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Verification: The Hysteron Proteron Argument

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This paper investigates the strange case of an argument that was directed against a positivist verification principle. We find an early occurrence of the argument in a talk by the phenomenologist Roman Ingarden at the 1934 International Congress of Philosophy in Prague, where Carnap and Neurath were present and contributed short rejoinders. We discuss the underlying presuppositons of the argument, and we evaluate whether the attempts by Carnap (especially) actually succeed in answering this argument. We think they don't, and offer instead a few sociological thoughts about why the argument seems to have disappeared from the profession's evaluaton of the positivist criterion of verifiability.

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1. Introduction

We team-taught a Philosophy of Language course in Fall 2015 using (in addition to other works) Lycan (2008) as a textbook. We were struck by this "Objection 2" that Lycan raised against verifiability theories of meaning:

> Suppose we look at a given string of words, and ask whether or not it is verifiable, and if so what would verify it. In order to do that, we already have to know what the sentence says; how could we know whether it was verifiable unless we knew what it says? . . .
>
> But, if we already know what our sentence says, then there is something that it says. And to that extent, it already is meaningful. Thus, the question of verifiability and verification conditions is conceptually posterior to knowing what the sentence means; it seems we have to know what a sentence means in order to know how to verify it. But that is just the opposite of what the Verification Theory says. (Lycan 2008, 101)

This prompted us to investigate the provenance of this objection, which we had not heard before, and we discovered that its history does not seem to have been systematically investigated. This paper is an account of what we've found.[1]

[1]Thanks go especially to Bill Lycan for his encouragement to undertake this effort. Although he had said that "So far as I know the objection is all mine", he encouraged us to investigate, saying "I'd be delighted to hear that Susanne Langer or someone had said it."

2. The History of the Argument

The most explicit version of this objection seems to be in Isaiah Berlin's (1939), which is in the *Proceedings of the Aristotelian Society*, and thus one supposes, came into the consciousness of the British philosophical community that year. Perhaps the War took philosophers' minds off that topic, since it seems quite difficult to locate in the 1950s–1960s, and indeed even in the present. In any case, we use Berlin's description as our name for the argument:

> The most obvious objection to this doctrine [of a Verificationist theory of meaning], which critics were not slow to urge, was that this formulation involved a glaring *hysteron proteron*; for before I could think of possible ways of verifying a given statement I first must know what the statement means, otherwise there could be nothing for me to verify. How can I ask whether a group of symbols asserts a truth or a falsehood if I am not certain of what it means, or indeed whether it means anything at all? Surely, therefore, understanding what the sentence means—what proposition it expresses—must in some sense be prior to the investigation of its truth, and cannot be defined in terms of the possibility of such an investigation—on the contrary the latter must be defined in terms of it. (Berlin 1939, 228)

Berlin thinks of this as an "obvious objection", although he does not cite any names of the "critics [who] were not slow to urge" this objection. As we mentioned, it is difficult to find this objection mentioned in the standard (even the sophisticated!) textbook discussions of the verifiability principle. It is not mentioned in Soames (2004) for instance, and as we mentioned above, was thought by Lycan to be a new comment on the principle. But while there maybe were not enough published works to justify calling this "the most obvious objection", there were in fact a few writers in the British and American journals of the late 1920s–1940 who urged this objection, or something similar to it.[2] (Even

[2]We went through the (American) journals *Journal of Philosophy* and *Philosophical Review*, as well as the (British) journals *Mind, Analysis, Proceedings of the Aristotelian Society*, and *Proceedings of the Aristotelian Society* supplementary

though these voices seem not to have seeped into the consciousness of more modern analytic philosophy.)

The earliest version that we have discovered is by the American pragmatist C. I. Lewis in his book *Mind and the World Order*:[3]

> We must first be in possession of criteria which tell us what experience would answer what questions, and how, before observation and experiment can tell us anything.[4] (Lewis 1929, 259):

Additionally, we have discovered a few other occurrences of what might be the *hysteron proteron* argument, although some of these few—especially the Russell and Lazerowitz ones—require a bit of "charity" or a previous "sensitization" to that argument, to see it as present in them. Perhaps the quotations from MacDonald and Schlick below will strike one almost as strongly as the just-given version of the *hysteron proteron* argument in Berlin, but the others are admittedly a stretch. We give further discussion of these quotations below in §4.

> What always puzzles me on this view is, how do I know what experiences will verify what propositions? I must be able to recognize the brownness of the table, and that depends on past experience. Unless I first know what to look for, how can I verify anything at all? . . . I must know what experience would verify my propositions; I must first understand them before I can prove them true. They would seem, then, not to be identical procedures. (MacDonald 1934, 145–46)

> You cannot even start verifying before you know the meaning, *i.e.*, before you have established the possibility of verification.[5] (Schlick 1936, 349)

> Mr. Ayer refuses to discuss the problem of meaning, but in the absence of some discussion of this question it is difficult to see how he can know that a form of words "records an observation". Does he know anything about the occurrence except the form of words? If not, how does he know that the words describe the occurrence? If yes, what is the nature of this non-verbal knowledge? And when some empirical proposition is verified by an occurrence, what is the relation between the occurrence and the proposition, and how is it known? (Russell 1936, 543)

> Obviously the philosopher's hearer will know "what he is telling him" only if he *understands* the sentence *s* that the philosopher is using, which of course entails that *s* has literal meaning. (Lazerowitz 1938, 36)

Other than these comments, it is very difficult to find the *hysteron proteron* argument in the British and American journals of the 1930s time frame.

3. What is the *Hysteron Proteron* Argument?

Suppose you are an underwater cave diver exploring some caves near Karaginsky Island off the Kamchatka coast. As you surface into a large underground cavern and pull yourself up onto a rocky bench, you see some sort of scratch-marks on the walls. You think they look out of place in such a cavern located in such an untrodden area, and you wonder if some early inhabitant of the area perhaps found a way into this now-hidden place and scratched some message on the walls . . . perhaps "Zhdan

volumes. We list here the few that we found. Although there were plenty of articles that discussed the verifiability principle, no other ones appear to mention this particular concern.

[3]The connection to Pragmatism forms a separate strand in the longer version of our work. Lewis had various thoughts about the *Hysteron Proteron* argument, some in his (1934) and many further in his (1941).

[4]Lewis was here talking about the Pragmatist's view according to which it was their connection to, or relations with, experiences that was relevant to the meaning of sentences, rather than the Postivist's account, which relies on (a suitable sense of) "observation".

[5]We discuss this article in a somewhat more broad setting in our concluding section, §12. This article is, among other things, Schlick's reaction to Lewis (1934), presumably trying to show a similarity between Positivism and Pragmatism in this regard. We give some further details about the relationship between these two papers (and also Carnap 1936–37) in our concluding section, and also discuss it further in §4.

was here" in some unknown language. But as you look closer you can see no incontrovertible evidence of a regularity in the scratches and you begin to consider the hypothesis that it is just an unusual, but naturally created, marking on the wall, and not a sentence of any language. As can be seen, the problem set is one of determining whether some physical object is an instance of language. So we call this the "language vs. some 'natural' process problem."

In some works the question is posed with more detail by, for instance, imagining that the wind sweeps sand off a beach and deposits in the form of some perfect English inscription. Here the question is whether such an inscription is or is not an English sentence. The general consensus is that this would not in fact be a sentence of English, and is usually thought not to be because its provenance or causal history does not include an intelligence that intended to make such a sentence. The literature also contains alleged cases of spoken utterances of sentences that are ambiguous as to which language they belong to, because they sound the same in each language. It is usually thought that the intentions of the speaker determine which language the sounds belong to, and that it is *not* a sentence of the other language despite the fact that it could be understood as such by a listener of that language who does not know the relevant facts about intentions. In the literature surrounding some discussions of verification, this is put as the question about writing: "How can we tell whether a certain collection of mounds of ink is a sentence *of language X*?" (Where the *X* is known, unlike the earlier question of whether it is in a language at all.) We will refer to this problem as "the mounds of ink problem". This is a subtype of the "language vs. some natural process problem", but where the indistinct scratches are replaced with what to all appearances seems physically to be identical to a sentence of some particular language and is recognized as such.

Once the mounds of ink are classified as a particular sentence of some language, the question arises as to what that sentence means (in that language). This is a problem raised by very many theorists over the decades surrounding our period of interest. Ramsey (1923, 468ff.), for instance, takes Wittgenstein to be discussing the relation between a propositional sign and a thought, and explicates it with the help of Peirce's type/token distinction. "A *proposition* is a type whose instances consist of all propositional sign tokens which have in common, not a certain appearance, but a certain *sense*." And Lewis (1929; 1934) holds that it is the memory of past experiences that give meaning to present sentences, or rather to the words that make up the sentences. We call this the "meaning determination problem."

Ramsey also remarks that if we can answer the question of what it means for a propositional token to have a certain sense, then "we incidentally solve the problem of truth; or rather it is already evident that there is no such problem. For if a thought or proposition token '*p*' says *p*, then it is called true if *p*, and false if $\sim p$." And as might be gathered from Ramsey's understanding of Wittgenstein, he might also think that we are just "shown" what a sentence means, and so there is no problem of how we come to recognize it as having a certain meaning, or truth conditions.[6]

We sum this all up by indicating where it is in all this that we see the *Hysteron Proteron* argument being situated. We say that in going from scratches on the wall to knowledge of what is involved with a sentence's surroundings, there are at least four discernible stages (discernible at least by analysis even if they might merge into one another in any particular case). And the analysis is directional: each later stage presumes that the former stages have been decided. (Of course, in a normal course

[6]It is an interesting question whether Ramsey's theory of truth in his later writings (Ramsey 1990) can deal with the issue of the *Hysteron Proteron*, although this is outside the ambit of the present paper, since he didn't *explicitly* mention or say anything that would clearly deal with the argument. For a general survey of Ramsey's position on truth, with some possible connections to the present topic, see Rumfitt (2011), which discusses many of Ramsey's works from the period (collected in Ramsey 1990, 1991).

of events the stages (1) and (2) are just presumed to be common knowledge. But perhaps it would be instructive to follow through the full list in some less obvious situation.)

