, first, a basic formal system without induction or an ω-rule; second, this basic formalism should be finitary. Both requirements are fulfilled by 'Language I' without induction. Let "IND" denote the rule of complete induction and "$\mathcal{L}$-1" $\mathcal{L}1$ without IND. Then we can restate Carnap's Conjecture ("CC" for short) that,

> It seems to me that it [i.e., the ω_H-rule made precise in the form of the ω_C-rule] does not yield more or less than the rule of complete induction,

as:

$$(CC) \qquad \text{IND} \vdash_{\mathcal{L}\text{-}1} \varphi \quad \text{if and only if} \quad \omega_C\text{-rule} \vdash_{\mathcal{L}\text{-}1} \varphi .$$

But stating CC is not the embarrassing mistake the first impression of Carnap's opening sentence seemed to be. Carnap does not call into question the Π_1-completeness as proved by Hilbert, but ponders the question how useful a formalized ('constructivized') ω-rule, like the ω_C-rule, might be for his own purposes. I want to make four remarks along this line.

First, at a time when the completeness issues that come with ω-rules had not been settled, Carnap's conjecture was a serious one: How much gain in completeness can we expect from adopting a (formalized) ω-rule? Recall that both Bernays and Gödel bothered to prove results even for non-formalized ω-rules most logicians would consider as trivial today: $\mathcal{PA} \subset \mathcal{PA}^{\omega_H}$ (Bernays) and $\mathcal{PA}^{\omega_B} \subset \mathcal{TA}$ (Gödel). Logicians were able to study formalized ω-rules—after a first step taken by [Rosser 1937]—first in the late 1950s.[29] Hence, I take it, an answer to CC was by no means obvious in 1931, and, consequently, Carnap was not the fool the 'natural' reading of RC 102-43-14 or the first scenario suggested he was.

Second, Carnap was right with his conjecture insofar as formalized ω-rules yield only a modest strengthening of the underlying formal system. To see this, recall that Carnap was explicit about the requirement that only a finite number of applications of an infinitary rule are allowed.

> We must do this [the evaluation in 'Language II'] in such a way that this process of successive reference comes to an end in a finite number of steps. [Carnap 1937, p. 106 (= [Carnap 1935], p. 173)]

Let "$\mathcal{F}_{\alpha}^{\omega_*}$", with α an infinite limit ordinal, denote a formal system of arithmetic $\mathcal{F}$, in which fewer than α applications of the ω_*-rule are allowed, with $*$ either H, B, or C. Then, according to the finiteness condition just quoted and his intention to use a formalized ω-rule, Carnap was interested only in one of the smallest and weakest of these systems, i.e., $\mathcal{L}\text{-}1_{\omega}^{\omega_c}$. Due to the lack of induction, $\mathcal{L}\text{-}1_{\omega}^{\omega_c}$ seems at most as strong as $\mathcal{L}1$. Hence one can ask:

$$\mathcal{L}\text{-}1_{\omega}^{\omega_c} = \mathcal{L}1 \ ?$$

To see this, one can argue as follows (modulo much handwaving): $\mathcal{L}1$ allows (in the limit) for at most ω many applications of the rule of complete induction; substitute each application of induction in $\mathcal{L}1$ with an application of the ω_C-rule in $\mathcal{L}\text{-}1^{\omega_c}$; then the deductive strength of $\mathcal{L}\text{-}1_{\omega}^{\omega_c}$ amounts (at the very most) to that of $\mathcal{L}1$. This way of estimating the deductive power of $\mathcal{L}\text{-}1_{\omega}^{\omega_c}$ does not presuppose anything that was not accessible to Carnap. (And using other ω-rules would not change the general picture.) In the absence of full proofs settling CC (available only much later), a rough estimation of the deductive power to be expected from $\mathcal{L}\text{-}1_{\omega}^{\omega_c}$ thus does not give

[29] [Shoenfield 1959] and [Feferman 1962] are the milestone papers in question.

cause for any hopes. The conclusion as to whether the ω_C-rule (or any other ω-rule) is to be preferred over the rule of induction—because a rough estimate gives $\mathcal{L}^{-}1^{\omega_c}_{\omega} \approx \mathcal{L}1$ (with $\mathcal{L}1 = \mathcal{L}^{-}1$ + IND!)—is, "therefore, merely a question of expediency."[30]

Third, Carnap was not the hard-core logicist he usually is portrayed as. His indebtedness to Frege (and other logicists) notwithstanding, he entertained a non-foundationalist, pragmatic attitude towards (the foundations of) mathematics, oriented toward its applicability.

