On RC 102-43-14

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0 Introduction

When Carnap's star shone brighter on the philosophical heavens than it does today, he was known for a lot of achievements; but even then his work in infinitary logic was not regarded as one his merits. Rather on the contrary, his work in this area is and always has been more or less completely neglected—unduly neglected, I would like to add, since it is here where he was more advanced than his most advanced contemporaries, and where his general philosophical attitudes probably showed most clearly. This was the main reason why I had chosen to address the meeting 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). Instead, it focuses on just one documentary evidence: a record to be found among the Carnap Papers, which probably is the earliest document proving his interest in and tackling with infinitary logic. Two reasons led me to restrict the scope of the paper. On the one hand, this peculiar note has deeply puzzled the relevant community since it first became known some 10 years ago. On the other hand, it requires quite some background to get elucidated sufficiently enough to make vanish the general puzzlement it has caused. So, a separate treatment seemed recommended.

The paper is organized as follows. The first section presents the background I think necessary for an appropriate explanation of the note which has proven so notoriously difficult to understand. This requires to deal with the ω-rule, Hilbert, Bernays, and Gödel, in this order. Thus prepared, the second section explains first, why Carnap's note is so puzzling. Then it attempts a sentence-for-sentence-interpretation of this note, intended to do away with the mysteries surrounding it. A third and final section offers a short summary of the methods employed, the assumptions made, and the conclusions reached.

For those who are curious now as to what this note says—it is the one with 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 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.[1]

And for those not familiar with what infinitary logics are all about, here a general 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.,

[1] "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 formu[!]lisierten 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 mirror at the *Philosophisches Archiv*, University of Konstanz; I extend my gratitude to Dr. Uhlemann, the curator of the *Archiv*, for her constant helpfulness in all matters concerning 'her' archive's collections. "BP" refers to the Bernays Papers, housed at the *Wissenschaftshistorische Sammlung*, 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 translation are mine, likewise the remarks in square brackets which suggest emendations, point to omissions, etc. A dot "·" indicates a line break in the original text suppressed in the translation; underlining and other means of emphasis in the original text are uniformly rendered as italics.

by admitting infinitely long formulae or infinitely long or branched proof figures. The need to extend first-order logic became pressing in the late 1950s, when it was not only realized but also accepted, that this logic is unable to express most of the fundamental notions of mathematics and thus blocks their logical analysis. Because in many cases IL do not suffer from these limitations, they are an essential tool in mathematical logic since then.[2]

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

Carnap's note is about "Hilbert's new rule of inference," which was a version of the ω-rule and caused, when Hilbert introduced it, at least initially quite some frowning on part of those concerned with foundational issues in logic and mathematics. So the first section shortly explains what the ω-rule is about, while the following three sections deal, respectively, with what we either can safely guess about Hilbert's or do know about Bernays' and Gödel's views on 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 on the ω-rule and only if we know what Gödel knew at that time, we can hope to shed some light on Carnap's minutes.

1.1 The ω-rule

The ω-rule is an infinitary rule of inference that has been employed within mathematical logic in various forms, depending on whether the context is recursion theory, proof theory, or model theory. Thus, strictly speaking, there is not 'the' ω-rule, but a whole family thereof. The first who came to think of and study a version of the ω-rule was Tarski in 1926; but it was Hilbert who, in 1930, hit upon this rule as well and put it in the limelight by his last two publications. In its simplest form it reads, for all expressions φ with one free variable:

$$(\omega\text{-rule}) \qquad \forall n \in \mathbb{N}\,[\,\vdash \varphi(\bar{\mathsf{n}})\,] \quad \Rightarrow \quad \vdash \forall x \varphi(x).$$

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

[2] [Buldt 1998], p. 769.

expression ψ (different for different formal systems) with one free variable such that:

$$(\omega\text{-incom.}) \qquad \forall n \in \mathbb{N}\,[\, \vdash \psi(\overline{\mathsf{n}})\,] \quad \& \quad \nvdash \forall x \psi(x).$$

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

According to the documentary evidence available (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 to safeguard 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's acquaintance with the ω-rule came through publications by Hilbert, the then leading, though at the same time controversial, preeminent figure in the field of logic and the foundations of mathematics, whose axiomatic and metamathematical research programs exercised a considerable influence on Carnap.

