Chapter 5
Rudolf Carnap's Inferentialism

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5.1 Introduction

Carnap's development from his *Logical Syntax of Language*[1] to his *Introduction to Semantics*[2] and *Meaning and Necessity*[3] has often been construed as an awakening from a dogmatic semantics-blindness. Carnap's own description of his awakening, as reproduced by Coffa, suggests an almost mystical initiation:

> Carnap used to tell his students a story about the first time Tarski explained to him his ideas on truth. They were at a coffeehouse, and Carnap challenged Tarski to explain how truth was defined for an empirical sentence such as 'This table is black'. Tarski answered that 'This table is black' is true iff this table is black; and then, Carnap explained, "the scales fell from my eyes".[4]

However, recent developments within logic and philosophy of language have indicated that to see his move from the syntactic period to the semantic one as simply a progress might be an oversimplification. The point is that many subsequent philosophers concentrating on meaning have concluded, perhaps paradoxically, that meaning *is* a matter of "syntax" in Carnap's original sense of the word. (From this viewpoint, Carnap's own terminology, which was subsequently embraced by the majority of logicians, may be seen as severely misleading.)

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[1] Rudof Carnap, *Logische Syntax der Sprache*. Wien: Springer 1934. Revised English edition *The Logical Syntax of Language*, London: Kegan Paul 1937.

[2] Rudolf Carnap, *Introduction to Semantics*. Cambridge (Mass.): Harvard University Press 1942.

[3] Rudolf Carnap, *Meaning and Necessity*. Chicago: The University of Chicago Press 1947.

[4] J. Alberto Coffa, *The Semantic Tradition from Kant to Carnap*. Cambridge: Cambridge University Press 1991, p. 304.

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In his *Logical Syntax of Language* (hereafter LSL), Carnap held that the fact that he was studying exclusively syntax is a kind of restriction – an inevitable, and hence not a deplorable restriction, but a restriction nonetheless. He did not doubt that there exists a semantics that is not in itself dependent on syntax (in John Searle's memorable phrase, "syntax alone is not sufficient for semantics"[5]), but he was convinced that it can be captured only indirectly, via syntax. However, later there appeared philosophers who were to conclude that the independence of semantics is illusory: that what we perceive as semantics is a matter entirely of syntax – or we should better say of what Carnap termed "syntax".

One of the philosophers who provoked this train of thought was the later Wittgenstein[6] – his conviction was that meaning is a matter of *use* and that the relevant kind of use is a matter of *rules*, hence that meaning is something akin to the role conferred on an expression by means of the rules which govern it within our language games. He protested against the dilemma "either an expression *stands for* something or it means *nothing*" – according to him an expression can come to mean something without coming to stand for something, just like a piece of wood may become a king, a rook or a pawn simply by us opting to treat them according to the rules of chess.[7]

Slightly later, but independently of this, Sellars[8] developed his theory of language as a matter of "rule-governed behavior". (Language, for Sellars, is to be understood as an activity that is essentially rule-based, though it is usually not based on explicit rules.) Sellars argued that claims to the effect that an expression means this-and-so are in essence *classificatory* claims, spelling out the *role* of the expression *vis-à-vis* the rules which govern them. And those rules that are the crucial ones from the semantic viewpoint are the *inferential* ones.

The recent movement "from semantics back to syntax" has culminated in what is now being called *inferentialism* – the conviction that semantics is a matter of the inferential rules of language and that meaning amounts to an inferential role.[9] The greatest obstacles an inferentialist must face are two: first, it is necessary to show that the meanings of *empirical expressions* can also be construed plausibly as inferential rules; and second, it must face up to and deal with the results of logic indicating that no inference relation can ever wholly catch up semantics – *viz*. Gödel's

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[5] John R. Searle, *Minds, Brains, and Science*. Cambridge (Mass.): Harvard University Press 1984.

[6] Ludwig Wittgenstein, *Philosophische Untersuchungen*. Oxford: Blackwell 1953. English translation *Philosophical Investigation*, Oxford: Blackwell 1953.

[7] Friedrich Waismann, *Wittgenstein und der Wiener Kreis*. Frankfurt: Suhrkamp 1984, p. 105. See also Jaroslav Peregrin, *Inferentialism: Why Rules Matter*. Basingstoke: Palgrave 2014, Chap. 3.