> Since […] I came to philosophy from physics, [I] looked at mathematics always from the point of view of its application in empirical science. [Carnap 1963, p. 48]

Accordingly, not only do his logic books stand out by featuring practical examples of how logic can be applied to the empirical sciences, but he was even willing to settle foundational problems in terms of applicability.

> According to my principle of tolerance, I emphasized that [… if there are] methods which, though less safe because we do not have a proof of their consistency, appear to be practically indispensable for physics [… then] there seems to be no good reason for prohibiting these procedures so long as no contradictions have been found. [*ibid*, p. 49]

---

[30] Consequently, if completeness results can be expected at all for $\mathcal{L}^{-}1^{\omega_c}_{\alpha}$, then only for ordinals α much bigger than ω. To *see* this, first consider the ω_B-rule and assume that a definitional extension of $\mathcal{L}1$, denoted by "$\boldsymbol{\mathcal{L}1}$," equals $\mathcal{PA}$. Then it follows from [Rosser 1937] that $\boldsymbol{\mathcal{L}1}^{\omega_B}_{\omega+n}$ is Π_{2n}-complete, for all $n \in N_0$, and from [Goldfarb 1975], that $\boldsymbol{\mathcal{L}1}^{\omega_B}_{\omega^2}$ is $\mathcal{TA}$-complete. Taking into account that the weaker system $\mathcal{L}^{-}1^{\omega_B}_{\alpha}$ has to catch up on induction, one will arrive at completeness results only for $\alpha > \omega + n$, and $\alpha > \omega^2$ respectively. Now turn to the ω_C-rule. We know from [Feferman 1962] (and [Kreisel 1965], p. 255 (remark 2(i)), who pointed out that, instead of the reflection principle "$\forall x\,[\,\mathrm{Pr}(\ulcorner \varphi(\dot{x})\urcorner)\,] \rightarrow \forall x \varphi(x)$," employed by Feferman, the corresponding rule, i.e., the ω_C-rule, will do as well), that $\boldsymbol{\mathcal{L}1}^{\omega_C}_{\alpha}$ can be $\mathcal{TA}$-complete; but only if, in order to define the α's, a suitable path through O, the class of all recursive ordinals, is chosen. This shows how much weaker the ω_C-rule is than the ω_B-rule, and thus how unlikely completeness results are for the even weaker system $\mathcal{L}^{-}1^{\omega_c}_{\alpha}$. These results show further that, even if Carnap's constructive scruples would not have prevented him from using stronger ω-rules, like the ω_H-rule or the ω_B-rule, he would have had to allow for a transfinite number of applications of these ω-rules in order to arrive at a considerable gain in completeness. In fact, this was what happened to his 'Language II;' *see* [Carnap 1938].

Hence I take it that another question Carnap presumably had was, what increase in applicability does one get from adopting an ω-rule, possibly formalized? In light of the preceding paragraph, his judgment must have been devastating. There is no reason to sacrifice a form of reasoning so well-entrenched as induction in favour of a highly artificial rule, designed for proof-theoretical purposes, without any apparent gain in completeness. Questions of expediency strongly suggest sticking with induction.

Fourth, Gödel's commentary finally answers a question. 'No, Carnap, you cannot restate the ω_H-rule as narrowly as the ω_C-rule, because "Hilbert conceives of it differently, more broadly." '

"Hilbert's new paper; highly questionable."