> We had a good deal of sympathy with the formalist method of Hilbert [...] and learned much from this school [...].[3]

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.

1.2 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 a secure grounding of mathematics by metamathematical (proof-theoretical) investigations made him a first authority also in foundational issues. Because his program was partly designed to silence his critics by outdoing them in their

[3] [Carnap 1963], p. 48.

constructivism, his stress on finitary considerations and finitary means in investigations into the foundations of logic and mathematics was well-known.[4]

So it came as a surprise to the 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 $\forall x \varphi(x)$ may be used as a first formula [in a derivation, i.e., as an axiom].[5]

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

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

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

1. The ω_H-rule can consistently be added to a formal system of arithmetic, say, $\mathcal{PA}$, the first-order system of Peano-Arithmetic.[6]

2. It renders the resulting semi-formal system, say, $\mathcal{PA}^\omega$, Π_1-complete in the sense of Hilbert (abbreviated as "H-complete for Π_1"); in short:

$$(\text{H-Com}_{\Pi_1}) \qquad \forall \varphi \in \Pi_1\ [\ \text{consistent}\,(\mathcal{PA}^\omega \cup \{\varphi\}) \ \Rightarrow\ \vdash_{\mathcal{PA}^\omega} \varphi\ ].^7$$

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

[5] "Falls nachgewiesen ist, daß die Formel $\cdot\ \mathfrak{A}(\mathfrak{z})\ \cdot$ allemal, wenn $\mathfrak{z}$ eine vorgelegte Ziffer ist, eine richtige numerische Formel wird, so darf die Formel $\cdot\ \forall x\varphi(x)\ \cdot$ 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 22). This strongly suggests that Carnap's discussion with Gödel was triggered by [Hilbert 1931a].

[6] *See* [Hilbert 1931a], p. 491 (= [Hilbert 1935], pp. 194 *seq.*). – Hilbert (and Bernays) usually worked with a formal system called "$\mathfrak{Z}$" (*see* [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 quotations.

[7] *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].

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

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

This completeness of the semi-formal system $\mathcal{PA}^\omega$ would have been enough to escape the original formulation of Gödel's incompleteness result.[8]

Any application of the ω_H-rule requires, for all $n \in \mathbb{N}$, 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 should be stressed the important and for our investigation crucial fact, namely, 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.[9]

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

1.3 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 about 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.[10]

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[8] Contemporary readers may wonder why Hilbert rested contend at proving Π_1-completeness and did not immediately show completeness in respect to 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.

[9] "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)

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

Bernays communicated the ω_H-rule and the accompanying completeness result in his second letter to Gödel, dated 18 Januar 1931.[11] Besides giving the ω_H-rule a slightly more general form, he more importantly shed some light on how the numerical correctness check for the $\varphi(\overline{\mathsf{n}})$'s in the antecedent of the ω_H-rule and on how the consistency of $\mathcal{PA} \cup \{\varphi\}$ in H-Com$_{\Pi_1}$ are to be determined according to Hilbert.

> If $\mathfrak{A}(x_1, x_2, \ldots, x_n)$ is (according to your terminology) a *recursive* formula, of which it can be shown, by finitary means, that for arbitrarily given number values $x_1 = \mathfrak{z}_1,\ x_2 = \mathfrak{z}_2\ \ldots\ x_n = \mathfrak{z}_n$ it results in a numerical identity, then the formula $(x_1)(x_2)\ldots(x_n)\ \mathfrak{A}(x_1 \ldots x_n)$ may be used as a first formula (i. e., as an axiom).
>
> Now Hilbert proves by a simple argument that each formula $(x_1)(x_2)$ $\ldots(x_n)\ \mathfrak{A}(x_1 \ldots x_n)$, where $\mathfrak{A}(x_1 \ldots x_n)$ 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.[12]

Thus, required conditions 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 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 held

[11] The correspondence between Bernays and Gödel relevant here is partly reproduced in [Buldt *et al.* 2002b], pp. 139–146, and will appear in its entirety in [Gödel 200?].