[8] Wilfrid Sellars, "Language, Rules and Behavior", in: Sidney Hook (Ed.), *John Dewey: Philosopher of Science and Freedom*. New York: Dial Press 1949, pp. 289–315; "Some Reflections on Language Games", in: *Philosophy of Science* 21, 1951, pp. 204–228; "Language as Thought and as Communication", in: *Philosophy and Phenomenological Research* 29, 1969, pp. 506–527.

[9] Robert Brandom, *Making it Explicit*. Cambridge (Mass.): Harvard University Press 1994; and Peregrin, *Inferentialism: Why Rules Matter*, *op. cit.*

proof that there is a gap between truth and proof, and Tarski's demonstration that inference is not capable of capturing all instances of consequence.

Now, the second of these problems is something with which Carnap, in the LSL, was already wrestling. And I think that from the perspective of current inferentialism, his wrestling may be very instructive. Perhaps Carnap's syntactic phase may be interesting not only as a prolegomenon to his more mature semantic phase, but also in its own right (and, moreover, there might be a perspective from which his move from the former to the latter might appear not as a progress at all).

5.2 Logical Syntax of Language

What, according to Carnap, is *syntax*? In the introduction of LSL Carnap writes:

> By the **logical syntax** of a language, we mean the formal theory of the linguistic forms of that language – the systematic statement of the formal rules which govern it together with the development of the consequences which follow from these rules.[10]

Thus, there are two basic concepts that are interconnected with the concept of *syntax*: that of *form* and that of *rule* – syntax is the theory of *formal rules*. And Carnap goes on to explain what he means by the attribute *formal*:

> A theory, a rule, a definition, or the like is to be called *formal* when no reference is made in it either to the meaning of the symbols (for example, the words) or to the sense of the expressions (e.g. the sentences), but simply and solely to the kinds and order of the symbols from which the expressions are constructed.[11]

The rules which constitute the domain of syntax are the formal ones, but are there some non-formal rules? Presumably yes (though Carnap has little to say about them) – they are rules in which there is a reference to "the meaning of the symbols or to the sense of the expressions". This would suggest that whereas rules like

An expression starting with the letter 'a' can be concatenated with an expression starting with the letter 'b'

or perhaps

An expression of the syntactic category *noun* can be concatenated with an expression of the category *verb*

are formal, the rule

An expression referring to an object can be concatenated with an expression expressing a property

is not.

[10] Carnap, *The Logical Syntax of Language*, *op. cit.*, p. 1.

[11] *Ibid.*

Carnap wants to argue that perhaps despite appearances, the rules logic should be interested in are exclusively the formal ones. He writes:

> But even those modern logicians who agree with us in our opinion that logic is concerned with sentences, are yet for the most part convinced that logic is equally concerned with the relations of meaning between sentences. They consider that in contrast with the rules of syntax, the rules of logic are non-formal. In the following pages, in opposition to this standpoint, the view that logic, too, is concerned with the *formal* treatment of sentences will be presented and developed.[12]

In this way he reaches the conclusion that his logical syntax, which deals with transformation rules (i.e. deduction or inference) is no different from syntax in the narrower sense of the word, which deals with formation rules (i.e. which accounts for well-formedness). I think this conclusion requires an important qualification.

Let me call the syntax in the narrower sense of the word, excluding Carnap's logical syntax, *linguistic* syntax, just to have a name for it. Consider a list of names, and assume, for simplicity's sake, that it does not contain any duplicates. Consider singling out various sublists of this list. Some of such sublists can be delimited purely formally. Thus we can have sublists consisting of:

all names with surnames beginning with *S*
all names that read *John Smith*, or *Martin Jones* or *James Black*
all names whose given names have less than five letters or their surnames have more than five letters
etc.

But we can have also non-formally delimited subsets:

names of all persons that were born after 1980
names of all persons who are members of the local chess club
names of all persons who speak Russian
etc.

Now assume that the local chess club consists of John Smith, Martin Jones and James Black. In this case, the sublist delimited by the second formal condition coincides with the one delimited by the second non-formal condition. Should we say that referring to a member of the local chess club is a formal property after all? This is hardly plausible. All that we have is a formal criterion that happens to give us the same result as a non-formal one and which thus can be considered as a *formal indicator of an informal property*.

