On a Straw Man in the Philosophy of Science
—A Defense of the Received View—

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Abstract

I defend the Received View on scientific theories as developed by Carnap, Hempel, and Feigl against a number of criticisms based on misconceptions. First, I dispute the claim that the Received View demands axiomatizations in first order logic, and the further claim that these axiomatizations must include axioms for the mathematics used in the scientific theories. Next, I contend that models are important according to the Received View. Finally, I argue against the claim that the Received View is intended to make the concept of a theory more precise. Rather, it is meant as a generalizable framework for explicating specific theories.

1 Introduction

In the Received View on scientific theories, developed in the tradition of logical empiricism for example by Rudolf Carnap, Carl Gustav Hempel, Herbert Feigl, and Ernest Nagel, scientific theories are represented as sets of sentences (called *theoretical sentences*) in predicate logic containing only the terms of the theory. These *theoretical terms* are then connected to *observation terms* through sets of *correspondence rules*, sentences that contain both theoretical and observation terms. The observation terms are given a semantic interpretation, which, through the correspondence rules and the theoretical sentences, restricts the possible semantic interpretations of the theoretical terms.

The Received View on scientific theories (the name is due to Putnam 1962, 240), also called the Syntactic Approach (Van Fraassen 1970) or Syntactic View (Wessels 1974, 215), the Standard Conception (Hempel 1970), or the Orthodox View (Feigl 1970), is currently not

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an accepted framework for the reconstruction of theories (cf. Suppe 2000, §1). Accordingly, any analysis or argument that presupposes the view will meet with strong opposition or simply disinterest on this count alone. If the Received View is indeed as unmitigated a disaster as it seems to many, this state of affairs is just as it ought to be. On the other hand, if the Received View is a fruitful framework for the reconstruction of scientific theories, this state of affairs is unfortunate, not only because it is a good thing to have as many means for the analysis of theories at one's disposal as possible, but also because there have been many analyses that presuppose the Received View, and whose results, if correct, would provide deep and helpful insights into the structure of theories.

For example, Hempel and Oppenheim (1948) and subsequent authors gave increasingly sophisticated and general explications of 'explanation' on the basis of the Received View. Their explications have now been succeeded by a host of mutually incompatible accounts, which not always reach the precision and breadth of the original line of explications. Similarly, the explication of 'reduction' by Nagel (1949, 1951) has not been improved upon so much as abandoned, and in many contemporary discussions, the term is not explicated at all but used in a rather intuitive way. The criteria of empirical significance helpfully summarized and then rejected by Hempel (1965c) have never recovered their status. Closely related to the notion of empirical significance is the notion of empirical content. Here, the explications developed by Craig (1953) and Ramsey (1929) have not been superseded in a generally accepted way, but neither are they being employed too often. In part building on the results of Ramsey, Carnap (1952, 1958) developed explications of 'analyticity', a concept which is now often considered impossible to explicate, of no use, empty, or some combination thereof.

Explications like these are generally not given up without a good reason, and indeed there have been severe criticisms of the Received View. It is the purpose of this article to defend the Received View as it emerges from the later writings of Carnap, Hempel, and (to a smaller extent) Feigl against some of these criticisms. I will not actually discuss the criticisms' conceptual merits, however, but rather their presumptions about the Received View. Many of these, I argue, are contentious or wrong, making the criticisms attacks on a straw man.

Many misconceptions of the Received View could be avoided by reference to the by now canonical exposition[1] by Suppe (1974a), for example the idea that according to the Received View, "[n]othing non-syntactic constitutes any theory. In particular, whatever is meant or referred to by a term or statement (i. e., the semantic aspect), it is not constitutive of theories" (Burgos 2007, 157). To the contrary, Suppe (1974a, §II.E) correctly emphasizes that the Received View includes a semantic interpretation.

But some misconceptions are not forestalled by Suppe's exposition, and two of those have become the basis of repeated, explicit criticism. One is the alleged demand in the Received View for exhaustive axiomatizations, the other is its perceived dismissive attitude towards

[1]Canonical within the philosophy of science (see, for example, Hughes 1989, 80; Morrison and Morgan 1999, 2, n. 1; Muller 2010, §2, n. 1), and sometimes even the history of philosophy of science (cf. Uebel 2008, §3).

models. Unfortunately, even Suppe's generally careful exposition contains mistakes that led to unjustified criticism. In this article, I will discuss his claims that the Received View demands axiomatizations in first order logic and that it is meant to make the meaning of the term 'theory' more precise.

2 The exclusive use of first order logic

According to Suppe (1974a, 16, 50), proponents of the Received View at all times assumed an axiomatization of scientific theories in first order predicate logic or, in Carnap's later writings, first order logic augmented by modal operators. In current summaries of the Received View, the option of modal operators is typically not mentioned, unlike the demand for first order logic (see, for example, Thompson 1988, §2; Morrison and Morgan 1999; van Fraassen 2000; Hendry and Psillos 2007, §2; Frigg and Hartmann 2008, §4.1; Muller 2010, §2).

Suppes (1967, 56f) also speaks of the "standard sketch" of scientific theories, by which he obviously means the Received View, and later discusses "standard formalizations", axiomatizations in first order logic (Suppes 1967, 58), without explicitly identifying the two (some authors who cite the text do so, however, for example Beatty (1980, appendix 1) and Suppe (1974a, 114, §IV, F, n. 241)). In his discussion, Suppes (1967, 58) puts forth what has become one of the standard criticisms of the reliance on first order logic: "Theories [...] like quantum mechanics [...] need to use [...] many results concerning the real numbers. Formalization of such theories in first-order logic is utterly impractical". This impracticality of first order axiomatization is, next to the perceived need for an exhaustive axiomatization discussed below, one pillar of Stegmüller's criticism of the Received View (Stegmüller 1978, 39f).

