

On Hempel on Hempel

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Abstract

Hempel publicly abandoned the Received View on scientific theories in the 1960s in favor of a new view. However, Hempel misrepresents his own works within the Received View in a number of his criticisms, and his new view turns out to be identical to the Received View on correspondence rules, observational terms, theoretical terms, and the demarcation between basic principles of a theory and correspondence rules. Hempel’s criticism of the assumption of axiomatization has counterexamples in his own previous work within the Received View, and his criticism of the meaning of theoretical terms in the Received View ignores developments he witnessed and discussed.

Keywords: Hempel; received view; correspondence rules; abstract calculus; axiomatization; observational terms; theoretical terms; analytic; synthetic; Carnap

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I Suppe on Hempel on Hempel

In his reminiscence about the 30-year-long supremacy of the semantic view on theories, Frederick Suppe (2000, S102) treats the reader to the violent death of logical positivism:

The *Received View on Theories* was the epistemic heart of Logical Positivism. Twelve hundred persons were in the audience the night it died. [...] The Received View had been under sustained attack for a decade and a critical mass of main protagonists had been assembled to fight it out. Carl Hempel [...] was expected to present the Received View's latest revision. Instead he told us why he was abandoning both the Received View *and* reliance on syntactic axiomatizations (Hempel 1974). Suddenly we knew the war had been won, and the Symposium became an energized exploration of where to go now.

Never mind that the Received View, which had been mainly developed by Carnap and previously relied upon by Hempel, was not the epistemic heart of logical empiricism, simply because a number of logical empiricists took different views on theories (Mormann 2007).¹ Never mind that, to stay in Suppe's violent metaphor, the 1200 conference participants in 1969 did not witness the death of the Received View, but at best discovered its body: As Suppe himself notes (Hempel 1974, 244, editor's note), Hempel's presentation was based on a lecture given in 1966, which in turn was based on a lecture from 1965. What I find most dispiriting is that Suppe presents the philosophical debate not as a successive improvement—if you will even a war—of ideas, but rather a sort of political maneuvering, in which allegiances count more than arguments.

A philosopher of science (as opposed to a historian of philosophy) should care about Hempel abandoning the Received View in the same way that a philosopher of religion should care about Antony Flew abandoning atheism (cf. Oppenheimer 2007): If his criticisms of the Received View are good, they stand on their own. If his criticisms are bad, Hempel's personal stance does not improve them.

As it turns out, Hempel's criticisms are bad, and bad in a particularly puzzling way for an erstwhile proponent of the Received View: Like many critics before and after him (Lutz 2012b), Hempel misrepresents the Received View, sometimes blatantly so, to then criticize the straw man. That Hempel misrepresents his own view is a bold thesis, so I hope that the gentle reader will indulge me while I lay out the evidence, letting Hempel's

1. Some people who really should have known better have parroted this assessment (e. g. Lutz 2012b, 77).

psychology be the topic for someone more versed in the matter (although I will engage in some groundless speculations in the epilogue).

2 Two views on scientific theories

2.1 Abandoning the Received View

In 1965, Hempel gave the lecture “The Legacy of Logical Positivism for the Philosophy of Science” as one of the Johns Hopkins Seminars in the Philosophy of Science.² His conclusion in the article based on the lecture is positive (Hempel 1969a, §6):

A fair-minded appraisal of the accomplishments of logical positivism should not focus on the bold and naturally oversimplified devices its adherents wrote upon their banners, but on the quality of the detailed logical and methodological studies carried out under those banners[.] Thus judged, logical positivism will be found, I think, to have been a strong and fruitful influence in recent systematic philosophy.

That same year, and in the following five year span, Hempel gave four lectures in which he abandoned the Received View. He first gave an Isenberg Memorial Lecture in East Lansing in the Fall of 1965 (Hempel 1969b), which was “develop[ed] further, and modifie[d] in certain respects” in his lecture at a Minnesota conference on correspondence rules in May 1966 (Hempel 1970, 142, n. 1). His lecture at the Urbana conference on the structure of scientific theories, on which Suppe reports, was given in March 1969 and published as a “summary-abstract” to “avoid excessive repetition” from the Minnesota conference (Hempel 1974, 244, editor’s note). Finally, Hempel gave another lecture at the Fourth International Congress for Logic, Methodology, and Philosophy of Science in Bucharest in 1971 (Hempel 1973b).