(1) How can one determine whether a physical manifestation is a piece of language? (The Language vs. Natural Process problem.)
(2) Having determined that it is a piece of some language, how does one determine *which* piece of *what* language it is? (The Mounds of Ink problem.)
(3) Having determined that it is some specific piece of a particular language, how do we determine what that piece means? (The Meaning Determination problem.)
(4) Having determined what the piece means in the language, how might we determine a truth value for it? (The Verifiability problem.)
(5) Having determined the truth conditions, how should we determine whether it is or isn't true? (The Truth problem.)

The rhetorical or "logical" fallacy that is classically called *hysteron proteron* is where the order of some accounting of events or of a progression of reasoning gives a mistaken statement of the actual or logical order. Thus "We heard and saw the lightning bolt" might be accused of a *hysteron proteron* because the order of description of the events does not match the order of the natural events. A circular argument might be accused of a *hysteron proteron* because the parts cannot be put into a linear order. Berlin called the verification theory of meaning a *hysteron proteron* because he found that the claim that the meaning of a statement was its method of verification[7] did not follow the natural order of needing to understand the sentence *before* being able to verify it.

So in our list of stages, the Berlin version of the argument is that the Verifiability Theory puts (4) either before or at the same (logical) time as (3)... out of order with the logical stream of events (and perhaps also with the temporal course of nature). But this leaves open the possibility of other *hysteron proteron* arguments about the entire procedure. For example, perhaps some theory claims to be able to determine the actual truth or falsity of a sentence without having to first determine its truth conditions. Or for another example, it might be thought by some theorist that one can determine the truth value of a sentence independently of its verifiability, by finding that it is written in a revered work, and thus it must be true—whether or not we can verify it. (Or indeed, regardless of what it means.)

The usual understanding of a Verifiability Criterion of Meaning (as for instance in Ayer 1936, 1946) is to merge steps (3) and (4), denying that one "comes before" the other in temporal or conceptual priority. If that merger can be substantiated, then the *hysteron proteron* argument of course will have no force. It is against the possibility of this merger that the Lycan, Berlin, and MacDonald versions take the time to add "we have to already know what the sentence says" (Lycan), and "I must first know what the statement means" (Berlin), and "I must know what to look for [before] I can verify anything" (MacDonald). So, a part of the positivist rebuttal of the *Hysteron Proteron* argument must also show that these claims are false.

In our discussion below we also describe Carnap's contention that not only are (3) and (4) conceptually contemporaneous, but also they are contemporaneous with (5) and also with (2). Our overall claim is that Carnap does not make good on these conceptual identifications, at least not during the period that we are discussing. The question of whether a move to a "holistic" picture of verifiability as confirmability of a theory "as a whole" (as the later Carnap and the other later positivists are usually seen as advocating) *will* allow such an identification is something that is beyond the scope of this paper.

Carnap often demands that "the metaphysician" be required to "state what observational consequences the ⟨offending term

[7] Or modifications to account for weaker claims such as "is meaningful if it is verifiable".

that is being introduced⟩ has". But while that may carry the day against someone who adds a new word to existing vocabulary, it does not tell against ordinary words that are already in use in ordinary language. Consider a sentence made up exclusively in that manner, for example "That colourful butterfly used to be an ugly brown pupa". How is this sentence to be verified? Well, you find that butterfly and (try to) trace its history; or perhaps you fall back on general knowledge of the history of individual butterflies; or maybe you cite some statements about the developmental trajectory of butterflies. And there can be arguments as to which way is best as a method of verification for the sentence.

But it needs to be asked: why do you do any of *those* actions, in preference (say) to looking at a nearby wall to see its colour, or continue to watch the butterfly to see if it becomes a hummingbird, or any of an infinity of other possible actions? The answer is that you *already* know the meaning of the sentence. What you do not know is its truth value. You have already passed step (3) in our ordering above and are now investigating steps (4) and (5). To insist that verifying the sentence in one way is better than some other way without knowing its meaning already, is to commit the *hysteron proteron* fallacy.

4. Some Comments on the Previous Quotations

Immediately after the quotation we gave from Berlin (1939), he continues with "But this objection is not as formidable as it looks." However, we find his remarks that are intended to show this not to be very convincing at all. He says:

> A supporter of the theory may reply that what he means by the expression "to know the means of the verification of p" is knowing in what circumstances one would judge the group of symbols "p" to convey something which was or was not the case; adding that what one means by saying that one understands a given sentence, or that the sentence has meaning, is precisely this, that one can conceive of a state of affairs such that if it is the case—exists—the sentence in question is the proper, conventionally correct description of it, i.e., the proposition expressed by the sentence is true, while if it is not the case, the proposition expressed is false. To understand a sentence—to certify it as expressing a given proposition—is thus equivalent to knowing how I should set about to look for the state of affairs which, if the state of affairs exists, it correctly describes. To say that a sentence is intelligible, i.e. that it expresses a proposition, without specifying what the proposition is, is to say that I know that I could set about to look for the relevant situation without saying what kind of situation it is. It follows that any sentence such that I can conceive of no experience of which it is the correct description, is for me meaningless. (Berlin 1939, 228)

This seems inadequate in two different dimensions. First, it seems to be vacillating between claiming *to know* what circumstances lead one to judge p to be true and which to judge it false, and *to conceive* circumstances such that if they be the case then the sentence is the correct description of those circumstances. Of course, under the usual conditions where one *knows* something, then it is true. So *knowing* what circumstances would lead one to judge it true or false simply begs the question. If you don't understand the sentence, how can you *know* what the "proper" and "relevant" circumstances are? Secondly, there seems to be a further vacillation between a sentence's being *the conventionally correct description* of a given or imagined state of affairs and the sentence's being *meaningful for me*. We think the latter notion—being meaningful *for me*—is not in accord with the general outlook of the positivists. They would not be content with allowing metaphysicians to claim that "The Absolute apprehends Being" is *meaningful for them* because they can conceive of the relevant experience even if others can't. The discussions in the positivist literature about such sentences do not conclude that such sentences are "meaningful for the metaphysician but not for us". They conclude with "they are not meaningful."

Additionally, we might also repeat our claim made at the end of §3: if you do not know the meaning of a sentence, you will not

know what experiences are confirmatory and which are disconfirmatory for that sentence. You will not know whether to watch a nearby wall to see if it changes colour in order to confirm "That colourful butterfly used to be an ugly brown pupa." Nor will you be able to say that the butterfly's flitting away is irrelevant to the meaning of that sentence. At least, not if we are talking—as we wish to be—about "the proper, conventionally correct description of [the state of affairs] expressed by the sentence" if it is true.

The particular occurrence in Lewis (1929) that we cited in §2 of what appears to be the *Hysteron Proteron* argument is actually directed in a different direction than that of Berlin and Lycan, as well as being different from some of Lewis's later ruminations, especially in Lewis (1941). In the 1929 context, Lewis's concern was to distinguish pragmatism from "scientism"; in particular he is concerned to show that science requires a careful inventory of meanings and concepts before it can proceed. As an example of his worry, immediately prior to the quote given in §2, he says:

> However much the give and take between the purposes of science and discovered fact may contribute to alter the procedure by which those aims are sought, and may induce new basic principles and categories, still the naming, classifying, defining activity is at each step prior to the investigation. We cannot even interrogate experience without a network of categories and definitive concepts. (Lewis 1929, 259)

Despite this apparent focus away from the *Hysteron Proteron*, we mention Lewis's statement because it seems to have been the impetus for Margaret MacDonald's (1934) remarks that we cited just afterwards and will discuss next. Furthermore, as we remark in footnote 34 below, Lewis's book was also the impetus for a course that Berlin and John Austin taught together in the late 1930s, and might therefore have had some causal influence on Berlin's (1939) account that we quoted in §2.

The goal of MacDonald's paper is stated in its first sentence: "to discuss one or two points arising out of the view held by certain modern philosophers that the whole meaning of a proposition is given in a set of conditional propositions about the experiences which would verify it." It is clear that she is concerned with "the doctrine of the Vienna Circle" and cites lectures given by Schlick in London during 1932. On the other hand, she regularly cites works of Peirce in her exposition of the doctrine, apparently thinking that there is a strong similarity between Peircian pragmatism and the then-current Viennese positivism. We should also remember that Lewis's pragmatism was also one of the "modern doctrines" of the era. The portion of the article immediately prior to the sentences we quoted in §2 sounds very much as though it is directed at pragmatism as well as positivism. Additionally, the last two-thirds of the paper concerns Lewis and pragmatism almost exclusively. Just prior to the quotation in §2 she says :

> . . . to say that I know the "meaning" of "*S*" is simply to say that I know what "*S*" is being used to represent. And if I know what "*S*" is being used to represent I understand "*S*." I understand a proposition when I am acquainted with those objects (including properties) which enable me to "construct" the situation which would make the proposition true and to recognize this situation if presented. When the proposition is true, *i.e.*, whether an arrangement of objects such as I have "constructed" does in fact exist does not depend on my understanding of the proposition. To discover whether the proposition is true I must look for the relevant evidence. That is to say, I must justify or "verify" the proposition. Thus truth and falsity depend upon what there is in the world, while understanding and verification depend upon my knowledge of what there is in the world. But for the philosophers we are discussing, the meaning of a proposition *is* the method of its verification. . . . When I say that "this table is brown," what I mean (or, part of what I mean) is that if I get into the appropriate situation I shall see a brown expanse which I regard as part of the table's surface, and the word "brown" is a *prescription* for the performing of this experiment on every occasion of its use. . . . [These experiences] will all relate to a time future to that at which I make an assertion about the table. (MacDonald 1934, 144–45)

It is in this context that MacDonald cites the remark from Lewis that we quoted above. She continues the article by remarking on (what she takes to be) the "most curious" feature of this view: "the position of propositions about that past which is beyond the reach of living memory. For what future action or experience of mine can constitute the *whole* meaning of such assertions as 'Queen Elizabeth died in 1603'? or 'The plesiosaurus once lived upon the earth'?"

As can be seen from an inspection of the literature in the British philosophy journals of the 1930s and later, the issue of "truth about the past" loomed large in the argumentation about the principle of verification. And the remainder of MacDonald's article (148–56) is mostly a critique of Lewis and the pragmatist theory of meaning. (Much of it arguing that pragmatism has the same flaws as the positivist theory, at least in regard to verifying claims about the past.) She concludes her article by emphasizing the similarities between these two theories, and hoping "to find some interpretation of propositions about the past which . . . would avoid some of the paradoxes which seem involved in the positivist-pragmatist view."