[12] "Die Hilbertsche Erweiterung besteht nun in folgender Regel: Wenn · $\mathfrak{A}(x_1, x_2, \ldots, x_n)$ · eine (nach ihrer Bezeichnung) *rekursive* Formel ist, von der sich finit zeigen lässt, dass sie für beliebig gegebene Zahlwerte $x_1 = \mathfrak{z}_1,\ x_2 = \mathfrak{z}_2\ \ldots\ x_n = \mathfrak{z}_n$ eine numerische Identität ergibt, so darf die Formel · $(x_1)(x_2)\ldots(x_n)\ \mathfrak{A}(x_1 \ldots x_n)$ · als Ausgangsformel (d. h. als Axiom) benutzt werden. · Hilbert zeigt nun durch eine einfache Überlegung, dass jede Formel · $(x_1)(x_2)\ldots(x_n)\ \mathfrak{A}(x_1 \ldots x_n)$, · bei welcher · $\mathfrak{A}(x_1 \ldots x_n)$ · 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 [...] *beweisbar* ist." (GP 010015.45, pp. 4–5; [Buldt *et al.* 2002b], pp. 139 *seq.*) – "Recursive in Gödel's sense" is what current usage knows as "primitive recursive."

to furnish the ω_H-rule and H-Com$_{\Pi_1}$ with what was needed, 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 never was published.)[13] 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 Akkermann (or von Neumann) for demonstrating the consistency of $\mathfrak{Z}$.[14]

Hence, Hilbert could 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.[15]

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

> Also concerning Ackermann's proof for the consistency of number theory, I believe I am about straightening things 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 $\mathfrak{Z}$.[16]

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[13] *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.

[14] "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) – While the letter makes a claim only as to the consistency of the ω_H-rule when added to $\mathfrak{Z}$, I entertain the view that Hilbert thought Ackermann's work using ϵ-elimination would also guarantee the finitary character of the ω_H-rule. This is a novel view and might not find the enthusiastic approval of all Hilbert scholars. Hence, I would like to stress that concerning my interpretation of Carnap's note nothing in particular hinges on this reading of Hilbert.

[15] *See* footnote 9 for the exact wording.

[16] "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*

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.[17]

innerhalb des Systems $\mathfrak{Z}$ *formulierbar* sind." (GP 010015.47, pp. 1–2) – 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 more often: Bernays was ahead of Hilbert in accommodating to new facts, with Hilbert lagging stubbornly behind (*see* in this connection [Reid 1970], p. 172, describing Hilbert as "slow to understand"). This might have very well 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 seeing 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, terminology-wise [Hilbert 1931b] continues Hilbert's old terminology as employed in [Hilbert 1904]. Third, we know that the relationship between Bernays and Hilbert saw, partly violent, disagreement over foundational issues (*see* [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 up with Gödel's results. (In case of the first alternative, the growing alienation, the fact, that Hilbert kept Bernays as his assistent on private expenses until the situation in Nazi-Germany became unbearable for Bernays in the spring of 1934 (when he left for Zurich, *see* [Reid 1970], pp. 205 *seq.*), would then be solely due to Hilbert's wish to see the two volumes of [Hilbert/Bernays] finally go to the press.)

[17] Certain weak versions of the ω-rule do indeed result in conservative extensions and

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

$$(\omega_B\text{-rule}) \qquad \forall n \in \mathbb{N}\, [\, \vdash \varphi(\overline{\mathsf{n}})\,] \quad \Rightarrow \quad \vdash \forall x \varphi(x).$$ [18]

Although his version of the ω-rule seemed to be much stronger, he had no clue as to whether it guaranteed already closure under the new rule or not. Hence, he posed this as a question to Gödel.

1.4 Gödel

It took even a Gödel some time to digest the news. First of all he had of course 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.[19]

are hence of purely proof-theoretical interest; *see* [López-Escobar 1976] for an example.