In all four of the lectures, Hempel lays out that according to the Received View, a theory consists of two sets of sentences, a set C of formulas of an uninterpreted calculus, and a set R of correspondence rules, which provide the interpretation of the calculus by connecting its terms with observation terms (Hempel 1969b, 49–50; 1970, 146–47; 1974, §II; 1973b, 368–69). Hempel calls this “Schema I” (Hempel 1969b, 49), “the ‘standard’ or the ‘received’ philosophical construal of scientific theories”, “the standard conception”, or “the standard construal” (Hempel 1970, 146; 1974, §II), and the “standard construal, or the standard analysis” (Hempel 1973b, 367).

In the East Lansing lecture, Hempel (1969b, 49) notes: “I have myself repeatedly used a construal of this kind in discussing the status of theories and of theoretical entities; but it seems to me now that, while not strictly untenable, it is misleading in several respects.” This is echoed in the Minnesota lecture (Hempel 1970, 147): “I have myself relied on the standard construal in several earlier studies, but I have now come to consider it misleading in certain philosophical significant respects[.]” In a footnote, Hempel

². See the *Virtual Archive of Logical Empiricism*, call number RC 090-75-03. phaidra.univie.ac.at/o:1720472

gives his article “The Theoreticians Dilemma”, written in 1956 or 1957 (Hempel 1958, 37, author’s note) as the example of his reliance on the Received View. The Urbana lecture could be considered a further sharpening of his stance, since Hempel (1974, §II) now states: “While I have repeatedly relied on the standard construal in earlier writings, I have come to feel increasing doubts about its adequacy.” Thus in East Lansing, the Received View seemed to be misleading, while in Minnesota, Hempel had become more certain and considered it misleading in significant respects. In Urbana, the charge had become straightforward inadequacy, although expressed with some uncertainty. In Bucharest, finally, Hempel (1973b, 377) categorically states that “at least one of the major problems to which the standard conception was addressed, the problem of meaning specification for theoretical terms, rests on a mistaken presupposition and thus requires no solution.” In Bucharest, Hempel (1974, 367) also does not point out any longer that he once held the Received View. He instead notes that it was developed (as opposed to merely applied) by “various thinkers [...]; among them, F. P. Ramsey, N. R. Campbell, R. B. Braithwaite, Hans Reichenbach, Rudolf Carnap, Herbert Feigl, and Ernest Nagel.”

“The Theoreticians Dilemma” had been revised during a sabbatical at the Center for Advanced Studies in the Behavioral Sciences in 1963–64 for republication in *Aspects of Scientific Explanation*. In that collection’s introduction, Hempel (1965, vii–viii) states:

While I still regard the central ideas of the reprinted essays as basically sound, I have naturally changed my views on various points of detail. Where it seemed appropriate, such changes have been indicated in footnotes marked “Added in 1964” or in the Postscripts by which I have supplemented three of the articles.

None of the criticisms Hempel presents in his four lectures beginning in 1965 are mentioned in footnotes in the article’s republication, which also does not have a postscript. The collection was published a year later, the same year that Hempel gave his positive evaluation of the Received View in “The Legacy of Logical Positivism for the Philosophy of Science”. Thus it is probably fair to say that the four lectures that followed provide Hempel’s reasons for abandoning the Received View.

2.2 A new view on scientific theories

After the Received View is given up, what, then, is the right way of reconstructing theories? At the Minnesota conference, Hempel (1970, §1) provides a better view (see also Hempel 1969b, 50; 1974, 246), with a proviso: In the subsequent discussion, he stresses that his “paper was intended principally as a criticism [and] did not put forward a properly developed alternative” (Achinstein et al. 1974, 261). Hempel’s new view reconstructs a theory T as a pair $\langle I, B \rangle$ of two sets of sentences, the internal principles I and the bridge principles B . The internal principles contain only terms from the theoretical vocabulary V_T , while the bridge principles connect these theoretical terms to terms from the antecedent vocabulary V_A . V_T and V_A form a partition of the whole vocabulary. Thus Hempel (1970, 143) suggests that

$$(1a) \quad T = \langle I, B \rangle ,$$

or, “alternatively, and with greater intuitive appeal, T may be construed as the set of logical consequences of the sum of the two sets:”

$$(1b) \quad T = c(I \cup B) .$$

At the East Lansing conference, Hempel (1969b, 50) does without the deductive closure:

$$(Schema II) \quad T = I \cup B$$

As Hempel notes, there “are obvious similarities to the [Received View], but there are also considerable differences; this will become clear in the following amplification and comparison of the two construals.”