This singles out an important fact, namely that sometimes syntactic properties can act as indicators – in the above sense of the term – of semantic properties. Of course, this is not an inherent feature of any kind of syntactic properties, but there are properties that in some contexts can be used in this way. However, once this possibility is in play, it wholly changes the nature of the syntax/semantic boundary.[13]

[12] *Ibid.*

[13] All of this is merely implicit in LSL; Carnap only discusses it explicitly in his subsequent writings, especially in *Formalization of Logic*, Cambridge (Mass.): Harvard University Press 1943.

5.3 Syntax as Approximating Semantics

One way of looking at formal logic is to look at it as a theory of such formal indicators. Consider conjunction. Many logicians would say that what makes a sign a *conjunction* is the fact that it expresses the well-known truth function, mapping two truth values on *truth* just in the case that both of them are *truths*. This is certainly a non-formal property: a conjunction sign can look however we want because what makes it a conjunction is not its form, but its meaning.

Now suppose that I have a wholly formally defined relation of inference among sentences of a language, $\vdash$, and I define that a sign $\oplus$ is a conjunction if for all sentences A and B it is the case that

($\oplus$1) $A \oplus B \vdash A$
($\oplus$2) $A \oplus B \vdash B$
($\oplus$3) $\mathrm{A, B} \vdash \mathrm{A} \oplus \mathrm{B}$.

This definition is purely formal and certainly does not guarantee us that $\oplus$ will be conjunction in the previous, informal sense.

But now suppose that the inference relation $\vdash$ is set up so that it preserves truth – that is, that if

$A_1,\ldots,A_n \vdash A$

and all of $A_1,\ldots,A_n$ are true, then A is bound to be true too. Given this, the formal definition of conjunction *does* generate the same result as the non-formal one. Indeed, given that $\vdash$ is truth-preserving, $A \oplus B \vdash A$ tells us that whenever $A \oplus B$ is true, A is bound to be true too, or, in other words, that the falsity of A entails the falsity of $A \oplus B$; similarly $A \oplus B \vdash B$ tell us that the falsity of B entails the falsity of $A \oplus B$; and A, $B \vdash A \oplus B$ tells us that the truth of both A and B entails that of $A \oplus B$. Altogether, $\oplus$ behaves in accordance with the usual truth function.

In this situation, the formally defined conjunction is what we called *a formal indicator* of the informal concept. Given this, the similarity between linguistic syntax and Carnapian logical syntax would be rather superficial – for whereas the rules of the first of them would *target* the forms of the expressions, the rules of the second would target meanings, and the forms would be just useful *means* of targeting them. It seems that it is precisely this view that is urged by Prior:

> It is one thing to define "conjunction-forming sign", and quite another to define 'and'. We may say, for example, that a conjunction-forming sign is any sign which, when placed between any pair of sentences P and Q, forms a sentence which may be inferred from P and Q together, and from which we may infer P and infer Q. Or we may say that it is a sign

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There he uses the term *mirroring*. He writes (p.3): "Thus e.g. the fact that a certain sentence S_1 is true is itself of a semantical, not a syntactical, nature. But it can be formalized, i.e. mirrored in a syntactical way, if a calculus K is constructed in such a way that S_1 is C-true in K." Thus it was only later that Carnap came to duly appreciate this intricacy of the syntax-semantics relationship. (However, even then he restrict himself to hints and does not give any thorough discussion of this intricate problem.)

> which, when placed between any pair of sentences P and Q, forms a sentence which is true when both P and Q are true, and otherwise false. Each of these tells us something that could be meant by saying that 'and', for instance, or '&', is a conjunction-forming sign. But neither of them tells us what is meant by 'and' or by '&' itself.[14]

Hence from this viewpoint, Carnap's claim that "logic is a part of syntax" should be replaced by the claim that logic deals with properties that are themselves not necessarily syntactical, and hence formal, *in terms of their formal indicators.*[15] Or, alternatively, we may accept Carnap's usage as an extension of the standard usage of the term *syntax* in such a way that inference is also subsumed under it (which is what *de facto* largely happened). But then we must keep in mind that syntax consists of two quite different parts – linguistic syntax, which would concern just forms and well-formedness, and logical syntax concentrating on inference.