Imagine Carnap, working on his 'Language I,' reading [Hilbert 1931a] and asking himself, at what price more completeness? Sure, adopting the ω_H-rule (and perhaps even adopting a formalized version of it) would be a gain in completeness, but exactly how much completeness? And would the gain in completeness be only a virtual, merely 'logical' one, or also an increase in the applicability of formalized number theory? But the most pressing question for Carnap, I presume, must have been whether some gain in completeness is worth sacrificing the definiteness of his 'Language I'—for Gödel had informed him that the ω_H-rule should be conceived of "more broadly" than he initially was prepared to do. Weighing a probably small increase in completeness (of doubtful value) against loosing the definiteness of his preferred 'Language I,' does not the loss outstrip the benefit, so that the net gain is at most zero (if not negative)?

We thus arrive at another conjecture of Carnap's, namely, that employing an ω-rule might, in terms of its philosophical net gain, amount to a real disadvantage. What exactly is the outcome of calculating

'losses of employing an ω-rule' vs. 'benefits of employing an ω-rule' ?

With all this in mind, he confided the sceptical entry to his diary, "Hilbert's new paper; highly questionable."

3. Conclusion

To be sure, my interpretation (like any other) of Carnap's difficult to understand note offers no more than guarded speculations. But this is the way historical studies are more or less. Like a fragment of a pre-Socratic, RC 102-43-14 is, taken by itself, evidence too poor to allow for historical reconstruction. Thus, I added two assumptions. (To belabour the obvious, one always needs some insights to get new ones.) The first assumption was that we need to read RC 102-43-14 as Carnap's personal and hence elliptical minutes, to which we have to add what Carnap had in and on his mind in those days. The second assumption was about what Carnap had on his mind during the summer of 1931. What all evidence seems to suggest is that Carnap was busy working on what later became his *Logical Syntax,* and was focusing especially on his 'Language I' during the time in question. In addition, we can draw on the *Logical Syntax* for information as to how Carnap's views developed. I regard both assumptions as highly plausible. The more scanty the facts, the more important becomes coherence for historical truth. The two assumptions enabled us to give RC 102-34-14 a coherent reading that, in addition, does justice to all other documentary evidence, while the "'natural' reading" and the "first scenario" do not. Thus, I'm inclined to think the present paper is justified from a methodological point of view.

So what is the bottom line? The hard facts are as follows. During the summer of 1931 Carnap got to know about the ω-rule from Hilbert's then most recent publication; he learned in particular that it can consistently be added to an arithmetical formalism and that it renders this formalism Π_1-complete. The conjectured facts are as follows. Contrary to the first impression his note conveys, Carnap (and of course Gödel) understood very well the differences between the rule of induction and the ω-rule. In fact, it was precisely this comprehension that made Carnap shrink back from building the ω-rule into this 'Language I.' Further, according to the discussion as reconstructed from his notes, there was no hope of gaining more completeness through an ω-rule formalized so as to make it fit into his 'Language I.' A highly doubtful gain in completeness was not worth the sacrifice of a well-entrenched principle; he preferred to stick to induction. The twist that enabled this interpretation was, essentially, to read Carnap's note on Hilbert's new rule as not referring to the ω_H-rule in the first place, but to a formalized version of it, like the ω_C-rule, for such a rule was of prime interest for Carnap while designing his 'Language I.'

Carnap's work in infinitary logic did not stop here. On the contrary, it was this acquaintance with the ω_H-rule that actually got him started doing serious work in infinitary logic. From then on he would be concerned, for a period of more than 10 years, with developing a satisfying account of infinitary logic, which finally culminated in his theory of junctives (for which see [Carnap 1943], §§ 19–24).[31]

Bibliography

Ackermann, Wilhelm

[1940] "Zur Widerspruchsfreiheit der Zahlentheorie," in: *Mathematische Annalen* **93**, pp. 162–194.

Awodey, Steve & Carus, André

[2001] "Carnap, Completeness, and Categoricity: The *Gabelbarkeitssatz* of 1928," in: *Erkenntnis* **45**, pp. 145–172.