3 Correspondence rules

The first alleged difference that I want to discuss is, to put it bluntly, silly.³ In the Minnesota lecture, Hempel (1970, 147) claims:

Whereas the bridge principles in our initial characterization of a theory are conceived as a subset of the class of sentences asserted by the theory, the status of the correspondence rules in the standard construal is less clear. One plausible construal of them would be as terminological rules belonging to the metalanguage of the theory[.] For this reason, no immediate analogue to (1b) is available as an alternative schematization of the standard view.

The problem with this view on correspondence rules is that while a full axiomatization determines “for any sentence S , whether S is asserted by the theory”, and thus “the infinite set of sentences that a proposed theory is intended to assert”, the Received View does not treat correspondence rules as sentences in the object languages, so that it cannot assume an axiomatization of the full theory (148–49). (Hempel does not consider the possibility of axiomatizing the calculus in the object language and, separately, the correspondence rules in the metalanguage.)

The reason this criticism is silly is that in “The Theoretician’s Dilemma”, Hempel’s example of him relying on the Received View, Hempel (1958, §§4, 6, 8) treats correspondence rules as definitions, conditional definitions, and more general sentences in the object language, and explicitly uses the conjunction of abstract calculus and correspondence rules as the “interpreted theory” T' . In particular, he uses the Ramsey sentence of T' (Ramsey 1931, §4), which, as Hempel (1970, 149, n. 8) himself notes, presupposes “axiomatization of the entire theory”, including the correspondence rules. To make the whole criticism even more puzzling, Hempel mentions Ramsey’s investigation explicitly as a contrast to the Received View in this respect, even though he also lists the investigation as one of the “characteristic stages in the evolution of” the Received View (146, n. 4).

3. I received incredulous stares when showing the following passage at the University of Vienna.

What is more, in the East Lansing lecture less than a year earlier, Hempel (1969b, 49) uses the union of abstract calculus and correspondence rules as a description of a theory in the Received View:

The conception of a theory as consisting of an abstract calculus C and a set R of rules of correspondence will be briefly represented by the following schema, which construes a theory as the sum of two classes of sentences:

(Schema I) $T = C \cup R$

In the unpublished lecture “On Theoretical Terms and Theoretical Entities”, held in front of the NY Philosophy Club on 21 January 1966 and thus four months before the Minnesota conference, Hempel repeats this construction.⁴ Now, Hempel (1970, 142) notes that the Minnesota lecture “modifies in certain respects” the East Lansing lecture, but it would be odd if over the course of four months Hempel had come to the conclusion that the correspondence rules in the Received View are given in the metalanguage, especially since he could have just looked up their status in the article he had revised two years earlier for republication. Furthermore, the criticism is not mentioned in the Urbana summary, and in the Bucharest lecture, Hempel (1973b, 373) states:

The interpretative sentences of a theory have been variously construed as formulated within the language of science or as sentences effecting a semantical interpretation and thus belonging to a suitable metalanguage. My remarks will be limited to the first, more common, construal[.]

Thus it seems that Hempel himself fielded this criticism only for a short period of time.

As far as the philosophy of science is concerned, though, it is just important that the presumption of Hempel’s criticism is also clearly false, since he himself assumed that correspondence rules are expressed in the same language as the theoretical sentences of the theory when working within the Received View, and so did Rudolf Carnap in all of his works since at least “Testability and Meaning” (Carnap 1936, 1937). So Hempel’s new view on correspondence rules is not a departure from the Received View, but is the Received View.

4 Observational terms

The current discussion of observational terms usually focus on the difficulty of clearly demarcating observational terms (e. g. Maxwell 1962). This is not Hempel’s criticism. He instead states in the Minnesota lecture (Hempel 1970, 143–44):

The antecedently examined phenomena for which a theory is to account have often been conceived as being described, or at least describable, by

4. Carl Gustav Hempel Papers, University of Pittsburgh, Identifier 31735062224286, digital.library.pitt.edu/islandora/object/pitt%3A31735062224286, page 3.

means of an observational vocabulary, i. e., a set of terms standing for particular individuals or general attributes which, under suitable conditions, are accessible to “direct observation” by human observers.

[But] the antecedent vocabulary of a given theory will often contain terms which were originally introduced in the context of an earlier theory, and which are not observational in a narrow intuitive sense.