[18] "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)

[19] "Zunächst kann man zeigen, daß auch die Systeme $\mathfrak{Z}^*$, $\mathfrak{Z}^{**}$ nicht deduktiv abgeschlossen sind, [...]." (BP Hs 975 1691a, p. 1) – 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 TA-completeness, something that seems to be put into question now. The explanation for this apparent conflict is, that the proof of TA-completeness assume closure

Second, Gödel filed the complaint that,

> one cannot rest assured at 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.[20]

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 $\varphi(\overline{\mathsf{n}})$ is numerically correct for all $n \in \mathbb{N}$—can be fulfilled with 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, 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 gathered the necessary background information, 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 turns then 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 onto the reader as 'natural,' is highly unsatisfactory; hence, this first section is entitled "Problems." The second

under the ω-rule, which can be attained only after a transfinite number of its application; *see* footnote 33 for details.

[20] "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) – 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.

section attempts a more satisfying interpretation of RC 102-43-14 and the accompanying entry to Carnap's diary, dated 12 August 1931. It offers a sentence-for-sentence interpretation and is thus 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-notched foundational researcher of his time. Sure enough, a much more favourable outcome; hence, this second section is called "Solutions."

2.1 Problems

The summer of 1931 was the time when, after having abandoned the first and ill-fated project *Untersuchungen zur allgemeinen Axiomatik* (Investigations Into a General Axiomatics), Carnap started writing *Versuch einer Metalogik* (Essay on Metalogic), which would finally become his *Logical Syntax of Language*.[21] 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.[22] 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 carry even the same title "Gödel Fragen" (14 March and 9 June 1931).[23] But apparently they continued their exchange beyond the questions

[21] For the abandoned manuscript of the *Untersuchungen*, recently published as [Carnap 2001], *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.

[22] Though the sheet carries the date 12 July 1931, [Köhler 1991], p. 144 (= [Köhler 2002a], p. 96), suggests that the actual exchange 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.

[23] *See* the material from the Carnap Papers included to [Köhler 1991; 2002a+b].

Carnap had prepared in advance—as indicated by a horizontal line in the manuscript dividing off the first section from a second—in order to discuss [Hilbert 1931a], which had just arrived at the library shelves.

It is astonishing, that Carnap's note seems to not 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 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.[24]

Two problems hit one in the eye: 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 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? How could Gödel leave Carnap at that, 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 spoken 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 would not have realized the difference between the two rules.

But, in the light of what was mentioned above concerning Gödel's correspondence with Bernays, this 'natural' reading is out of question and we thus find us at the horns of a dilemma. For, alternatively, one could hold onto 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).

[24] RC 102-43-14; note, dated 12 July 1931; [Köhler 1991], p. 144 (= [Köhler 2002a], p. 96; *see* footnote 1 for the German text.

This last alternative cannot be dismissed out of hand, since Carnap seems to have been rather slow in grasping already the importance of Gödel's first incompleteness theorem. Gödel told Carnap about his first 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.[25] As unattractive as this alternative is, as unlikely is, from what else we know, the first alternative. 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 23). 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 poor enough a conclusion to encourage us looking for a different reading of the text and thereby saving Carnap from the charge of being slow on the uptake. This will be done (and, as I hope, be accomplished) in the next section.

2.2 Solutions

Trying my hands 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.

2.2.1 "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.[26] not to the whole work, but in particular to its 'Language I.' But if this is true, then the concluding sentence simply says, that

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

[26] *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 featured its first appear-

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.[27]

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

> I want make do *without sentence variables, predicate variables* (and variables for number functions).[28]

Hence, 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 to and to ban ω-rules from his 'Language I.' Seen in this light, the conclusion, "therefore, complete induction for my system," makes perfectly sense.

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ance 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 being, 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 get established here).

[27] [Carnap 1963], p. 49.

[28] "Ich möchte *ohne Satzvariable, Prädikatsvariable* und (Zahlfunktionsvariable) auskommen." (RC ► **???** ◄; 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, § 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].

2.2.2 "*Gödel*: But Hilbert conceives of it differently, more broadly; the condition is meant to be the following: 'If ... is provable with 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 all 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 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—, asked him, what it could possibly mean, that Hilbert characterized the ω_H-rule as a finitary rule. Carnap wanted to know, for, according to the last sentence of his minutes, he was pondering the question whether he should better include an ω-rule to his system or not. To get clear about this issue, Carnap, always eager to give concepts a meaning as precise as possible, probably even suggested "provable by formalized means" as an 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.)

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. Though without knowing how tightly Hilbert's claims were connected to Ackermann's consistency proof, the conditions for applying the ω_H-rule appeared Gödel to be far too unspecific as 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 withheld his opinion from Carnap.