It may strike one as ironic or in some other way peculiar for us to cite Schlick as presenting the *Hysteron Proteron* argument, since, after all, isn't this supposed to be a consideration *against* positivism? And isn't Schlick the head of the Vienna Circle? We will discuss the circumstances surrounding Schlick (1936) in §12, but we should say a few words here to explain the present peculiarity. Although we see the Schlick material quoted above as a clear instance of the *Hysteron Proteron* argument, of the type that Lycan and Berlin put forward, this was not the focus Schlick himself put that consideration towards, at least not at this point in the article.

Schlick (1936) is a response to Lewis (1934), and to a large extent addresses a particular difference between (what Schlick takes to be[8]) positivism and (what some have thought to be) pragmatism on the issue of meaning. Schlick says, shortly before the quotation we cite in §2:

> Any judgment about empirical possibility is based on experience and will often be rather uncertain; there will be no sharp boundary between possibility and impossibility. . . . Is the possibility of verification which we insist upon of this empirical sort? In that case there would be different degrees of verifiability, the question of meaning would be a matter of more or less, not a matter of yes or no. In many disputes concerning our issue it is the empirical possibility of verification which is discussed; the various examples of verifiability given by Professor Lewis, *e.g.* are instances of different empirical circumstances in which the verification is carried out or prevented from being carried out. . . . A proposition is presented to us ready made, and in order to discover its meaning we have to try various methods of verifying or falsifying it, and if one of these methods works we have found the meaning of the proposition . . . If we really had to proceed in this [empirical] way, it is clear that the determination of meaning would be entirely a matter of experience, and that in many cases no sharp and ultimate decision could be obtained. How could we ever know that we had tried long enough, if none of our methods were successful? Might not future efforts disclose a meaning which we were unable to find before?
>
> This whole conception is, of course, entirely erroneous. It speaks of meaning as if it were a kind of entity inherent in a sentence and hidden in it like a nut in its shell . . . [A] proposition cannot be given 'ready made'; meaning does not inhere in a sentence where it might be discovered, but [rather] it must be bestowed upon it. And this is done by applying to the sentence the rules of the logical grammar of our language . . . These rules are not facts of nature which could be 'discovered', but they are prescriptions stipulated by acts of

[8]Since Schlick's article is a bit later than the positivism of the period we are generally concerned with (1930–1935), it is not so clear to us how closely his account here follows that of Carnap in the same period. Carnap's own writings changed rather strikingly with the appearance of his (1936–37). Schlick (1936) was published in the July 1936 issue of *Philosophical Review*—shortly after Schlick's murder by Johann Nelböck on 22 June 1936. The most detailed account of this assassination is Stadler (2001, part 2, secs. 3–3.2).

> definition. And these definitions have to be known to those who pronounce the sentence in question and to those who hear or read it. Otherwise they are not confronted with any proposition at all, and there is nothing they could try to verify, because you can't verify or falsify a mere row of words. (Schlick 1936, 348–49)

We see here a clear distinction being drawn between two methods of "verification"—thought by Schlick to distinguish positivism from some versions of pragmatism.[9] A problem that Schlick seems at this stage to be identifying with (some forms of) pragmatism is an over-reliance on "empirical experiences" in determining verifiability, and this would lead to meaning being a matter of degree—and hence a distinction between analytic "matters of meaning" and synthetic "matters of fact" being impossible. It is in this context that Schlick puts forward the claim that "you cannot even start verifying before you know the meaning, *i.e.*, before you have established the possibility of verification". And he concludes this part of his discussion with "the possibility of verification which is relevant to meaning cannot be of the empirical sort." When put like this, these claims are not the *hysteron proteron* argument.

A study of Schlick's remarks in the initial section of his article, however, brings forth considerations that seem more closely aligned with the *hysteron proteron* argument. In that section Schlick remarks that there is a distinction between a *sentence* as "a mere sequel of sounds or a mere row of marks on paper" (339) and a *proposition*, which *is* a meaning. Since propositions are by definition meaningful, it follows that, as Schlick (1936, 340) puts it: "We cannot inquire after the meaning of a proposition, but we can ask about the meaning of a sentence", when the sentence is seen as a sequel of sounds or marks on paper. And Schlick then remarks that it is propositions that are primarily subject to verification: it amounts to being told the conditions under which that proposition would be true and those that would make it false. One can see, then, that if we wanted to apply verification to *sentences*, that would be in a secondary sense. We would ask for "the conditions under which that sequel of sounds or row of marks will form a true proposition and the conditions where it would form a false proposition." This is at least very close to what we identified as the *hysteron proteron* argument that Lycan and Berlin describe, and we characterized in §3. For, the argument would become the issue of determining what the proposition is, which is to be verified, without already knowing the verification conditions.

However, Schlick then remarks that one could merely be told, in a language we already understand, the circumstances when such sentences were affirmed or denied—some sort of quasi-empirical description of the proposition that such a sentence is normally seen as expressing. But we can see that this conception of how one can learn the proposition expressed by a row of words doesn't really solve the *philosophical* issue underpinning the *hysteron proteron* argument, but instead only pushes it back one step, to apply to this new sequel of sounds that are supposed to describe what the verification conditions are for the first row of words.

But Schlick does tell us another way—one that is more suitable for philosophical discourse, he says. It is "by indicating the logical rules which will make a proposition out of the sentence, *i.e.*, will tell us exactly in what circumstances the sentence is to be *used*." He adumbrates this view in the opening section of this article (which he also says Lewis agrees with), where it is claimed that if all the words of a language are given a meaning, we will be in a position to determine whether any sentence using them expresses a proposition. Of course, this requires a lot of inferencing to be done along the way, as Schlick concedes, and one can be unconvinced that this can always be relied on. Schlick (340–41) gives the example of "a sky three times the blue as in

[9] But in the next paragraph Schlick says that Lewis's version of pragmatism agrees with positivism on this issue, citing Lewis (1934, 142). We find it rather difficult to determine whether this citation actually confirms this claim of Schlick's.

England", saying that "the word 'blue' is used in a way which is not provided for by the rules of our language. The combination of a numeral and the name of a color does not occur in it . . . ". It is both a problem that it is probably impossible to give such a list of conditions for an entire language, and also a problem that sentences like "This diode will emit three different blues" are perfectly fine. Note, however, that this picture of how a "row of words" gets its meaning presumes knowing features of a metalanguage, which one might argue will give rise once again to a *hysteron proteron* argument at a different level.

This conception is very similar, if not identical to, that proposed in Carnap (1931–32).[10] We will consider its appearance in that work below, in our §9. We also consider whether such a view can be adapted more generally, so as to apply to "unreconstructed" natural language, in §10.

As we remarked in §2, it is somewhat more difficult to see the *hysteron proteron* objection in the quotations from Russell (1936) and Lazerowitz (1938), but perhaps we can do some "charitable addition" to make it stand out better. When Russell asks, ". . . how can [Ayer] know that a form of words 'records an observation'. Does [Ayer] know anything about the occurrence except the form of words? If not, how does he know that the words describe the occurrence?", perhaps he is charging the verification principle of having no way to understand anything about the words, and in particular not knowing whether they "record an observation". This sounds more like the *Hysteron Proteron* argument.

Lazerowitz likewise can be made to put forth the *Hysteron Proteron* argument, if one thinks of "the philosopher" who is being discussed as one who puts forward (what a positivist would call) a typical metaphysical statement. A hearer can understand this claim, but not be able to thereby be able to determine its truth or falsity. Lazerowitz puts this as saying that sentences have "a literal meaning" which is prior to and independent of any determination of truth or falsity.

5. A Strange Fact about the Second Edition of Ayer's *Language, Truth and Logic*

As is well-known, Ayer's *Language, Truth and Logic* was initially published in 1936, and according to Ayer's autobiographical sketch (1992), he was encouraged to do so by his friend Isaiah Berlin. The article we quoted the *hysteron proteron* argument from above, Berlin (1939), also contained another criticism of Ayer's book (and of Ayer's version of the verifiability principle generally):

> If I say
>
> > This logical problem is bright green.
> > I dislike all shades of green.
> > Therefore I dislike this problem.
>
> I have uttered a valid syllogism whose major premise has satisfied the definition of weak verifiability as well as the rules of logic and grammar, yet it is plainly meaningless. . . . No criterion that is powerless in the face of such nonsense as the above is fit to survive. (Berlin 1939, 234)[11]

Many philosophers know that in the Introduction of the second edition of *Language, Truth and Logic*, (Ayer 1946), Ayer crafted a response in terms of direct and indirect verification specifically to address this criticism of the verifiability principle. But as many philosophers also know, in Alonzo Church's review of

[10] This work is sometimes cited as "Carnap (1931)" and sometimes as "Carnap (1932)". Benson (1963, 1023), whose bibliography of Carnap's work considers it to be a 1932 publication, explains thusly: "Volume 2, Number 4 of *Erkenntnis* was undated and appeared in 1932. However, the date given on the title page of Volume 2 was 1931." In Ayer's bibliography of logical positivism, Ayer (1959b, 400–01), it is listed as "1931–32", and that is what we follow.

[11] Well, strictly: disliking the *colour* green does not imply disliking *things* that are coloured green. But presumably Berlin's argument could be slightly reformulated so as not to exhibit this particular flaw.

the second edition (Church 1949), this more intricate definition of verifiability was also shown to admit every statement into the realm of meaningfulness (so long as there are three logically independent observation statements[12]). Ayer also spent some considerable time in this new Introduction responding to the question of whether the verifiability criterion applies to sentences or to propositions, citing Lazerowitz (1939) as its inspiration. His response was to interpose the notion of "statements" between sentences and propositions, and have the criterion apply to them.

It does not seem that Ayer in any way attended to the *hysteron proteron* objection in the new Introduction. This is very strange, since it is not only a strikingly plausible objection but also it appeared only some four pages before the "green logical problem" objection in Berlin (1939)—the argument that Ayer spent so much effort on. Perhaps he was convinced—unjustifiably, in our opinion, as we said in §4—by the considerations that Berlin had put forward after presenting the *hysteron proteron* argument.