In an comparably elaborate example, Hempel (1970, 144–45; 1969b, 53–54) notes that in the case of Bohr’s theory of the hydrogen atom entailing the Balmer formula for the wavelengths emitted by hydrogen, the description of the wavelengths is not given in directly observational vocabulary, but rather the antecedent vocabulary of earlier theories. This is not the first time Hempel uses this example. It also occurs in his textbook *Philosophy of Natural Science* (Hempel 1966, §6.2), a “substantial part of [which] was written in 1964, during the last months of a year [Hempel] spent as a Fellow of the Center for Advanced Study in the Behavioral Sciences” (vii), and thus probably right after finishing the revision of “The Theoretician’s Dilemma” for republication, and before giving his endorsement of logical positivism’s philosophy of science in the Johns Hopkins seminar. So Hempel arguably did not always see the use of antecedent vocabulary from earlier theories as a problem for the Received View.

As Hempel’s criticism in the Minnesota lecture presumes, Hempel (1958, 179) states in “The Theoretician’s Dilemma” that in “regard to an observational term it is possible, under suitable circumstances, to decide by means of direct observation whether the term does or does not apply to a given situation.” However, Hempel (1958, 42–43) cautions that this characterization

offers no precise criterion by means of which any scientific term may be unequivocally classified as an observation term or as a theoretical one. But no such precise criterion is needed here; the questions to be examined in this essay are independent of precisely where the dividing line between the terms of the observational and the theoretical vocabularies is drawn.

So Hempel did indeed mean to distinguish between directly observational terms and theoretical terms when applying the Received View, but this does not immediately entail that his arguments actually rely on this. The preliminary discussion (§§1–8) makes up the bulk of the article and focuses on the successive liberalization of the correspondence rules. With the exception of the claim that disposition terms cannot be defined in observation terms (§6), the arguments are independent of any particular dividing line. The only assumption is that observation terms are interpreted, while theoretical terms are not. Analyzing the eponymous dilemma, Hempel (1958, 73) makes this independence explicit: He does not distinguish between observational and theoretical terms at all, but rather between a basic vocabulary V_B and a theoretical vocabulary V_T and states that

we will have to assume, of course, that V_B consists of terms which are antecedently understood. They might be observational terms, in the somewhat vague sense explained earlier; but we need not insist on this.

Thus Hempel did provide a (vague) demarcation of observational terms, but, crucially, did not rely on it in his analysis. There is even a telling terminological slip-up in which Hempel (1958, 84) speaks of “an observational vocabulary V_B ”, suggesting that he even thought of observational terms simply as interpreted. And in the East Lansing lecture, Hempel (1969b, 54) was also still clearly aware of this, as he writes immediately after the example of the Balmer formula that,

while the rules of correspondence, R , are often said to connect theoretical terms with observational ones, several writers who have made expository use of that schema have clearly envisaged a less stringent conception that is quite compatible with the one just outlined.

Hempel was one of these writers, as just noted, and so was Carnap, who early on allowed for R connecting theoretical sentences with an “inferred primary protocol” involving antecedent terms (Carnap 1931, 437–38) and later simply assumed more and less elementary (i. e., observational) terms (Carnap 1939, §24), as Hempel (1951, 68, n. 11) himself also notes. Hempel (1963, VII) also points out that Carnap (1932) “early rejected the idea of a privileged class of ‘protocol sentences’ conceived as terminal statements in the process of empirical verification”.

Thus Hempel’s criticism of the Received View on observational terms is based on a straw man as well, and his new, competing view amounts to the one Carnap held, and he himself held when still working within the Received View.

5 The theory as an abstract calculus

In Hempel’s account, a theory is in the Received View reconstructed as containing a set C of uninterpreted sentences, an abstract calculus. But the use of an abstract calculus is wrong, according to Hempel, both because it is abstract, and because it is a calculus.

5.1 The theory as abstract

Hempel (1970, 153) states in the Minnesota lecture that

the internal principles of most scientific theories employ not only “new” theoretical concepts but also “old,” or pre-theoretical, ones that are characterized in terms of the antecedent vocabulary. For the theoretical scenario is normally described in part by means of terms that have a use, and are understood, prior to, and independently of, the introduction of the theory.

As a criticism of the Received View as described by Hempel, it is a decidedly odd one: Hempel’s *new* view distinguishes between antecedent vocabulary and vocabulary introduced by the theory in question. The Received View, according to Hempel, distinguishes between directly observational and non-observational vocabulary. According to Hempel’s description of the Received View, then, two theories could share theoretical terms, since theories can share terms that are not directly observational. So the criticism Hempel aims at the Received View here is, if anything, a criticism of his own new view.