But all this was all grist again on Carnap's mills 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 the constructive purity of his 'Language I.' In the light of the clarification Gödel was able to provide, the following "therefore complete induction" was hence a fully justified conclusion on Carnap's side.

2.2.3 "*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 onto every reader of Carnap's note, namely: Why is there no record of Gödel correcting Carnap's apparently outright mistaken opening statement?

The First Scenario. The first scenario 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 the stronger rule. (This is the result of comparing the ω-rule with ω-incompleteness as given in § 0 above, which must have hit also Carnap in the eye.) If this were true, i. e., if Carnap did not see Π_1-completeness to follow from the ω_H-rule, well, then Carnap was sure enough set right by Gödel; for Gödel knew better, as we have seen above. Moreover, this being the easy lesson we just assumed, 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 an oversight was corrected far too trivial to require a written record for later perusal.

This reading of the text requires as an auxiliary hypothesis that Carnap considered worth 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 both, 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 were the easy lesson that immediately stuck as this scenario assumes, then why should have Carnap preserved his former error at all? Second, it leaves, where we may expect a thread underlying the recorded discussion, the first and second sentence completely unrelated to each other; thus, this scenario does not provide a coherent meaning to the whole note. Third, it forced us to believe that Carnap was unable to add up two and two. For on the one hand Carnap knew about the Π_1-incompleteness of $\mathcal{PA}$ for approximately a year, while on the other hand he read in Hilbert's paper a proof for the Π_1-completeness of $\mathcal{PA}^\omega$. Carnap was slow, perhaps, but definitely not blind.

The Second Scenario. The second scenario avoids these weak spots and does so by a reading of the opening sentence in the light of Gödel's subsequent answer. It is brought about by 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 had in mind, when he stated that the correctness of $\varphi(\overline{\mathsf{n}})$ can finitarily be proven for all $n \in \mathbb{N}$; 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 very well Gödel's arithmetization technique, but even granted it a modest further development for his own purposes.[29] Thus, taking these considerations together, Carnap's initial understanding of the ω_H-rule must have been something like:

$$(\omega_C\text{-rule}) \qquad \vdash_{\mathcal{L}1} \forall x\, [\, \mathsf{Pr}_{\mathsf{L1}}(\ulcorner \varphi(\dot{x}) \urcorner)\,] \quad \Rightarrow \quad \vdash_{\mathcal{L}1} \forall x \varphi(x)$$

(the index "$\mathcal{L}1$" refers to the formal system of 'Language I'). I do not propose, of course, that during the discussions with Gödel Carnap wrote down on the

[29] *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.

coffee house table the ω_C-rule exactly as given above. Rather, the peculiar formulation of the ω_C-rule is intended to make explicit what Carnap's best guess could have been, while striving for making precise Hilbert's claim that the ω_H-rule is finitary.[30] What I do assume, however, is that Carnap thought of formalizing the antecedent of the ω-rule; for otherwise, as indicated above, the reference as to "provable with 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:

$$(CC) \qquad \{\varphi : \mathsf{IND} \vdash_{\mathcal{L}^{-}1} \varphi\} \overset{?}{\approx} \{\varphi : \omega_c\text{-rule} \vdash_{\mathcal{L}^{-}1} \varphi\}.$$

But stating CC is not the embarrassing mistake the first impression of Carnap's opening sentence made us believe it were. Carnap does not put into

[30] 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 due to the intention 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:

$$(\omega_{C^*}\text{-rule}) \qquad \forall n \in \mathbb{N} \left[\vdash_{LI} \mathsf{Pr}_{\mathsf{LI}}(\ulcorner \varphi(\overline{\mathsf{n}}) \urcorner)\right] \;\; \Rightarrow \;\; \vdash_{LI} \forall x \varphi(x).$$

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).

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. Four remarks along this line.