6. "The" Verification Principle

Descriptions of a verification principle and also the nature of the presupposed protocol sentences underwent serious examination by the Vienna Circle during the period 1930–1935 (and yet further changes came in the decades following (e.g., Carnap 1936–37, 1950, 1956). So, stating a definite version of the principle from this period is not really possible. However, we can state some definite parts of the viewpoint that Carnap held through this 1930–1935 period, even if not all the members of the Vienna Circle held to all of these points. And even though Carnap himself changed his position a little later.

First, the principle was to apply only to the issue of "cognitive meaning" (or perhaps it might be better thought of as *defining* what cognitive meaning was).[13] In addition to cognitive meaning there was thought to be other types of meaning, the most well-known being "emotive meaning", expressed in Ayer (1936, chap. 6), and elaborated upon in the new Introduction of Ayer (1946, 20–22). As Ayer remarks in the latter location, it was later developed more fully in Stevenson (1944). In Ayer's intellectual autobiography (Ayer 1992), he mentions the "classical problem" of the Verifiability Criterion not being either analytic or synthetic, and says that in response to this "embarrassing issue" he would claim that it was a *proposal*, and would challenge the questioner to come up with anything better. Apparently then, proposals have some sort of meaning other than cognitive meaning. Also, Carnap allows that there can be images and feelings that are associated with some words by some people, but he also claims that these do not bestow any meaning on these words (Carnap 1931–32, 67). And it is conjectured (Carnap 1931–32, 78) that perhaps metaphysical statements "serve for the expression of the general attitude of a person towards life", but again this is said not to be any "real meaning". Art, and more specifically music, is accorded the best and purest way of expressing one's attitude towards life. ("Metaphysicians are musicians without musical ability"; 1931–32, 80.)

Second, sentences (as well as words) that are cognitively meaningful are meaningful in this way because they are deductively related to "protocol sentences". That is, such a meaningful sentence will in some deductively-specifiable way entail one of these "primary sentences". However, as Carnap says (1931–32, 63), "the question concerning the content and form of the primary sentences (protocol sentences) . . . has not yet been definitely settled." He continues by saying that some positivists hold that primary sentences refer to "the given", although there is no unanimity as to what is given; others hold that these primary

[12] And modulo the cases described in Lewis (1988).

[13] We offer a way of accommodating the idea that cognitive meaning could be seen as a subtype of meaning-in-general in §11.

sentences "speak of the simplest qualities of sense and feeling (e.g. 'warm,' 'blue,' 'joy')", but others think of them as referring to "total experiences and similarities between them". And some think of them as directly describing "things".

Third, and as implicit in the second point, the fundamental unit of consideration is the *individual sentence*. It is an individual sentence that entails one (or many) protocol sentences. It is true that individual words also have meanings, but these are discovered by consideration of open formulas: Carnap's example (1931–32, 63) is "arthropode", which is given a meaning by the open formula "the thing x is an arthropode", which in turn yields protocol sentences such as "x is an animal", "x has a segmented body", and the like. It is only later that the positivists took on board a more "holistic" theory of meaning.[14]

It is our contention that, aside from the Lewis and MacDonald statements quoted above, there was no mention of a *Hysteron Proteron* argument of the sort we describe in §2 above before 1934. And these, especially Lewis's remark, are not so clearly directed at the sort of *hysteron proteron* that we identified at the end of §3 and attributed to Berlin and Lycan. There perhaps was some awareness of a similar feature that perhaps could occur at different stages of the larger investigation imagined in that §3, but none of them are really on exactly the same topic. In the next section we identify what we think is the first clearly-stated instance of our *Hysteron Proteron* argument as applied explicitly to the positivist verifiability criterion. (Of course, one can dispute whether that criterion has been correctly characterized, but certainly it is intended against that criterion, and is a version of the criterion mentioned by Berlin and Lycan.) It seems to us that the positivists, particularly Carnap and Neurath, did not correctly respond to this argument at the time—only perhaps somewhat later, in the case of Carnap. (Although, again, reasonable historians of this period of philosophy might still disagree about whether these 1936/1937 and similar responses really do touch on the true underlying issue as we identified it in §3, or instead are in reality directed against some other argument that is similar in one way or another.)

[14]Usually Hempel (1935) is credited with this change in the "standard" positivistic doctrine, although Hempel himself sees it as already in place in the writings of Neurath and Carnap. Interestingly, however, his citations for this change in Neurath are from the 1930–1933 time frame, while those of Carnap for such a change are from 1934, and even these are not so clearly "holistic", or so it seems to us. The change to "testability" in Carnap (1936–37) seems a more clear dividing line in his thought about the topic.

7. A Different Source of the *Hysteron Proteron* Objection

Another strand in the dispute over verification, this time not coming from the ordinary language group of philosophers, nor from within the Logical Positivist circle, but instead from (at least one of) the Continental phenomenologists, was brought up by Roman Ingarden in the 1934 meetings of the World Congress of Philosophy in Prague.[15] Ingarden published the first of his many large-scale works in 1931, and it is plausible to suppose that the members of the Vienna Circle were at least passingly familiar with Ingarden's name and the fact that he was a student of Husserl. Ingarden says of the English translation of the 1965 third German edition of Ingarden (1931, 95 n 49):

> I first spoke out against the original conventionally physicalist conception of linguistic formations that prevailed up to 1934 (as far as

[15]Küng (1982, 224) says that "Ingarden was the first non-positivist to write a critical paper about the Vienna Circle's principle of verification", citing Ingarden (1936) (but he also remarks on the "revised and extended versions in Polish and French", Ingarden 1935). On the other hand, Kraft (1953, 36) instead points to Petzäll, saying "But the adoption of this verifiability criterion of meaning was soon subjected to incisive criticism. First Petzäll [Petzäll and Åhman 1931] pointed out some untenable consequences of this conception of meaning, and then Ingarden [1935] emphasized that the latter implies that 'metalogical' sentences are non-sensical; also Weinberg [1936] made the same point." We discuss the implicit issue here of metalinguistic statements below.

> my foreign publications are concerned) in a paper delivered at the Prague Congress and then in a somewhat expanded article in the *Revue Philosophique*, "Essai logistique d'une refonte de la philosophie" (1935). The interested reader may refer to that. A part of my counterarguments is contained in §9 of this book.

(The "§9 of this book" refers to §9 of the first edition of Ingarden 1931. He is later looking back on that as his "counterarguments", even though they were written before 1934.) The 1935 French article is a very close translation of his 1934 German paper presented at the Prague Congress. However, §9 of Ingarden (1931) is titled "Single words and word sounds", and seems to be exclusively about the question of how we figure out single words from sounds that are repeated. It does not contain the portions of his 1934 presentation that criticize the "methodological positivists" (as Ingarden called the Vienna Circle in his Prague talk), nor does it contain remarks about the verifiability criterion. The question of how to discern words of a language from repeated sounds is one manifestation of our "mounds of ink" problem from §3 above.

Ingarden, as we have said, was a student of Edmund Husserl's, and much of Ingarden's philosophical thought was concerned with describing a phenomenology that avoided Husserl's "transcendental idealism", which Ingarden viewed as seriously mistaken. It seems to us that the Positivists who might have been familiar with Husserl's work would have thought of the crucial "eidetic intuition of essence" (*Wesensschau*) it employs as hopelessly "metaphysical", and possibly for that reason they did not attend to Ingarden's alternative as closely as they might have. On the other hand, Carnap and Neurath *did* attend the talk, showing at least a modicum of interest in the fact that a phenomenologist was going to discuss verification.[16]

Common to the phenomenological viewpoint that Husserl and Ingarden shared (along with many others of Husserl's students) is the fundamental idea that at least some objects, of some sorts, somehow *depend on minds*, or perhaps better, *on thinking beings*; they *depend on* a thinking being's giving them some sort of "meaning" or "interpretation". In Husserl's hands, this amounted to saying that "the world of" tables, rocks, countries, wars, planets and so forth had an ontologically second-class status in comparison to the mental activities/acts and so forth of thinking beings. And this approach of Husserl's was taken (by Ingarden, at least) to be a type of idealism, of the sort where the "ordinary, real world" is a construction or projection or . . . of mind. (It is not so clear whether this is an individual mind or some sort of Mind in some collective sense.) The classification of it as Transcendental was to indicate that these items are not part of any particular mind, thus separating it from the "subjective idealism sense that Berkeley favours".[17] Since the world *depends on* minds, if there were no minds, there wouldn't be a world.

Ingarden by contrast viewed his own theory as "realist": the real world exists on its own and would exist even if there were no minds. But nonetheless, *some* objects/events/activities/accomplishments/processes in the world *depend on* minds. Of particular relevance to the issue of verifiability is Ingarden's insistence that sentences—viewed in our paper of interest, Ingarden (1936), as sound waves or collections of little mounds of ink—have no meaning on their own. They *depend on* an intentional act of be-

[16] Furthermore, at the 1929 Davos conference, and for some period after that, Carnap interacted quite pleasantly and with apparent interest in phenomenology as expressed in Heidegger. See the various accounts in Friedman (2000), especially Chapter 1 (and scattered elsewhere in the book). In an earlier phase of Carnap's philosophical trajectory, he seemed to be quite in favour of various philosophical positions that Husserl took. (See Friedman 1999, 47ff., 51 and Friedman 2000, 66ff.)