Of course, the previous section established that Hempel himself, in “The Theoretician’s Dilemma”, relies on a distinction between antecedent and (new) theoretical terms, where the antecedent terms may but do not have to be observational. But even if one assumes that the Received View requires a new theoretical vocabulary, Hempel’s criticism of a theory as abstract is a non-sequitur as long as C is not fixed independently of the requirement that it contain only new vocabulary, for then C can simply be taken to fulfill the requirement by definition. Hence Hempel (1970, 153) adds that

[t]he assumption, in the standard construal, of an axiomatized uninterpreted calculus as a constituent of a theory [...] suggests that the basic principles of a theory—those corresponding to the calculus—are formulated exclusively by means of a “new” theoretical vocabulary[.]

So Hempel conjectures that in the Received View, the abstract calculus describes the “basic principles of a theory”. But apart from it being a conjecture, it is also not clearly defined as long as it is not clear how to identify the basic principles of a theory. Hempel might have had in mind the description of a theory as it is found in textbooks and research papers, like Bohr’s description of his model of the hydrogen atom.

With these assumptions in place, the Received View indeed entails that all terms used in a theory are uninterpreted. But the actual Received View does not entail this claim. At best, Hempel here goes against his own admonition to critics of the Received View to “not focus on the bold and naturally oversimplified devices its adherents wrote upon their banners, but on the quality of the detailed logical and methodological studies carried out under those banners” (Hempel 1969a, §6): While sometimes, proponents of the Received View have spoken of the terms of the theory as uninterpreted (see Lutz 2012b, 77–78, for a current example), this cannot have been an assumption in the Received View. For one, the correspondence rules of one theory are often entailed by another theory, for instance a theory describing a measurement device; Carnap (1926, 30), for instance, in a monograph discussed by Hempel (1952, §§10–11, n. 62, 64), relies on a theory about the expansion of materials with temperature to derive a correspondence rule for the notion of length. A theory that is to entail a correspondence rule then obviously has to contain interpreted terms, otherwise it could not connect the uninterpreted terms to interpreted ones.

Furthermore, there is in general no reason for a theory to be restricted to non-observational terms; rather, the basic principles of a theory can connect theoretical and observational terms, and correspondence rules can be used for those terms that have a stronger connection to observation terms than can be inferred from the basic principles of the theory alone. All analyses within the Received View that I am aware of allow for this. Prominent examples are Craig’s theorem and the Ramsey sentence, which are simply developed for $T = C \cup R$, that is, the whole of the theory including both abstract calculus and correspondence rules. These analyses are central in “The Theoretician’s Dilemma”, where Hempel (1958, §9) relies only on the conjunction of abstract calculus and correspondence rules as the “interpreted theory” T' .

5.2 The theory as a calculus

Hempel is against the reconstruction of the basic principles of a theory as uninterpreted, but he is also against their reconstruction as a calculus. Or, more precisely, he is against the simple *assumption* that a theory is formalized in a calculus. In the Urbana lecture, Hempel (1974, 247–48) states:

[T]he fact that certain philosophical issues may be clarified by an appropriately chosen axiomatization of some particular theory surely does not suffice to justify the idea embodied in the standard construal, that the internal principles of any scientific theory be conceived as being axiomatized in some unspecified way.

This is developed more fully in the Minnesota lecture, where Hempel (1970, 152) states that

whatever philosophical illumination may be obtainable by presenting a theory in axiomatized form will come only from axiomatization of some particular and appropriate kind rather than just any axiomatization or even a formally especially economic and elegant one.

For Hempel, then, whatever advantages a formalization has can come about only by axiomatizing a theory in a specific way, based on philosophical insights that precede the axiomatization.

As a first criticism, Hempel (1970, 148–49) notes that while an arbitrary axiomatization provides “a general criterion determining, for any sentence S , whether S is asserted by the theory”, it “does not provide us with a general method of actually finding out whether a given sentence” is so asserted, because first order logic is not decidable. As a criticism of axiomatization it is remarkably poor, given that, first, there is no better replacement and, second, establishing the precise content of scientific theories is so difficult that the lack of decidability registers barely, if at all. Hempel here seems to be suggesting that because the water bucket may spill, we should instead use paper bags or just let the house burn down.