5.4 Inferentialism

But we can look at the whole situation also in an entirely different way. We may hold that inference is not a formal indicator of something informal (especially of consequence, which resides in the ineffable realm of semantics): rather, it is itself the backbone of the semantics of language. Go back to the inferences we used to characterize the conjunction sign. As people like Prior would tend to see it, they are the indirect way of getting hold of the real meaning of the sign, the truth function. But why, we ask now, must the truth function be the real meaning of the sign? What leads us to this assumption?

What does one learn when one learns that the English 'and' is a conjunction? A reasonable answer would be that one learns that to assert a complex sentence formed by means of 'and' is correct just in the case it is correct to assert each of its two subsentences individually; and this is precisely what (⊕1) – (⊕3) tell us. Therefore we can say that the semantics of conjunction directly *resides* in the rules governing the conjunction sign. According to this view, stating what is meant by 'and' or by '&', is, *pace* Prior, nothing over and above stating that 'and' or '&' is a "conjunction-forming sign".

What, then, about the truth function? How do we account for the fact that it seems to characterize the semantics of conjunction? Certainly not so that the relation of the word to it would be akin to the relation between a name and its bearer – surely our ancestors did not christen the function by the term. But we have already shown that the inferential pattern constitutive of conjunction can be seen as determining the truth table; and from the converse perspective we can say that the truth table sums up the inferential pattern.

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[14] Arthur N. Prior, "Conjunction and Contonktion Revisited", in: *Analysis* 24, 1964, p. 191.

[15] *Viz.* are "mirrored" by syntactic properties – see footnote 13.

This lastly presented view is the inferentialist one, according to which semantics itself might also be a matter of (formal) rules. But whether we accept this or not, we should notice that wholly in accordance with the previous, more conservative view, it implies that there is a radical difference between linguistic syntax and Carnapian logical syntax, and that bringing both of them under the common umbrella of *syntax* might be severely misleading.

Here we seem to clash with the Searlian dictum "syntax is not enough for semantics" – but this clash should not be seen as so controversial as it might look *prima facie*. The point is that the dictum appears almost self-evident only when we read the term *syntax* it contains in the sense of *linguistic* syntax – whereas if we construe it as a Carnapian, *logical* syntax, then it is far less clear why it should hold. What inferentialism claims is certainly not that *linguistic* syntax would ever be able to bear semantics – but it *does* claim that *logical* syntax directly captures what is constitutive of semantics.

5.5 Semantics

What, according to Carnap, is semantics? We have very explicit answers to this question in Carnap's later writings, but there is little about it in LSL. Most of what Carnap writes about semantics are marginal remarks. Thus, he writes:

> We only mean that syntax is concerned with that part of language which has the attributes of a calculus – that is, it is limited to the formal aspect of language. In addition, any particular language has, apart from that aspect, others which may be investigated by other methods. For instance, its words have meaning; this is the object of investigation and study for semasiology.[16]

From such pronouncements we learn that language has a semantics, but not very much about what the semantics consists in. Again on p. 233 Carnap writes:

> We have already seen that this formal method can also represent concepts which are sometimes regarded as not formal and designated as concepts of meaning (or concepts of a logic of meaning), such as, for instance, consequence—relation, content, relations of content, and so on. Finally we have established the fact that even the questions which refer to the interpretation of a language, and which appear, therefore, to be the very opposite of formal, can be handled within the domain of formal syntax.

From this we learn that questions of interpretation, which undoubtedly belong to (or constitute the core of?) semantics "appear the very opposite of formal". And again, on page 159 we read:

> Questions about something which is not formally representable, such as the conceptual content of certain sentences, or the perceptual content of certain expressions, do not belong to logic at all, but to psychology.

[16] Carnap, *The Logical Syntax of Language*, *op. cit.*, p. 5.