[17] See Husserl (1913, §55):

> If anyone reading our statements objects that they mean changing all the world into a subjective illusion and committing oneself to a "Berkeley idealism," we can only answer that he has not seized upon the *sense* of those statements. They take nothing away from the fully valid being of the world as the all of realities . . . The real actuality is not "reinterpreted," to say nothing of its being denied; it is rather that a countersensical interpretation of the real actuality, i.e., an interpretation which contradicts the latter's own sense as clarified by insight, is removed.

stowing meaning. For instance, by seeing them *as sentences of a particular language* or seeing them as *the intentional product of a thinking being with a purpose*. We are to contrast these sorts of acts of finding an intention with believing those same marks were the result of some "random" physical event, such as being burnt onto a piece of toast you just removed from a toaster or constructed of sand near a wind-swept beach. Once again, there will be a distinction between an individual thinking being bestowing a meaning to a particular sentence on a particular occasion and the society-wide "collective mind" bestowing a meaning (which maybe gives the sentence something akin to its conventional meaning in the language or in society). The sound waves and the collections of ink are nothing more than inert physical things (and they can exist as such without any thinking beings), but once they are endowed with meaning—which act requires a thinking being—they become more than sound waves or marks on paper: they become *sentences of the language/society* and can be said to *express a proposition*. Ingarden seems to say that we then will see the mounds of ink as meaningful parts of a language. This is related to Ingarden's "levels of reality" in his *Literary Work of Art*: ink to sentences, sentences to meanings, meanings to fictional worlds. Here Ingarden doesn't sharply distinguish syntax and semantics of a language.[18]

So, such an object must be *given an interpretation* or *interpreted as meaningful* before we can say it has meaning or talk of it as being meaningful or try to verify it—surely a version of the *hysteron proteron* objection: we need to ascribe meaning to marks on paper and waves in the air before studying any of their further properties that might be related to meaning, such as whether they are verifiable. More generally, the act of meaning-endowing has to precede any sort of "scientific study" of meaning.

Here is a part of Ingarden's argument:

> The sentence "In the interior of that electron one will find a nucleus, that is still there, even though it has absolutely no effect on anything exterior to it" is (according to Schlick) an example of a meaningless, that is to say, unverifiable sentence. It can be granted that this sentence is not verifiable. Why, however, is it not verifiable? Well, because it has a meaning that does not allow for any verification because the nucleus of the electron is specified in the meaning to be with "absolutely no external effects". That is to say, in order to determine the nonverifiablity of the aforementioned sentence one must presuppose two things: 1. That this sentence has a sense that precludes verification, 2. that it is possible to identify this sense independently of its verification (the possibility of which has just been denied). (Ingarden 1936, 206; transl. 4)

Both Neurath and Carnap responded to the paper (Carnap 1936; Neurath 1936). Carnap responds directly to the *hysteron proteron* objection, that is, to the notion that knowledge of meaning is prior to knowledge of verification conditions, but almost in passing:

> To determine whether a given sentence is verifiable or not, one does not need to already know its meaning. This can be established purely formally: one tests whether the given sentence, on the basis of the rules of the language of concern, stands in a deductive relationship with sentences of a particular form, namely the so-called observation sentences. (Carnap 1936, 244; transl. 5)

Thus Carnap replies that determining the verification conditions, and so the scientific meaning of a sentence, is a purely mathematical, combinatorial, problem of determining its deductive relations with observation sentences. He refers the audience to his then-forthcoming (Carnap 1934a), which he says will provide the details. It is difficult to see how Carnap thinks that this set of deductive relations can account for meaning in ordinary, *natural* language. For, his method presupposes the *prior* step of translating into a formalized language. And that is to say, it presupposes that the sentence is meaningful in natural language, and so the translation into a formal language can only be done for the purpose of determining *what* the verification conditions are . . . but this is *after* it is acknowledged to be meaningful. If it

[18] We are influenced here by Simons (2005).

weren't assumed to be meaningful, it wouldn't and couldn't be translated.

In fact, Ingarden's charge is silently accepted in contemporary formal philosophy and even elementary symbolic logic, where the question of how we translate ordinary languages into a formal language with a precise syntax and proof theory is avoided and only some "informal hints about translation into a formal language" are given.[19] After all, the sentences *in* the formal language *are* meaningful, rather by definition. So, one can translate only sentences known to be meaningful. If you were to translate "This stone is thinking of Vienna" as $T(s, v)$, or translate "Julius Caesar is a rational number" as $R(j)$—or as $(R(j) \wedge N(j))$—you do not generate a meaningless sentence, but rather a false one, or perhaps a *necessarily* false one. The very willingness to do the translation in the first place indicates that you take the sentence as meaningful. In this way, one sees that Ingarden's objection is implicitly accepted: one *does* have to know the meaning of a sentence before one can know its verification conditions, even if one thinks the verification conditions are a matter of deductive consequences. The conclusion is that willingness to represent a natural language sentence in a formal language has already committed one to the conclusion that the sentence is verifiable, and that is *before* we can evaluate its truth or verification conditions. We discuss this general issue below (in §§9–10), briefly mentioning a possible direction that Carnap's thought could be developed which would allow it to be applied to natural languages, thus possibly evading the objection that sentences need first to be represented in a formal language. But we conclude that it won't completely do the job.

Carnap devotes more of his brief response to the other part of Ingarden's argument, namely that by the verification principle, the principle rules itself meaningless. This is also the only part of Ingarden's critique that is ever discussed in the literature, and what Ingarden is best-known for in the realm of critiquing the Positivists.[20] But Ingarden doesn't trot out the shopworn "the principle is neither analytic nor synthetic". Instead, Ingarden argues that, because according to the verificationists' own views, sentences about syntax and meaning are not empirically verifiable—they are analytic and hence they are meaningless. Here Carnap responds that Ingarden misses the fact that the verification principle is intended only to explain the meaning of *synthetic* sentences. Sentences *about* the syntax of language are *analytic* sentences, and hence are not explained by verification conditions, but instead by the science of formal syntax, as Carnap proposes in his new book Carnap (1934a). Carnap here distinguishes himself from Wittgenstein, who, according to Carnap, held in the *Tractatus* that metalinguistic sentences are meaningless (*sinnlos*). Thus, according to Carnap, while Ingarden's objection *does* hold against Wittgenstein's sort of verificationism, by ruling the analytic metalinguistic statements meaningless, it won't hold against a different view of these sentences. And here Carnap begs to differ from Wittgenstein:

> I agree with Mr. Ingarden: if the results of philosophy are not sentences, then we must remain silent. In distinction from the (at least former) view of Wittgenstein our circle is now of the position that one can present the results of philosophy, of logical analysis, in

[19] For example, in one of the more pedantic elementary logic textbooks, Kalish and Montague (1964), a rather careful method of generating a translation of a *symbolic* sentence into (stilted) natural language is given. Then the notion of a "stylistic variant" of a translation is presented with a few examples. ("We countenance looseness . . . we attempt no precise description of *stylistic variance*; intuition (here identified with linguistic insight) rather than exact rules must guide".) Such a *free translation* of symbolic formulas into natural language is then used to "explicate" the desired notion of "ϕ is a *symbolization* of an English sentence ψ on the basis of a given scheme of abbreviation". The explication is: "if and only if ψ is a stylistic variant of the literal translation of ϕ on the basis of that scheme." One can see that the issue has simply been sidestepped.

[20] The only discussion we have found of the wide breadth of Ingarden's critique is Woleński (2011), who in turn cites Zabłudowski (1966) (which we have not been able to read). But even Wolenski's article does not explicitly discuss the *Hysteron Proteron* argument.

> exact sentences, namely as syntactic sentences. Setting aside other differences, it appears to me that the main difference between the phenomenological position and that of our circle consists of the fact that we maintain that there are no scientific sentences of a third kind in between the empirical, synthetic sentences and the analytic sentences, namely the supposed results of the phenomenological "intuition of essences" [*Wesensschau*]. (Carnap 1936, 244; transl. 6)

Carnap, on behalf of the Circle, announces a difference from (early) Wittgenstein. The Circle now claims that one can assert meaningful, scientific (albeit analytic), sentences *about* language, but the Circle differs from the phenomenologists who think that these sentences are in a third category, verified by some statements of the intuition of essences—Husserl's "eidetic intuition". Carnap holds that there is no science of the results of this supposed method.

But to us this seems to entirely miss the strength of Ingarden's complaint. How, it should be asked, can Carnap claim to know what are the relevant group of observations that need to be considered, without some idea of what the sentence is claiming? As we discussed in §§3–4, when faced with the Schlick sentence that Ingarden cited, why do we not go to observe caterpillars, or hummingbirds, or any of the (almost?) infinite number of different observations that exist? Ingarden says we know which ones to look for because we know (via *Wesensschau*?) what the sentence means. But what can the positivists offer? Given its assumed meaningless status, on what grounds can it be known what is relevant? We continue this line of thought in the next few sections.

Neurath's reply, though presented as an amplification of Carnap's, can be seen as a step toward a different attitude within the Vienna Circle, to Ingarden's argument. Neurath says:

> To complement Carnap's comments, it must be emphatically stressed that "sentences about sentences" have a place in physicalism, insofar as they are—speaking in the "material mode of speech"—about sentence structures, while the empirical sentences are about other things. Metalogic becomes a science of particular "ornaments".[21] One insists appropriately that mistakes are possible in this area. One can have overlooked relations between signs that one only notices later. One can make a mistake when one sees a sentence as analytic within a given system. (Neurath 1936, 244; transl. 6)

The two positions outlined—Carnap's and Neurath's—are aimed in completely different directions. Carnap is at pains to announce that the metalinguistic claims (about well-formedness of an object language) are meaningful, even though they are analytic or defining statements about the object language. As he says, "We positivists now take the position", contra Wittgenstein, that analytic statements are meaningful—indeed, they are either logically true or logically false. Neurath, on the contrary, says that one can be wrong in one's assessment of the truth or falsity of analytic statements, and that they can be investigated empirically.

Neurath seems to be claiming here that assertions about the syntax of a language (of the sort that would show that a sentence is analytic) are actually empirical, or at least fallible, claims about the physical instances of sentences. One direction that this thought can lead is to the later "empirical semantics" of another member of the Vienna Circle, Arne Naess, and his later followers (Naess 1938, 1953, 1954; Tennessen 1949). Another direction is that attitude we attributed (following Schlick) to some of the American pragmatists in finding that the "remembered results of an activity" can be the foundation of verification. (See the discussion in §4.) These two alternatives form a different type of response to Ingarden's *hysteron proteron* argument, namely that one must empirically determine what the verification conditions of a sentence (and so its meaning) are in a given language. This will determine whether it is an analytic or synthetic sentence. Metalinguistic research would then be a part of the whole of empirical science. We return to Carnap's "metalinguistic defence" in the next section.

[21] Such as whether a sentence is "decorated" as "analytic" or "synthetic".