But Hempel (1970, 152) also gives examples for insights that can come only from appropriate axiomatizations. The axioms for a set of sentences, for instance, can to some extent be chosen at will, and thus do not automatically, simply in virtue of being axioms, express the foundational assumptions of the theory. To do that, the axioms have to be chosen based on philosophical analysis. Similarly, an axiomatization does not identify any terms as primitive, and even a chain of definitions can to some extent (when terms are interdefinable) be chosen at will. Thus both primitive terms and the order of definition must be chosen based on philosophical analysis.

However, this is a proof by example or, in other words, a non-sequitur: That the assumption of axiomatization does not entail answers to some questions does not warrant the conclusion that the assumption entails no answers at all. And when relying on the Received View, Hempel himself only required the assumption that the theory is axiomatized in some way or other to gain philosophical insight.

In “The Theoretician’s Dilemma”, Hempel (1958, §9) discusses the functional replaceability of theoretical terms. His first method replaces the theory T by a set of sentences containing a sentence $O_1 \rightarrow O_2$ for every deducibility relation $O_1, T \vdash O_2$, in which T is used to derive observation statement O_2 from observation statement O_1 . By construction, the resulting set does not contain theoretical terms, and Hempel proves that it establishes all and only those deducibility relations that T establishes. Hempel’s proof for this claim relies only on the assumption that the theory is axiomatized in first order logic. This entails, for instance, compactness, so that Hempel can assume that all sets of sentences in the premises can be expressed by T and a single observation statement. The assumption of axiomatization also entails the deduction theorem and its converse, so that Hempel can use that $O_1, T \vdash O_2$ if and only if $T \vdash O_1 \rightarrow O_2$.

The second method that Hempel discusses relies on Craig’s theorem, which states that at every recursively enumerable theory in first order predicate logic has a primitive recursive axiomatization (Craig 1953). Craig’s theorem also requires no further assumption about how, specifically, the theory is axiomatized.

The third method Hempel discusses is Ramsey’s elimination method for theoretical terms (Ramsey 1931, §4), which existentially generalizes on all theoretical terms. The resulting Ramsey sentence entails the same observational sentences as the original theory, which can be proved by only assuming that the theory is axiomatized in predicate logic.

These are of course just some of the features that all axiomatizations in predicate logic have in common. Others are monotonicity, truth-functionality, the possibility of defining a Tarski semantics (also used by Hempel 1958, 82), and so on. Thus, while Hempel does not misrepresent himself in his claim that the analysis of the Received View sometimes assumed any axiomatization whatsoever, he could have found counterexamples to his conclusion that such an assumption can provide no philosophical insight in his own writings.

There are two other problems with the criticism: One is that the Received view is a framework in which to reconstruct theories (Lutz 2012b, §5.2). Thus even if the assumption of the Received View had no philosophical implications, the actual reconstructions could have. Hempel (1970, 150) even states that Reichenbach’s axiomatization of general relativity in the Received View was “philosophically stimulating and illuminating”. Finally, it is difficult to see how Hempel’s new view can escape his criticism of the Received View: In his new view, $T = c(I \cup B)$, which very much suggests that I (and B) are in some sense axiomatized, since otherwise their deductive closure would be ill-defined. And even though the new view is not supposed to be a fully developed alternative, it is still supposed to show the failures of the Received View.

6 Meaning

Hempel’s main criticism of the Received View arguably concerns the way it assumes that theoretical terms receive meaning. This is the criticism that Hempel (1973b) singles out in his Bucharest lecture, the basic point being that the Received View gets the role of both the correspondence rules and the abstract calculus wrong. In the Minnesota lecture,

Hempel (1970, 149) states that in the Received View

the meanings of theoretical terms are determined in part by the postulates of the calculus, which serve as “implicit definitions” for them; and in part by the correspondence rules, which provide them with empirical content.

About the correspondence rules R , Hempel (1970, 159) conjectures:

[R 's] designation as operational *definitions*, coordinative *definitions*, or *rules* of correspondence conveys the suggestion that that they have the status of metalinguistic principles which render certain sentences true by terminological convention[.] But such a conception of correspondence rules is untenable for several reasons[.]

This analysis of correspondence rules as conventions is repeated in the Bucharest lecture (Hempel 1973b, §5), in which Hempel (1973b, 370) adds a criticism of the abstract calculus C :

[W]hat exactly is being asserted by this doctrine of implicit definition for theoretical terms? The term ‘definition’ suggests terminological convention or legislation[. But if] the truth of the theoretical postulates were enforced by terminological *fiat* then the entire theory would be made true a priori; it could be known to be true independently of any empirical evidence—and regardless, moreover, of what interpretations the correspondence rules may assign to the empirical terms.