Let us complement these fragmentary remarks about semantics and meaning by an explicit *communiqué* of the nature of semantics, as presented in Carnap's later writings:

> When we observe an application of language, we observe an organism, usually a human being, producing a sound, mark, gesture, or the like as an expression in order to refer by it to something, e.g. an object. Thus we may distinguish three factors involved: the speaker, the expression, and what is referred to, which we shall call the *designatum* of the expression. ... If we abstract from the user of the language and analyze only the expressions and their designata, we are in the field of *semantics*.[17]

Hence the overall situation seems to be clear. An expression is meaningful in that it refers to a designatum. Those properties that concern only the expression as a "sign-design" are its formal properties; those which concern its designatum are non-formal. Similarly, those rules that involve the expression merely as a sign-design are formal rules; while those which concern its designatum are non-formal rules. And while syntax is a matter of the formal rules, semantics is a matter of the non-formal (non-formal rules, and perhaps also non-formal "non-rules").

Some parts of semantics, though by their nature non-formal, may, however, be approximated by syntactical means, by formal rules. Thus, the study of *interpretation*, which is a matter of the relation of expressions to their designata, can be substituted by the study of the relation of the expressions to the expressions of another (already understood) language. Despite this, there seems to remain some kind of semantic residuum that is ineffable in the sense of not being approximable by formal rules: it is clear that substituting translation for interpretation is possible only when the interpretation for the other language is taken for granted, and hence it does not give us any *ultimate* answer to the question of the nature of designation.

This indicates that Carnap's way of seeing things in LSL was closer to the syntax-as-approximation paradigm than to inferentialism. Carnap was obviously (though perhaps non-reflectively) convinced that there is some part of semantics that stands ineffable. That is to say, we can integrate some parts of semantics into syntax (by finding corresponding formal indicators), but there will always be a residuum.

Why should we think that syntax is only an imperfect approximation of semantics and not go for a wholesale inferentialism according to which semantics is nothing over and above that which is present in inferences? There seem to be especially two obstacles. First of them concerns empirical words, words like *dog*, *run*, or *black*. The semantics of these words, it would seem, cannot be a matter of inferences, or *merely* of inferences, it must reside in some relation to the world, a relation such as reference, representation, designation etc. Second, there is the problem diagnosed by Gödel and Tarski, which points out that syntax (axiomatics) faces its own limitations even before it reaches our empirical vocabulary: already within logic there are semantic phenomena resisting syntactic treatment.

Carnap, obviously, did not think about empirical languages very much and perhaps he took for granted that the empirical part is, from the syntactic viewpoint,

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[17] Carnap, *Introduction to Semantics*, *op. cit.*, pp. 8–9.

ineffable.[18] However, he could not avoid thinking about the other problem, and a part of LSL consists precisely in coping with it. And Carnap's method of coping, I think, is very instructive.

5.6 Incompleteness

In 1931, Gödel published his famous incompleteness proof.[19] He showed that however we set up axioms of arithmetic, there will always be an arithmetical sentence such that neither it, nor its negation will be derivable from them.[20] If we assume that either the sentence or its negation must be true, then it follows that there is a true sentence that is unprovable. It seems to follow that *provability* can never catch up *truth* – that it may act at best as its imperfect approximation.

Moreover, it would seem that the truth of the unprovable true sentence is fixed by the axioms of arithmetic. Indeed, the fact that the sentence is true follows from what it means (its meaning can be captured metaphorically as "I am not provable") and any meaning it has is conferred on it (more precisely on the terms it consists of) by the axioms. So there is a sense in which the truth of the sentence *follows from* the axioms, or is their *consequence*; but the sentence cannot be *derived* from the axioms. Hence it would seem that something similar to what we concluded about provability and truth holds also for derivability (or inferability) and consequence.[21]

An even more instructive case for consequence eluding inference was presented by Tarski.[22] He pointed out that whereas the conclusion *All natural numbers have the property P* is a consequence of the infinite set of premises of the shape *n has the property P*, it cannot be derived from it. (The reason, of course, is that it does not follow from any finite subset of the set of the premises.) And though this example may have controversial aspects, it vividly illustrated the difference between consequence and inference. Hence it would seem that the tools of Carnapian logical syntax – tools like derivations, inferences or proofs – are bound to stop short before capturing genuinely semantic terms such as truth and consequence – provability

[18] Hence we will also not discuss this problem here. I have done so elsewhere: Peregrin, *Inferentialism: Why Rules Matter*, *op. cit.*, Chap. 2.

[19] Kurt Gödel, "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I", in: *Monatshefte für Mathematik und Physik* 38, 1931, pp. 173–198.