Seeing Ingarden as a source for the *hysteron proteron* objection is interesting, because his way of looking at the objection comes from a different philosophical viewpoint than the criticisms made by the British ordinary language philosophers, and different again from the "internal" criticisms made from within the Vienna Circle, and different yet again from the "friendly" criticisms of the American pragmatists. This gives us a perspective from which we can see that the problem of assigning meaning to a written or verbal item in the environment has ramifications not only for the phenomenologists but also for the positivists and possibly the pragmatists also. Although it is not so clear that there can be any uniform solution to the *hysteron proteron* problem from these three different standpoints, the possibility of such a solution is nonetheless tantalizing.

8. Carnap and Ingarden on "The Metalanguage"

Very few commentators discuss Ingarden's remarks from this conference. Those who do almost uniformly give general remarks of the sort we quoted in footnote 15 above from Küng and from Kraft.

Sometimes (e.g., Gordon 1996; Thomasson 2012; New World Encyclopedia 2015) this paper of Ingarden's is credited with being the first to give the "classic" objection to the verification principle: that it is neither analytic nor synthetic, and hence is meaningless by its own lights. But it is rather difficult to find this in Ingarden (1936).[22]

[22]Tom Hurka remarked to us that its first English appearance seems to have been in Ewing (1937). But it is already in Carnap (1934b), where—after presenting the verifiability principle—he remarks:

> . . . the following objection. . . has been repeatedly raised:—"If every proposition which does not belong either to mathematics or to the empirical investigation of facts, is meaningless, how does it fare then with your own propositions? You positivists and antimetaphysicians yourself cut off the branch on which you sit." This objection indeed touches upon a decisive point. (Carnap 1934b, 7)

Instead, Ingarden made much of the notion of "metalinguistic statements", insisting that on the positivist's own reckoning they are meaningless. We've seen already that Carnap picked up on one thread of these criticisms: the Circle now claims that metalinguistic statements about an object language are analytic in the metalanguage, and that—unlike the earlier Wittgensteinian account—the current positivists hold these analytic sentences to be meaningful. Carnap's idea here, as detailed in his (1934a), is that the formation rules that describe what a sentence of language $\mathcal{L}_0$ is, are stated in the metalanguage $\mathcal{L}_1$ as (what Carnap calls) **L**-sentences . . . or, as we would say, definitions of well-formed formulas and sentences in $\mathcal{L}_0$—and hence are analytically true (in $\mathcal{L}_1$).

But Ingarden made various other arguments in this article about being a sentence. These do not seem to be addressed in Carnap (1934a), and Ingarden's "solution" to all these problems is dismissed by Carnap in his comment "we maintain that there are no scientific sentences of a third kind in between the empirical, synthetic sentences and the analytic sentences, namely the supposed results of the phenomenological eidetic intuition." It seems to us that if Ingarden can make out his case that the machinery that the positivists are using to admit certain strings of symbols into sentencehood leaves out something important—namely, the *meaning* of the sentence—then there needs to be some sort of further tool that will do the job. It needn't be "phenomeno-

Carnap's remarks about this objection, however, seem to us unsatisfactory. After distancing the Circle from the *Tractatus* answer that all philosophy *is* indeed meaningless, he takes the position that "the logic of science" can proceed by "a purely formal procedure" that has no need to "reckon with the meaning", and which can answer "all those questions which are formulated as contentful [*inhaltliche*] questions". The idea is that formal deduction can be carried out, and produce the truth conditions of the items in the language, without knowing anything about "meaning". To us this seems just to be a case of the *hysteron proteron* fallacy, in the sense that we described towards the end of §3, and again remarked about in the last section (§7) when discussing the issue of representing natural language in a formal deductive system.

logical eidetic intuition", of course. But there would need to be something. We return to this issue in the next section, where we discuss Carnap's idea of a logically perfect language. In the remainder of this section we turn to a few other of Ingarden's claims about meaning and the notion of a metalanguage.

In addition to making the claim that the metalanguage statements are analytic, and hence meaningless—the charge that Carnap explicitly addresses—Ingarden also discussed the thought that, taken to be physical objects (as the positivistic commitment to physicalism has claimed), strings of symbols are nothing but "little mounds of ink" or some particular group of "sound waves". If you want to view them as a *sentence* and not just as an inert physical object then you have to *see them as* something other than little mounds of ink or sound waves. To see a physical object as manifesting an empirical property is to treat any sentence that describes that manifesting as empirical, or synthetic, or scientific. But it is universally accepted that there are *no* observational clues present *within* the little mounds of ink or *within* the sound waves themselves that could give rise to this conclusion. Thus the metalinguistic statement "This mound of ink is a sentence" is not meaningful.

But if one is to consider these physical objects to be *sentences*—that is, *meaningful* pieces that are the result of a conscious activity and will cause some other conscious entity to recognize them as such—this requires more than just having a certain form, as is shown by the example of wind-swept sand and sound sequences that are identical but occur in two different languages. And we are also drawn in that direction, although perhaps not drawn to making use of a "phenomenological eidetic intuition of essence". Perhaps, instead, simply *agreeing* to treat them as meaningful,[23] or noting that a string of words exhibits a form that is recognizable as a sentence of a public language with a public meaning.

[23] As Carnap's talk of finding the deductive consequences of a sentence seems committed to.

(Of course, unless some very delicate distinctions can be made in all this, the point will be that some meaningful statements might not be verifiable.)

Ingarden objects to the idea that (meaningful) sentences, and the (meaningful) words that comprise them, are sound patterns from which we abstract some familiar meaning properties. Instead he thinks of a word as psychologically being a gestalt pattern that is the same in every instance. (He thinks of Husserl as seeing some of this with his anti-psychologism, but as mistakenly seeing meanings as abstract essences which do not change.) Ingarden instead (in his 1931) invoked the notion of a constructed abstract entity, which is also what works of art are.

In our opinion, the upshot of this second "parting of the ways" (apologies to Friedman 2000) seems to be that the positivists came away from the Congress worrying about metalinguistic statements (possibly taking hope from Carnap's assurance that it was all dealt with in Carnap 1934a), when they should have started worrying about the *hysteron proteron* argument. And that this was a lost chance for them to discuss something of mutual importance. It is not at all clear to us how the positivists really have an answer to this objection.[24]

9. Carnap on the Relation Between Meaning and Protocol Sentences

Although in his response to Ingarden Carnap focussed mainly on the issues concerning the metalanguage, he did briefly address the issue of the *hysteron proteron* argument, as we quoted in §7. A non-observation statement somehow gains its meaning from the observation statements that it entails. This general strategy had been deployed earlier, however: Carnap (1931–32) had already

[24] It might be added that the American Pragmatists have similar reservations about the positivists' verifiability criterion, although there are analogous problems with their own account of how sentences acquire the particular meaning they have. But that portion of our story awaits our longer discussion.

discussed the idea that there were two sources of "metaphysical nonsense", one occurring with certain words and the other due to the embedded implications of the words making up a sentence—in the next section we will describe the second source as a kind of failure of semantic compositionality. The former type of nonsense was to be eliminated by deleting such words from language;[25] the latter type was said to exhibit a shortcoming of natural languages. Concerning the former source, he says:

> . . . for most of the other *specifically metaphysical terms are devoid of meaning*, e.g. "the Idea," "the Absolute," . . . "thing in itself," . . . "emanation," . . . [26] the metaphysician tells us that their empirical truth-conditions cannot be specified; if he adds that nevertheless he "means" something, we know that this is merely an allusion to associated images and feelings which, however, do not bestow a meaning on the word. (Carnap 1931–32, 67; original emphasis)

But even after the elimination of these "metaphysical terms" there is still a "metaphysical remainder" in natural languages. And about this latter source, Carnap first recalls the dispute over protocol or observation sentences concerning the nature of such sentences.

> In the theory of knowledge it is customary to say that the primary sentences [protocol sentences] refer to "the given"; but there is no unanimity on the question what it is that is given. At times the position is taken that sentences about the given speak of the simplest qualities of sense and feeling (e.g. "warm," "blue," "joy" and so forth); others incline to the view that basic sentences refer to total experiences and similarities between them; a still different view has it that even the basic sentences speak of things. Regardless of this diversity of opinion it is certain that a sequence of words has a meaning only if its relations of deducibility to the protocol sentences are fixed, whatever the characteristics of the protocol sentences may be; and similarly, that a word is significant only if the sentences in which it may occur are reducible to protocol sentences. (Carnap 1931–32, 63)

And then secondly, he describes how deductions that conclude in these protocol sentences—whatever they are—*should* give rise to the meaningfulness of all other meaningful sentences. But despite this, there remain meaningless sentences, and this is a flaw of natural languages:

> The grammatical syntax of natural languages, however, does not fulfill the task of elimination of senseless combinations of words in all cases. . . .
>
> 1. "Caesar is and"
> 2. "Caesar is a prime number"
>
> The word sequence (1) is formed countersyntactically; . . . The word sequence "Caesar is a general," e.g., is formed in accordance with the rules of syntax. It is a meaningful word sequence, a genuine sentence. But now, word sequence (2) is likewise syntactically correct, for it has the same grammatical form as the sentence just mentioned. Nevertheless (2) is meaningless. "Prime number" is a predicate of numbers; it can be neither affirmed nor denied of a person. . . . The fact that the rules of grammatical syntax are not violated easily seduces one at first glance into the erroneous opinion that one still is dealing with a statement, albeit a false one. . . . The fact that natural languages allow the formation of meaningless sequences of words without violating the rules of grammar, indicates that grammatical syntax is, from a logical point of view, inadequate. If grammatical syntax corresponded exactly to logical syntax, pseudo-statements could not arise. If grammatical syntax differentiated not only the word-categories of nouns, adjectives, verbs, conjunctions etc., but within each of these categories made

[25] It should be noted that Carnap (1934a, part V) seems instead to "translate" entire sentences with these offending words into the "formal mode of speech", and then (on the whole) to evaluate the metaphysical claim thus made as falling within the field of some science, including logic. For instance, in Carnap (1934a, 297) we read that "The moon is a *thing*; five is not a thing, but a *number*" is to be translated as "'Moon' is a thing-word (thing-name); 'five' is not a thing-word, but a number-word." Presumably, recalling his earlier contretemps with Heidegger (Carnap 1931–32) where he said that Heidegger made "the mistake of employing 'nothing' as a noun" (68), he would now (in 1934) rather say "'Nothing' is not a name-word; 'nothing' is a quantifier-word."