Bernays, Paul

[1941] "Sur les questions méthodologiques actuelles de la theéorie Hilbertienne de la démonstration," in: *Les entretiens de Zurich sur les fondements et la méthode des sciences mathématiques, 6–9 décembre 1938,* ed. by Ferdinand Gonseth, Zurich: Leemann, pp. 144–152.

[31] If there is historical truth—and I firmly believe there is—then it is not one but many. In order to arrive at historical truth(s) it is sufficient to tell a story that conforms with the facts and the evidence available; but there are—and I'm likewise convinced of this—always many stories satisfying this requirement. I have told my story about RC 102-43-14; if others were encouraged to come up with better stories, I will be gratified.

— see [Hilbert/Bernays 1934, 1939].

Buldt, Bernd

[1998] "Infinitary Logics," in: *Routledge Encyclopedia of Philosophy*, ed. by Edward Craig, London & New York: Routledge, vol. 4, pp. 769–772.

[2001] "Vollständigkeit/Unvollständigkeit," in: *Historisches Wörterbuch der Philosophie*, ed. by Gottfried Gabriel *et al.*, Basel: Schwabe, vol. 11, pp. 1136–1141.

[2002] "Philosophische Implikationen der Gödelschen Sätze? Ein Bericht," in: [Buldt *et al.* 2002b], pp. 395–438.

& W. DePauli, C. Klein, E. Köhler, M. Stöltzner, P. Weibel (eds)

[2002a] *Kurt Gödel, Wahrheit und Beweisbarkeit. Vol. 1: Dokumente und historische Analysen*, Wien: Hölder-Pichler-Tempsky.

[2002b] *Kurt Gödel, Wahrheit und Beweisbarkeit. Vol. 2: Kompendium zum Werk*, Wien: Hölder-Pichler-Tempsky.

Carnap, Rudolf

[1934] *Logische Syntax der Sprache* (Schriften zur wissenschaftlichen Weltauffassung; 8), Wien: Springer, ^{2}1968.

[1935] "Ein Gültigkeitskriterium für die Sätze der klassischen Mathematik," in: *Monatshefte für Mathematik und Physik* **42**, pp. 163–190.

[1937] *Logical Syntax of Language*, London: Kegan.

[1938] "Review of [Rosser 1937]," in: *Journal of Symbolic Logic* **3**, p. 50.

[1943] *Formalization of Logic* (= Studies in Semantics; 2), Cambridge/MA: Harvard UP, ^{2}1959.

[1963] "Intellectual Autobiography," in: *The Philosophy of Rudolf Carnap*, ed. by Paul Arthur Schilpp, La Salle/IL: Open Court, pp. 3–84.

[2000] *Untersuchungen zur allgemeinen Axiomatik*, postum ed. by Thomas Bonk & Jesus Mosterin, Hamburg: Meiner.

Carus, André

— see [Awodey/Carus 2001].

Coffa, J. Alberto

[1991] *The Semantic Tradition from Kant to Carnap. To the Vienna Station*, Cambridge: Cambridge UP.

Dawson, John W. Jr.

[1985] "The reception of Gödel's incompleteness theorems," in: *PSA 1984: Proceedings of the 1984 Biennial Meeting of the Philosophy of Science Association*, Michigan: PSA, vol. 2, pp. 253–271.

[1997] *Logical Dilemmas. The Life and Work of Kurt Gödel*, Wellesley/MA: Peters.

DePauli-Schimanovich, Werner (ed.)

— see [Buldt *et al.* 2002a, b].

Feferman, Solomon

[1962] "Transfinite recursive progressions of axiomatic theories," in: *Journal of Symbolic Logic* **27**, pp. 259–316.

Gödel, Kurt

[2003] *Collected Works, Vol. IV: Correspondence A–G*, ed. by Solomon Feferman, John W. Dawson, Jr., *et al.*, Oxford: Clarendon P.

Goldfarb, Warren D.

[1975] "On the effective ω-rule," in: *Zeitschrift für mathematische Logik und Grundlagen der Mathematik* **21**, pp. 409–412.