[20] Jaroslav Peregrin, "Gödel, Truth and Proof", in: *Journal of Physics: Conference Series* 82, 2007. http://iopscience.iop.org/1742-6596/82/1/012006

[21] True, if we formalize arithmetic as a first-order theory, the unprovable sentence will *not* come out as a consequence of the axioms, and derivability will coincide with consequence. But this is just because first-order logic is *a priori* set up so that any consistent theory has a model – in other words, that consequence in it will duplicate derivability. This is different when we formalize arithmetic within second-order logic.

[22] Alfred Tarski, "O pojeciu wynikania logicznego", in: *Przeglad Filozoficzny* 39, 1936, pp. 58–68. English translation "On the Concept of Following Logically", in: *History and Philosophy of Logic* 23, 2000, pp. 155–196,

will never catch up truth (the *whole* truth, that is) just as derivability (or inferability) will never catch up consequence. And Carnap, already in LSL, was very well aware of this. What is his reaction?

Unlike in his later writings, where, influenced by Tarski, he goes for a construction of formal semantics, in LSL he tries to extend the possibilities of logical syntax to make it more inclusive and to bridge the Gödelian gap between syntax and semantics. In the case of his Language I the situation is quite straightforward – he considers a relaxation of the concept of *rule of proof* so that it would encompass the infinitistic omega rule:

$$P(1), P(2), P(3), \ldots \vdash \forall x P(x)$$

In contrast to many other logicians, Carnap was quick to grasp the point and consequences of Gödel's proof.[23] (It is remarkable, as Dawson documents,[24] how slow many very clever logicians were in comprehending this.) It was clear to him that the gap disclosed by Gödel is nonnegotiable – unless you go for a wider concept of derivation. And as the Tarskian example above suggests, one of the causes for inference lagging behind consequence is its inability to accommodate inferences with an infinite number of premises. So what if we make room for such kind of inferences?

On the positive side, it closes not only the Tarskian, but also the Gödelian gap. (It is well known that adding the omega rule dispenses with the incompleteness of arithmetic.) On the negative side, it stretches the concept of rule to encompass rules nobody is ever able to apply. But of course, this is the same with Tarskian semantics: saying that a sentence is true if it is satisfied by every member of an infinite domain is equally unusable in practice as saying that it is true if it is derivable from an infinite number of premises. We are limited in where we can get with rules in the strict sense of the word; we can get further with semantics, but only at the cost of the "get" becoming more virtual than real; and if we accept this virtualization of the "get", we need not abandon rules in favor of semantics, because we can do the same with a relaxed concept or rule.

5.7 Semantics vs. Generalized Rules

Coffa argues that Carnap's relaxation, in his Language I, of the notion of rule of inference in order for it to encompass the omega rule, "was apparently inspired by an incorrect diagnosis of the problem".[25] Coffa thinks that Carnap fell on the correct

[23] "Far from having been written in ignorance of Gödel's results, Carnap's LSL was inspired by an appreciation of the significance of Gödel's work that only a handful of logicians could match at the time," as Coffa (*op. cit.*, p. 286) puts it.

[24] John Dawson, "The Reception of Gödel's Incompleteness Theorems", in: Thomas Drucker (Ed.), *Perspectives on the History of Mathematical Logic*. Boston: Birkhäuser 1991, pp. 84–100.

[25] Coffa, *op. cit.*, p. 288.

solution only in the context of Language II - and the correct solution was something quite close to Tarskian semantics. Indeed Tarski himself claims that this definition "cannot be extended in a natural way to other less elementary languages."[26]

Why did Carnap abandon his solution based on the relaxation of rules of inference? Coffa writes: "Carnap never explained the reason for this change of strategy, but one may conjecture that at some point he realized that the first technique had worked for the case of Language I only because of the weakness of its expressive power."[27] But why would it not work for the other languages? Coffa first cites Tarski:

> The profound analysis of Gödel's investigations shows that whenever we have undertaken a sharpening of the rules of inference, the facts, for the sake of which this sharpening was felt to be necessary, still persist, although in a more complicated form, and in connexion with sentences of a more complicated logical structure. The formalized concept of consequence will, in extension, never coincide with the ordinary one.[28]

This is certainly true; but from the current viewpoint it is irrelevant. The point is that allowing for the omega rule cannot be considered as "sharpening" in this sense – on the contrary, it is, as it were, "blunting". The omega rule is not a rule that can enter what we call *proofs* in the strict sense of the word; and precisely for this reason it is eligible to close the gap between inference and consequence.