First, at a time when 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: $\mathcal{PA} \subsetneq \mathcal{PA}^{\omega_H}$ (Bernays) and $\mathcal{PA}^{\omega_B} \subsetneq \mathsf{TA}$ (Gödel). Logicians were able to study formalized ω-rules—after a first step taken by [Rosser 1937]—only by the late 1950s.[31] 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 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.[32]

Let "$\mathcal{F}^{\omega_*}_{\alpha}$," with α an infinite limit ordinal, denote a formal system of arithmetic $\mathcal{F}$, in which less 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., $\mathcal{L}^{-}1^{\omega_c}_{\omega}$. Due to the lack of induction, $\mathcal{L}^{-}1^{\omega_c}_{\omega}$ is at most as strong as $\mathcal{L}1$:

$$\mathcal{L}^{-}1^{\omega_c}_{\omega} \overset{?}{\approx} \mathcal{L}1.$$

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 $\mathcal{L}^{-}1^{\omega_c}$; then the deductive strength of $\mathcal{L}^{-}1^{\omega_c}_{\omega}$ amounts (at the very most) to that of $\mathcal{L}1$. This way of estimating the deductive power of $\mathcal{L}^{-}1^{\omega_c}_{\omega}$

[31] [Shoenfield 1959] and [Feferman 1962] are the milestone papers in case.

[32] [Carnap 1937], p. 106 (= [Carnap 1935], p. 173).

does not presuppose anything that were 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 $\mathcal{L}^{-}1^{\omega_c}_{\omega}$ does thus not give cause for any hopes. The conclusion as to whether the ω_C-rule (or any other ω-rule) is to preferred over the rule of induction—because a rough estimate gives $\mathcal{L}^{-}1^{\omega_c}_{\omega} \approx \mathcal{L}1$ (with '$\mathcal{L}1 = \mathcal{L}^{-}1 + \mathsf{IND}$'!)— is, "therefore, merely a question of expediency."[33]

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 at its applicability.

> Since [...] I came to philosophy from physics, [I] looked at mathematics always from the the point of view of its application in empirical science.[34]

Accordingly, all his logic books do not only 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

[33] 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 $n \in \mathbb{N}_0$, and from [Goldfarb 1975], that $\boldsymbol{\mathcal{L}1}^{\omega_B}_{\omega^2}$ is 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 $\alpha > \omega + n$, $\alpha > \omega^2$ 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 $\forall x[\mathsf{Pr}(\ulcorner\varphi(\dot{x})\urcorner)] \rightarrow \forall x\varphi(x)$, 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 TA-complete; but only if, in order to define the α's, a suitable path through $\mathcal{O}$, the class of all recursive ordinals, will be chosen. This shows how much weaker the ω_C-rule is compared to 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 should 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].

[34] [Carnap 1963], p. 48.

> 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.[35]

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

2.2.4 "Hilbert's new paper; highly questionable."

Imagine Carnap, amidst of moulding 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 up a probably small increase in completeness (of doubtful value) with loosing the definiteness of his preferred 'Language I,' does not the loss outstrip the benefit, such that the net gain is at most zero (if not negative)?

We thus arrive at another conjecture of Carnap, namely, that employing an ω-rule has, in terms of its philosophical net gain, in its wake a real disadvantage:

$$\{\text{losses of employing an } \omega\text{-rule}\} \ \overset{?}{\text{outstrip}} \ \{\text{benefits of employing an } \omega\text{-rule}\}.$$

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

[35] *ibid.*, p. 49.

3 Conclusion

Sure enough, my interpretation (like any other) of Carnap's difficult to understand note does not offer more than guarded speculations. But this is the way historical studies more or less are. RC 102-43-14 is (like a fragment of a pre-Socratic), taken by itself, evidence too poor to allow for historical reconstruction. Hence, I added two assumptions. (To labour the obvious, one always needs some insights to get new ones.) The first assumption was 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 these 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 focussing especially on his 'Language I' during the relevant time in question. In addition, we could draw on the *Logical Syntax* for information as to how Carnap's developed views looked like. I regard both assumptions as highly plausible. The more scanty the facts, the more important becomes coherence for historical truth. The two assumptions made, enabled us to give RC 102-34-14 a coherent reading that 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 arithmetic 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 also no hope for gaining more completeness through an ω-rule so formalized as to make it fit into his 'Language I.' A highly doubtful gain in completeness was not worth the sacrifice in constructivity; he rather stuck 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 to do serious work in infinitary logic. From now on he will 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).[36]

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

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