Hilbert, David

[1905] "Über die Grundlagen der der Logik und Arithmetik," in: *Verhandlungen des Dritten Internationalen Mathematiker-Kongresses in Heidelberg vom 8.–13. August 1904*, Leipzig: Teubner, pp. 174–185.

[1930] "Naturerkennen und Logik," in: *Naturwissenschaften* **18**, pp. 959–963.

[1931a] "Die Grundlegung der elementaren Zahlenlehre," in: *Mathematische Annalen* **104**, pp. 485–494 (partly repr. in: [Hilbert 1935], pp. 192–195).

[1931b] "Beweis des Tertium non datur," in: *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen aus dem Jahre 1931, Mathematisch-Physikalische Klasse*, pp. 120–125.

[1935] *Gesammelte Abhandlungen, Bd. 3*, Berlin: Springer.

& Bernays, Paul

[1934] *Grundlagen der Mathematik I* (= Grundlehren der mathematischen Wissenschaften; 40), Berlin: Springer, rev. ²1968.

[1939] *Grundlagen der Mathematik II* (= Grundlehren der mathematischen Wissenschaften; 50), Berlin: Springer, rev. ²1968.

Kleene, Stephen Cole

[1943] "Recursive predicates and quantifiers," in: *Transactions of the American Mathematical Society* **53**, pp. 41–47.

Klein, Carsten (ed.)

— see [Buldt *et al.* 2002a, b].

Köhler, Eckehardt

[1991] "Gödel und der Wiener Kreis," in: *Jour fixe der Vernunft. Der Wiener Kreis und die Folgen*, ed. by P. Kruntorad, Wien: Hölder-Pichler-Tempsky, pp. 127–158 (see [Köhler 2002a]).

[2002a] "Gödel und der Wiener Kreis," in: [Buldt *et al.* 2002a], pp. 83–108 (rev. version of [Köhler 1991]).

[2002b] "Rudolf Carnap – Kurt Gödel. Gespräche und Briefe 1928–1940," in: [Buldt *et al.* 2002a], pp. 109–128.

— see [Buldt *et al.* 2002a, b].

Kreisel, Georg

[1965] "Review of [Feferman 1962]," in: *Zentralblatt für Mathematik und ihre Grenzgebiete* **117**, pp. 254–256.

Kuratowski, Kazimierz & Tarski, Alfred

[1931] "Les opérations logiques et les ensembles projectifs," in: *Fundamenta Mathematicae* **17**, pp. 240–247.

López-Escobar, E. G. K.

[1976] "On an extremely restricted ω-rule," in: *Fundamenta Mathematicae* **20**, pp. 159–172.

Mostowski, Andrzej

[1947] "On definable sets of positive integers," in: *Fundamenta Mathematicae* **34**, pp. 81–112.

Reid, Constance

[1970] *David Hilbert*, New York: Springer.

Rosser, James Barkley

[1937] "Gödel theorems for non-constructive logics," in: *Journal of Symbolic Logic* **2**, pp. 129–137.

Shoenfield, Joseph R.

[1959] "On a restricted ω-rule," in: *Bulletin de l'Académie Polonaise des Sciences, Série des sciences mathématique, astronomique et physique* **7**(1), pp. 405–407.

Stölzner, Michael (ed.)

— see [Buldt *et al.* 2002a, b].

Tarski, Alfred

— see [Kuratowski/Tarksi 1931].

Weibel, Peter (ed.)

— see [Buldt *et al.* 2002a, b].

Zach, Richard

[1999] "Completeness before Post: Bernays, Hilbert, and the development of propositional logic," in: *Bulletin of Symbolic Logic* **5**, pp. 331–366.

[2001] *Hilbert's Finitism: Historical, Philosophical, and Metamathematical Perspectives*, Ph.D., Berkeley; available at http://www.ucalgary.ca/~rzach.

[2002] "The Practice of Finitism: Epsilon Calculus and Consistency Proofs in Hilbert's Program," to appear in: *Synthese*.