But there were more concrete reasons for Carnap abandoning his attempts at a "syntactic" treatment of consequence in favor of a "semantic" one. These are connected with the fact that Language I contained merely first-order quantification (especially quantification over the domain of natural numbers). Language II contained also higher-order quantification. And if we consider quantification over predicates, Coffa is convinced, we cannot find a solution analogous to the addition of the omega rule.

Why not? There are two reasons, one more general and one more specific. The general reason is that there may be universes with nameless individuals. In the case of Language I, the universe contained only natural numbers, each of which had a name – the corresponding numeral. And it seemed that it was only this fact that allowed the omega rule to do the work it did – for if the universe were to contain, aside of all the normal natural numbers, one more object which would have no name, then it would not seem right to derive $\forall x P(x)$ from $P(0)$, $P(1)$, $P(2)$, ..., and there would be no way of adding a premise to make it work.

The more specific reason is that once we move to predicative quantification, then there are *bound* to be nameless objects in our domain. Indeed, the number of subsets of the set of natural numbers is uncountable, and hence there must be uncountable number of potential denotations for predicates, and yet we can only have a countable amount of names. So in this particular case no generalized rule tantamount to the omega rule seems to be available.

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[26] Tarski, "On the Concept of Following Logically", *op. cit.*

[27] Coffa, *loc. cit.*

[28] Alfred Tarski, *Logic, Semantics, Metamathematics*. Oxford: Clarendon Press 1956, p. 295.

There is no doubt that Carnap did abandon his thoughts about generalized rules in favor of semantics and that it was roughly for the reasons sketched by Coffa. However, our question now is whether his change was inevitable (and if not, whether it was reasonable), or whether Carnap could have continued on his early inferentialist track, avoiding Tarskian semantics and anticipating current inferentialism. And my view is that indeed he could.

Consider the problem of nameless individuals. How do we know that we have a universe with such individuals? According to Tarskian semantics, the specification of the universe is part of the specification of the language. But in real life, we do not and cannot (perhaps with some marginal exceptions) do this. We establish what we are talking about by means of talking (with the marginal exception of ostension, which, however, does not play a role in the case of sets of numbers). Thus, the fact that we do not want to infer $\forall x P(x)$ from $P(0)$, $P(1)$, $P(2)$, ..., cannot be seen as *a consequence of* the fact that our universe of discourse contains an object over and above the natural numbers, but rather as *constitutive of* the fact – saying that $\forall x P(x)$ is not inferable from $P(0)$, $P(1)$, $P(2)$, ... is *nothing else* than saying that there is a surplus object in the universe.[29]

What, then, about the domain of sets of individuals that are to interpret unary predicates in Language II (plus all the still more complex domains "above" it)? Clearly not all such sets can have names, at least not within a fixed, countable language. Thus we cannot have rules analogous to the omega rule. But how much does Tarskian semantics help us with this? Seemingly, it tells us when any sentence of the form $\forall x P(x)$ is true, namely when $P(x)$ is satisfied by every individual of the universe. However, this is to say that we can conclude $\forall x P(x)$ if we have $P(x)$ *is true of* i, where i runs through all the elements of the relevant domain. And this, of course, is nothing else than an analogue of a generalized omega rule, only with metalinguistic premises (the number of which equals the cardinality of the domain – and thus, in the case of an uncountable domain, it is uncountable). Hence it is not clear why this would be any better than a straightforward generalized omega rule with premises taken from the object language, rather than the metalanguage (though this would require adding the requisite names to it).[30]

[29] Moreover, it seems that natural languages, though they certainly do not contain names of everything that is, potentially, in their universe of discourse, incorporate mechanisms that allow for creating such names (and indeed it would seem that *to be in the universe* is *to be a potential referent of such a name*).

[30] See Philippe de Rouilhan, "Carnap on Logical Consequence for Languages I and II", in: Pierre Wagner (Ed.), *Carnap's Logical Syntax of Language*. Basingstoke: Palgrave 2009, pp. 121–146.