Thus in the Received View, both R and C are supposed to be true by convention, but they cannot be if the theory as a whole is to have empirical content.

A good hint that Hempel’s overall analysis of meaning in the Received View is flawed comes from his own Bucharest lecture. Hempel (1973a, §4) recounts that originally, correspondence rules were meant to be explicit definitions of theoretical terms in observation terms, but Carnap soon found explicit definitions too restrictive for correspondence rules, so he introduced reduction sentences, essentially necessary or sufficient conditions for theoretical terms. Hempel (1970, 161) recounts:

Carnap pointed out in his theory of reduction sentences that when a term is introduced [...] by means of several reduction sentences, the latter taken jointly normally have empirical implications.

So Carnap himself pointed out that correspondence rules could not be conventions. Carnap (1936, 451) calls the logically strongest observation sentence entailed by the conjunction of two reduction sentences their “representative sentence”, which Hempel (1958, 71) recounts in “The Theoretician’s Dilemma”. Carnap (1952, 71) later suggested taking the material implication with the representative sentence in the antecedent and the conjunction of the reduction sentences in the consequent as the (analytic) meaning postulate for the introduced term. Hempel (1963, 705) himself discusses this “interesting variant of the method of introducing predicates by reduction sentences” in a text whose first version

was written in 1954 (Carnap 1963, 961) and thus before “The Theoretician’s Dilemma”, but he does not mention it in any of the four lectures.

In his response to Hempel’s criticism of the analytic-synthetic distinction (Hempel 1963, VII), Carnap (1963, 24.D) spells out a generalization of this procedure for whole theories:⁵ A theory’s Ramsey sentence (which generalizes the representative sentence) describes the synthetic component of a theory, and a theory’s by now so-called Carnap sentence, a material implication with the Ramsey sentence as the antecedent and the theory as the consequent, describes its analytic component.

To my knowledge, Hempel refers to Carnap’s general solution for the analytic-synthetic distinction only once, in a symposium on occasion of Carnap’s death (Hempel 1973a, 260). Ironically, the symposium was published the same year as the Bucharest lecture, in which Hempel (1973a, §6) concludes that the Received View’s analysis of the meaning of theoretical terms mistakenly assumes that their meaning must be specified linguistically. But this assumption, he states,

calls for the singling out, from among all the sentences asserted by a theory, of certain sentences which interpret the theoretical terms, and which hold true by virtue of linguistic legislation: and this general notion cannot be made good.

In the Minnesota lecture, Hempel (1970, 159) gives as one advantage of his new view that the “concept of bridge principle as invoked in [his new] characterization of theories does not presuppose the analytic-synthetic distinction and treats the bridge principles [as] on a par with its internal principles”. But of course, the same holds true for the Received View.

So Hempel has missed a core development of the Received View. In early versions of the Received View, some proponents may have assumed that the analysis of theories has to proceed through a *specific* kind of formalization—the correspondence rules were explicit definitions and could thus be analytic (as long as each of them defined a different theoretical term), and the abstract calculus could indeed be completely abstract, with the meaning given by the explicit definitions. But at the latest with Carnap’s distinction between pairs of reduction sentences on the one hand and their synthetic representative sentences and analytic meaning postulates on the other, there was no such assumption anymore. And with the development of the method of the Ramsey and Carnap sentences, the reconstruction of a scientific theory had become completely untethered from the analytic-synthetic distinction: One can just axiomatize the basic principles of a theory as they are found in textbooks and research papers, resulting in the calculus C , which is not necessarily uninterpreted. For all of a theory’s terms that can be independently measured, one can axiomatize the respective measurement procedures, resulting in the correspondence rules R . It is only after the reconstruction that the Ramsey sentence of the theory $C \cup R$ provides the theory’s synthetic component and the Carnap sentence provides its analytic component, the meaning of the theoretical terms.

⁵ Carnap’s first publication of the idea (Carnap 1958, 245, n. 3) refers for details to this response, to be published in a collection described as “in preparation”.