[26] Carnap here lists 17 of these "specifically metaphysical terms" and ends the list with "etc."

> the further distinctions that are logically indispensable, then no pseudo-statrments could be formed. . . . In a correctly constructed language, therefore, all nonsensical sequences of words would be of the kind of example (1). . . . It follows that . . . metaphysics could not even be expressed in a logically constructed language. (Carnap 1931–32, 67–68)[27]

We see here in Carnap (and that of Positivism generally) the move towards construction of "logically perfect languages" that will enforce their anti-metaphysical viewpoint. The central idea of such an artificial language is that all individual words must come with a set of the observational sentences that it licenses, and the correct syntactical construction rules will have to take these into account somehow in their formation of legitimate combinations of larger syntactic units from smaller ones. As Carnap notes, this will require a syntactic component in the logically perfect language that makes all the "logically indispensable distinctions, so that no pseudo-statements could be formed". Although Carnap apparently thinks that natural language is, in its very nature, imbued through and through with meaningless metaphysics, in the next section we shall briefly investigate whether his general idea might nonetheless be carried out entirely within natural language.

10. Logically Perfect Languages

In his (1934a), Carnap imagines carrying out this procedure by subcategorizing all nouns (and verbs and adjectives) into such groupings as "thing-names", "number-names", "physical-color properties", and the like. But perhaps a more direct version can be gleaned from the idea suggested in the earlier (1931–32) and along the lines we just canvassed. What would be required is that sufficient effort be deployed to describe the observational consequences of all the descriptive words, to discover all the compatible and incompatible implications, and to determine exactly how observational consequences of syntactically complex phrases are determined compositionally from those of their simpler parts when they are composed by any relevant syntactic rules.

In this picture, one might claim that when the syntactic rules operate to form slightly larger units from the words—for instance, to form *large building* as a Common Noun Phrase (CNP) from the adjective *large* and the noun *building*—they simultaneously check whether this syntactic combination is semantic-philosophically correct. Here each of *large* and *building* will have their set of observational implications, and these will somehow have to pass a test of "having compatible implications" in order for the CNP to be assured of a coherent set of the resulting observational implications for *large building*.[28] To form the Noun Phrase (NP) *The large building*, we somehow use the observational implications just uncovered for *large building* and those relevant to *the*:[29] we check for compatibility, and we generate a new set of observational implications. We similarly find the observational implications of the Verb Phrase (VP) *is brown* from those of the Adjective *brown* and the Copula *is*, and now investigate whether the observational implications of *The large building* and of *is brown* are compatible, and generate a final set of observational implications for the sentence that is made from an NP+VP, *The large building is brown*. We could also find the observational implications of the VP *is irrational*, and then discover that there

[27] From the different philosophical milieu of British ordinary language philosophy, Gilbert Ryle speaks to what is apparently the same issue in his article on "category mistakes":

> When a sentence is (not true or false but) nonsensical or absurd, although its vocabulary is conventional and its grammatical construction is regular, we say that it is absurd because at least one ingredient expression in it is not of the right type to be coupled or to be coupled in that way with other ingredient expression or expressions in it. Such sentences, we may say, commit type-trespasses or break type-rules. (Ryle 1937, 200)

[28] For instance, by identifying a certain subset of the buildings—the ones that are large-for-a-building.

[29] Such as asserting familiarity or uniqueness in the context of utterance.

is no compatibility with those of *The large building*—and hence no observational implications of the sentence *The large building is irrational*. And so that sentence would be judged empirically meaningless (excepting metaphorical interpretations).

All this seems to be just what was envisaged by Carnap in his remark that if syntax differentiated not only nouns, verbs, etc., but within these also "made the further distinctions that are logically indispensable, then no pseudo-statements could be formed", as well as by Schlick (1936), as we discussed in §4. But note that the account we just briefly and incompletely described was applied to terms of a *natural language*, not of an ideal language. If the attitude taken by Montague ("There is in my opinion no important theoretical difference between natural languages and the artificial languages of logicians . . . "; Montague 1970, 373) and other formal semanticists is correct, then the Carnapian (Positivist?) goal of a logically perfect language might already be a part of a correctly-conceived natural language. Or at least, of an *almost* natural language, if one left out the "specifically metaphysical words".

The Montagovian attitude and many of the advancements in formal semantics since that time, can suggest that the requirement of a "logically perfect language" would not strictly be necessary to carry out the sort of project that Carnap has envisaged: the rationales that might be given for a logically perfect language could as well be accommodated within natural languages, if only we had a sufficiently detailed account of the logically correct semantic features of all the words and syntactic constructions. Such a "thoroughly modern" semantic theory could possibly do the work of a Carnapian logically perfect language, if sufficient effort were deployed to describe the observational consequences of all the descriptive words, to discover all the compatible and incompatible implications, and to determine exactly how observational consequences of syntactically complex phrases are determined compositionally from those of their simpler parts when they are composed by any relevant syntactic rules.

There of course has been a challenge from those who deny the need for *any* formal restatement of ordinary language. In the time-period between the mid-1930s and the early 1960s, the ordinary language philosophers of Oxbridge filled this role, saying things like "ordinary language is just fine the way it is" and "it is only when philosophers misuse language that metaphysical difficulties ensue". Perhaps this general anti-theory perspective just shows how distant Ordinary Language Philosophy is from Positivism, Pragmatism, and Phenomenology; and if so, then maybe there just is nothing to say in response. But could one also take the challenge a bit differently, and see them as saying that we don't *need* to "compute observational consequences"? Or maybe that computing them is just another "philosopher's mistake"? Or perhaps the charge is even more pointed, involving something like "ordinary language has plenty of metaphysical items in it; but they never get in the way so long as we are not doing philosophy"? It is not at all clear what the advocate of a logically perfect language or its modern alternative that we outlined—whether their sympathies lie with Positivism, Pragmatism, or Phenomenology— could do in the face of such challenges other than to simply deny their claims . . . pointing, perhaps, to what they think of as meaningless drivel coming from very many philosophers.[30]

Even the positivists might demur from the thoroughly modern approach on grounds similar to the ordinary language philosophers. They might think—like the ordinary language philosophers maybe think—that ordinary language has plenty of metaphysical items in it. But unlike the ordinary language people, they probably would still want to eliminate them, even if or-

[30]This apparently is the direction Carnap (1931–32) took. Although the examples he cites all come from Heidegger (1929), Carnap remarks (69 n 2): "The following quotations . . . are taken from M. Heidegger . . . We could just as well have selected passages from any other of the numerous metaphysicians of the present or of the past; yet the selected passages seem to us to illustrate our thesis especially well."

dinary people who aren't doing philosophy can use them and never get entangled in metaphysical issues. But if this is the attitude that they take, then they will quite probably be against the "modern" version—at least in the way it is currently practiced, because many instances the type of metaphysics that the Positivists found objectionable would be given legitimacy. Or at least, they would be given legitimacy if one thought that language was "a guide to reality" in the sense that when one's language allows a person to assert some metaphysical claim ("The Absolute is worthy of study") that makes a claim about reality to the effect that there is something existing which is named by the noun phrase "the Absolute".

We see two general sources of problems for this view. The first is that the types of restrictions discussed by Carnap (and also by scholars in the Montague tradition) seem to be "too simplistic" to really work at ruling out meaninglessness in general. Here are a few places that generate this type of challenge:

1. Even within the realm of what seem to be obvious "observation claims" there is much that is still to be determined. Consider colour predicates, such as *red*. Many have argued that there is no unique observation or set of observations that can show how they are related to determining whether a sentence such as *This object is red*: it might be wine, or a cherry, or a watermelon, or hair, or a fire truck, or skin, or . . .
2. The sentence *Mary put a little chicken onto the salad* is ambiguous, and the cause of that ambiguity is the word *chicken*. The OED gives separate entries: *A domestic fowl kept for its eggs or meat, especially a young one* and *Meat from a chicken*, and that would be how such "readings" of the sentences could be observationally distinguished. But for most edible animals, there is no such distinction in dictionaries—the sentence *Mary put a little kangaroo onto the salad* does not have such a lexical ambiguity, and so there is no linguistic reason to think it is ambiguous, even though it is.
3. Suppose one says *That shelf contains my Greek books*. How can this be verified? Should it include the books written in (one of) the dominant languages of Greece? Ones written by an inhabitant of Greece (even if not in Greek)? The travel guides to Greece? The books that were given to me by my Greek colleagues? The books that were bought in Greece? The books I have taken to Greece on my trips? The ones I intend to take on my next trip? Etc., etc.[31]
4. We mentioned Ryle's notion of a category mistake in footnote 27. One way of understanding Ryle—a way that seems very similar to Carnap's—is that there can be pairs of nouns, each of which admit only of a certain range of predicates and these ranges are disjoint. Thus we might get **Julius Caesar is an irrational number* and **π crossed the Rubicon*, if we were to attempt to use a predicate from one range and apply it to an object in a different realm. But this thought founders on its too-frequent success: Smart (1953) pointed out that phrases like *The seat of the chair is hard* is good, but **The seat of the bed is hard* is not, remarking that "if furniture words do not form a category, we may well ask what do." (Smart also considers a number of other types of claims, such as "The current in this wire is $\log_{10}(7 - x)$ amps" where he remarks that $8, 9, 10, \ldots$ will not substitute for x, though the name of any negative number or of any positive number ≤ 7 will.) Perhaps all of this shows that natural language categorizes much too finely for such a blunt test to succeed in detecting "semantic nonsense".
5. Another type of example has been brought out in Shaw (2009)[32] in considering relational predicates. One of Shaw's very many examples is to note that both *Jack* and *Jill* can appear as subject and as prepositional object of the sentence

[31] This example is modified from one in Sainsbury (2001).

[32] We thank Gary Ebbs for alerting us to this work, and to James Shaw for a copy of his dissertation and permission to cite it.

form *x jumped over y*. But they cannot occupy both positions in the same sentence: **Jack jumped over himself* and **Jill jumped over herself* are bad. Once again it seems that the fine-grainedness of meaning inherent in natural language will make this type of "category theory of meaning violation" be impossible.