In summary, Hempel recounts Carnap's discussion of empirical implications of correspondence rules, in which Carnap presents an account of constructing analytic meaning postulates from conjunctions of reduction sentences, Carnap replies to Hempel's criticism of the analytic-synthetic distinction by generalizing this account to theories in general, and Hempel in his criticism of the Received View still ignores the central point of Carnap's account: The distinction between correspondence rules and abstract calculus is not the same as the distinction between analytic and synthetic components of theories.

7 Conclusion

I have previously been publicly puzzled by Hempel misrepresenting Carnap as relying on first order logic for the reconstruction of scientific theories (Lutz 2012b, 84–85, 87), but the preceding discussion suggest that it is just one example of a large pattern of misrepresentation. Hempel's criticisms of the Received View on correspondence rules, observation terms, and theoretical sentences misconstrue the Received View as he himself assumed it, and contrast it with a new, allegedly more correct view which in these respects is identical to the actual Received View. Hempel's criticisms of the use of axiomatizations in the Received View is simply mistaken, as is exemplified by his own use of axiomatizations in the Received View. And his criticisms of the meaning of theoretical terms and the distinction between correspondence rules and basic theoretical principles in the Received View ignore its development after its earliest versions, a development that Hempel witnessed and commented on when still relying on the Received View.

8 Epilogue: On Suppe on Hempel on Hempel

Unless we ourselves take a hand now, they'll foist a republic on us. If we want things to stay as they are, things will have to change.

Tancredi Falconeri to Don Fabrizio⁶

This is the evidence promised at the outset. But why would Hempel misrepresent his own and Carnap's position on scientific theories? Here I can but speculate. Friedman (2003) notes how Hempel was very close to Neurath's naturalism during the time of the Vienna Circle, and how he followed Carnap in focusing on explication while in close contact with him. Under the influence of Kuhn, who joined him in Princeton, Hempel swayed back to a naturalistic description of science. One possibility is that Hempel simply displayed the zeal of the convert, and as a result engaged in motivated reasoning against the Received View. The weakness of his arguments from the metalinguistic status of the correspondence rules and the undecidability of first-order axiomatization point in this direction. But his misrepresentations might also be the result of something like

6. Giuseppe Tomasi di Lampedusa. 1960. *The Leopard*. New York: Pantheon Books. Translated by Archibald Colquhoun

a Gestalt-switch in Hempel’s thinking: It might be that his renewed proximity to naturalism was something of a paradigm shift in Kuhn’s sense, and Hempel had thereafter difficulties appreciating the basic assumptions of the previous paradigm, the Received View.

Another possibility is that Hempel was aware of the political maneuvering, with war as its extension by other means, that was taking place around the Received View. Recall the description of logical positivism’s demise by Suppe (2000, S102):

Carl Hempel [...] was expected to present the Received View’s latest revision. Instead he told us why he was abandoning both the Received View *and* reliance on syntactic axiomatizations (Hempel 1974).

And now consider again Hempel’s criticism of treating a theory as a calculus. Hempel was not abandoning syntactic axiomatizations, but the assumption of some not further specified axiomatization, *syntactic or not*. Suppe, however, wants to present the competing semantic view, which includes the set-theoretic axiomatizations by Patrick Suppes, as having won the war, and so he relies on an old trick of rhetoric, the misuse of conversational implicature: By needlessly restricting Hempel’s position to *syntactic* axiomatizations, Suppe implicates that Hempel still accepts axiomatizations in general, and thus specifically the semantic ones. But Hempel (1970, 150) introduces his criticism of axiomatizations stating that “Suppes has argued that formalizing and axiomatizing scientific concepts and theories is ‘a primary method of philosophical analysis’”, to then conclude that “whatever philosophical illumination may be obtainable by presenting a theory in axiomatized form will come only from axiomatization of some particular and appropriate kind rather than just any axiomatization”. So Hempel explicitly includes Suppes’ axiomatizations in his critique, and thus Suppe misleads, although he does not lie. Political maneuvering again.

So here is a particularly speculative speculation⁷. If Hempel was aware of the political maneuvering and could see the end of the Received View as an accepted approach to research in the philosophy of science, it would have made sense to distance himself as much as possible from the Received View while taking over as much of it as possible into an allegedly new view. For his extant analyses to stay as they were, things would have to change.

I am not endorsing any of these speculations, but I think they are more plausible than that Hempel simply forgot what he had been doing a year earlier. Maybe the most plausible option, however, remains that I am misrepresenting Hempel, for otherwise we would have to accept that Hempel misrepresents Hempel.

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⁷. Which I owe to Alexander Linsbichler, who also suggested the introductory quote.

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