The second problem for a Carnap-style solution within natural language itself is much broader in its scope. Even assuming that the previously-indicated problems can somehow be overcome—they are, perhaps, merely issues of detail as to how the project is carried out—there seems nonetheless a more fundamental issue that would require a resolution which is not obviously forthcoming. Many (or even most) modern formal semanticists take the view expressed by Emmon Bach (1986), to the effect that there is a large difference between metaphysics (what there is) and what speakers *talk as if* there is.

> Metaphysics I take to be the study of how things are. It deals with questions like these:
>
> > What is there?
> > What kinds of things are there and how are they related?
>
> Weighty questions, indeed, but no concern of mine as a linguist trying to understand natural language. Nevertheless, anyone who deals with the semantics of natural language is driven to ask questions that mimic those just given:
>
> > What do people talk as if there is?
> > What kinds of things and relations among them does one need in order to exhibit the structure of meanings that natural languages seem to have?
>
> Questions of this latter sort lead us into *natural language metaphysics*. (Bach 1986, 573)

The picture expressed here from within the modern alternative suggests that the Positivists would reject it because it relies on evidence garnered from the way natural language is used—and this is seen as precisely what should be removed from the language. In fact, a survey of modern formal semantics shows that it would be extremely difficult to remove all traces of (what the Positivists would claim is) metaphysical entanglement from natural language. After all, it is plausible to think that natural language has historically arisen from times when what is now seen as metaphysical or magical was taken to be "observable" (in some sense of that word). And traces of that, it is plausible to think, still exist in the current epoch of language evolution.

11. A Possible Rapproachment?

Both the phenomenologists and the positivists need to give up something, it seems to us. Here are our recommendations.

The positivists should forget the claim that learning truth conditions of a sentence *S* is the same as learning the meaning of *S*, and instead follow a pathway suggested in Schlick (1936), whereby one learns meanings independently of and prior to the truth conditions.

The phenomenologists will have to abandon their claims of almost-mystical knowledge of meaning. Certainly it need not be something as obviously mysterious as an "eidetic intuition of essences". Perhaps it is simply a part of language-learning in the first place, taking place before any thought of determining whether some sentence is or isn't true, or how to tell which it is. This would seem in some way or other to be friendly to the ordinary language philosophers.

In admitting that there is intuitive meaning prior to truth conditions, the positivist actually can appeal to another aspect of their overall position that seems to have got lost in the present dispute: their distinction between cognitive meaning and the various other types of meaning such as emotive meaning, proposal meaning, and whatever other sorts of meaning they sometimes advert to. In our proposed picture, cognitive meaning forms an interesting subclass of meaning. And a positivist—although probably not a phenomenologist—can claim that it is

the most interesting type of meaning. That it is the type of meaning that can be determined (at least in principle) because of its clear (physicalist?) truth conditions. The phenomenologists can continue to claim that there is more to meaning than just this sort of cognitive meaning, perhaps claiming that important philosophical and metaphysical statements are of some other type than cognitively meaningful ("philosophically meaningful"?).

Along the way we may wish also to add that the (apparent) viewpoint of some pragmatists can be accommodated by admitting yet another aspect of meaning (an empirical type of cognitive meaning?) whereby the truth conditions are arrived at through experience in some as-yet unspecified manner.

It seems to us that there is no fundamental reason that the affected parties couldn't adopt such a rapproachment.

12. Some Concluding Remarks

Limbeck-Lilienau (2012, 103ff.) relates the following story, drawn from his investigations of the letters of the people involved. C. I. Lewis, in an attempt to continue interaction between the American pragmatists and the logical positivists, sent preprints of his 1933 APA Presidential Address (published as Lewis 1934) to Carnap, Schlick and Morris with the desire that they should write replies. Although Carnap wasn't eager to do so, Feigl convinced him that writing replies would be very important in getting Lewis (who was, after all, the President of the American Philosophical Association) to support Carnap's desire to emigrate to America. Carnap wrote to Schlick, suggesting that Schlick might take up the topic of comparing pragmatism to the Vienna Circle, while he (Carnap) would concentrate on the question of empirical meaning. Both wrote these papers, sending them to one another and also to Lewis and Morris. Schlick's was published in 1936,[33] in the *Philosophical Review*, as was Lewis's original paper. However, although Carnap wrote a "Notes for a Reply to Lewis", this was not published in the form originally intended, but these notes became the basis for a series of talks by Carnap in America during Spring 1936 and a seminar at Harvard in Summer 1936. These various thoughts were finally published in 1936 and 1937 as two long parts of a very long paper in the journal *Philosophy of Science* (Carnap 1936–37). The beginning sections of the first part of this paper seem to be almost identical with the earlier "Notes", so we can see where Carnap thought positivism's conflict with pragmatism was located.

This paper starts by asking two questions: first, "under what conditions does a sentence have meaning, in the sense of cognitive, factual meaning?"; and second, "how can we find out whether a given sentence is true or false?" He then says "The second question presupposes the first one. Obviously we must understand a sentence, i.e. we must know its meaning, before we can try to find out whether it is true or not." Clearly this is an instance of the *hysteron proteron* argument!

But unfortunately, it is not at all clear what Carnap thinks, in this article, the correct response would be. Or rather, it is not at all clear that what Carnap offers is really relevant to the argument. What he says is:

> . . . from the point of view of empiricism, there is a still closer connection between the two problems. In a certain sense, there is only one answer to the two questions. If we knew what it would be for a given sentence to be found true then we would know what its meaning is. And if for two sentences the conditions under which we would have to take them as true are the same, then they have the same meaning. Thus the meaning of a sentence is in a certain sense identical with the way we determine its truth or falsehood; and a sentence has meaning only if such a determination is possible. (Carnap 1936–37, 420)

The discussion in the remainder of the early part of Carnap's article concerns how the notion of testability should be seen as a replacement for verifiability, and how there are many different

[33] The quotation we gave above in §2 as an example of an instance of the *hysteron proteron* argument is in this article of Schlick's; see §4.

levels or aspects of testability about which one might legitimately differ from another person as to which ones are more important than some others. And it is here that Carnap contrasts his aspects with those of Lewis, emphasizing that there is to be a "tolerance" for different choices of these levels/aspects and for differing orderings of importance even in the case where it is agreed what the aspects are.[34]

But none of this seems particularly relevant to the *hysteron proteron* argument, especially in the guise that Lycan put it (as we quoted at the beginning of this paper, and which we partially repeat here):

> Suppose we look at a given string of words, and ask whether or not it is verifiable, and if so what would verify it. In order to do that, we already have to know what the sentence says; how could we know whether it was verifiable unless we knew what it says? (Lycan 2008, 101)

So far as we have been able to ascertain, the only Positivist who ever discussed *hysteron proteron* argument in the terms that it set out was Schlick (1936), as we mentioned in §4. There was also, of course, Carnap's single sentence in (1936) and the claim we just looked at in (1936–37). But these latter seem not to accurately characterize the *hysteron proteron* argument nor even be much aware of its real direction, despite its prominence in Berlin (1939), an article which was taken seriously at least by Ayer. They keep saying that meaning and truth conditions are the same, without explaining how it could be possible to get truth conditions if you do not antecedently have meaning. Furthermore, even with Carnap and Neurath in the audience, this argument was not especially attended to when it was presented by Ingarden (1936), but was seen as a side-issue. Perhaps the explanation lies in a mixture of the following:

(a) Despite the fact that Husserl and members of his "school" like Ingarden and Heidegger were known to the Positivists, perhaps the differences among their different versions of Phenomenology were not very apparent to the Positivists.

(b) The Positivists, and especially Carnap, were convinced that the philosophy of Heidegger was irredeemably and hopelessly "metaphysical", and in the very strongest sense was totally meaningless (Carnap 1931–32, §5).

(c) (a) and (b) led Carnap and Neurath—and through them, all the other Positivists—to ignore the Phenomenological aspects of Ingarden's work and pay attention only to the "metalanguage" criticism, which Carnap thought he had answered satisfactorily in his (1934a).

(d) Although the Positivists and the Pragmatists saw many similarities between their philosophies, the Positivists rejected that part of Pragmatism which seemed like the repudiated Phenomenological doctrines concerning the conferring of meaning, even though it is not so totally clear that this aspect of pragmatism and the similar (in this respect) Ingarden version of phenomenology really are necessarily repudiated when the Husserlian and Heideggerian versions are dismissed.

The linchpin here seems to be Heidegger in his role indicated in (b) above. All in all, this Parting of the Ways of major schools of philosophy (and of their main spokespeople) looks once again to be due to the deleterious influence that Heidegger had on European philosophy between the World Wars, as well as on German academic society as a whole during that time. And of course, there was his (and his students', and their students')

[34] Paul Franco has pointed out to us the interesting and possibly relevant fact—relevant especially for understanding the interesting roles of ordinary language philosophy and American pragmatics in our general investigation—that Isaiah Berlin and John Austin jointly taught a seminar at Oxford in the late 1930s on the book (Lewis 1929). Berlin (1973, 7) remarks that he "had a year or two before read an interesting book on philosophy by a professor at Harvard of whom [Berlin] had not previously heard." Berlin loaned it to Austin, who read it and "three days later he suggested to me that we should hold a class on this book which had also impressed him."

harmful effect on the field of philosophy during the 1940s–1980s, which it has still not recovered from.

Acknowledgements

This is part of a proposed, but—alas—still unfinished longer work that investigates the interactions in the 1930s amongst the (Austrian and German) Logical Positivists, the (Continental) Phenomenologists, the (American) Pragmatists, and the (British) Ordinary Language philosophers. We hope to eventually finish that longer work! In the meantime we wish to thank Jack Clontz, Rick Creath, Gary Ebbs, Paul Franco, John Heintz, Bruce Hunter, Tom Hurka, Bill Lycan, Alex Rueger, Thomas Uebel, and Jan Woleński for discussions and advice, as well as the referees and action editor (Richard Zach) of this journal for their comments. Versions of this paper have been presented at the Western Canadian Philosophical Association and at the Society for the Study of the History of Analytical Philosophy meetings. We are grateful for the comments made by audiences at those meetings, and to John Heintz who was a commentator at the WCPA occasion.

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