Beyond being cumbersome, first order logic has a deficiency that even in theory cannot be avoided: It cannot determine structures with an infinite domain up to isomorphism. As Suppe (2000, §3) puts it, "[a] general problem [with the Received View] was that the Löwenheim-Skolem theorem implied that [the theory's] models must include both intended and wildly unintended models." One way to avoid this problem is to simply determine the theory's models directly by defining a set-theoretical predicate as suggested by Suppes (1992, §2). In this so called Semantic View on theories, one defines in the metalanguage the structures that are to be the models of the theory, rather than giving a set of sentences in the object language, the models of which are the models of the theory. In the metalanguage, it is then possible to determine the models of the theory up to isomorphism. In this respect, the Semantic View is then superior to the Received View, as van Fraassen (1989, §8.6) also stresses: "[W]hen a theory is presented by defining the class of its models, that class of structures cannot generally be identified with an elementary class of models of any first-order language". French and Ladyman (1999, 116f) also make this point.

I do not want to discuss whether this criticism is valid, that is, whether something essential about scientific theories is missed when they are described in first order predicate logic (cf.

Pearce and Rantala 1985, 135). Rather, I want to note that in none of the sources that Suppe (1974a, §II.E, n. 107) cites for the final version of the Received View (Carnap 1956b, 1958, 1963b, 1966, Hempel 1965f, 1963), the Received View is restricted to first order logic, and that indeed there is clear evidence that Hempel and Feigl allowed infinite type theory, and overwhelming evidence that Carnap, from the beginning of the Received View to its final version, assumed infinite type theory.

Suppe (1974a, 12) notes that an article by Carnap (1923) seems to be the first published version of the Received View, and Feigl (1970, 3) credits it as the second, after a work by Campbell (1920). Both works were published after Hilbert, in a talk, isolated first order logic as a distinct subsystem of logic, but before this result appeared in print (cf. Anellis 1996). Arguably, the distinction between first order logic and type theory was completely clarified only by 1935 (Hodges 2001, 1–4).[2] Hence it is unsurprising that Carnap's article does not mention first order logic or any restriction of the mathematical formalism that could point towards the restriction to first order logic, except that he refers to the ideal physics as an "axiomatic system of pure deduction"[3] (Carnap 1923, 103). Syntactic deduction is complete for first order logic, but not for type theory (Gödel 1930, 1931), and relying only on syntactic deduction essentially reduces the expressive power of type theory to that of first order logic (Henkin 1950). Thus Carnap in effect restricts himself to first order logic when relying only on syntactic deduction. But this should not be read as an exclusive endorsement of first order logic, for, first, the concept had not been clarified at that point, second, these relations were not known at that point, and third, Carnap thought that type theory was complete. Indeed, given his focus on type theory in his research on axiomatics, it would be rather surprising if Carnap demanded first order axiomatizations (see Reck 2007).

In Carnap's later writings, demanding a restriction to first order logic would be incompatible with his logical pluralism stated, for example, in his principle of tolerance (Carnap 1934, §17). Carnap (1956b, §II) considers different restrictions on the *observational* part of the language (since this part is considered to be completely interpreted by observation), but the theoretical language (L_T) is unrestricted (Carnap 1956b, 46):

> For L_T we do not claim to have a complete interpretation, but only the indirect and partial interpretation given by the correspondence rules. Therefore, we should feel free to choose the logical structure of this language as it best fits our needs for the purpose for which the language is constructed.

Carnap would accept or reject a logic for the description of a theory based on the logic's expedience. Therefore, Carnap could at most be mistaken about the expedience of type theory.

But Carnap is very much in agreement with Suppes (1967, 58) on the inexpedience of first

[2] I thank Albert Visser for discussing this issue and pointing me to Hodges's text.

[3] "Bei dieser Abgrenzung des Wissenschaftsgebietes [. . .] hat *Haas* Recht, die ideale [Physik] als axiomatisches System reiner Deduktion aufzufassen."

order logic, because he explicitly suggests infinite type theory for reconstructions: Already in the *Aufbau*, Carnap (1928, §30) writes that

> Russell has applied his theory [of types] only to formal-logical structures, not to a system of concrete concepts (more precisely: only to variables and logical constants, not to nonlogical constants). Our object spheres are Russell's "types" applied to extralogical concepts.

Since the concept of object spheres is used throughout Carnap's reconstruction, there is no question about Carnap's stance on its expedience.

It is clear that Carnap's later writings on the Received View allow for type theory, because each of the works on which Suppe's exposition is based either explicitly uses infinitely typed logic or refers to a work that does for further elucidation. Carnap (1956b, 43) for example conjectures that "acceptance of the following three conventions C1–C3 is sufficient to make sure that L_T includes all of mathematics that is needed in science":

> Conventions about the domain D of entities admitted as values of variables in L_T.
>
> C1. D includes a denumerable subdomain I of entities.
>
> C2. Any ordered n-tuple of entities in D (for any finite n) belongs also to D.
>
> C3. Any class of entities in D belongs also to D.

C1 includes objects in the domain, C2 includes relations of any elements of the domain in the domain, and C3 includes classes of any elements of the domain in that domain. The result is that any predicate or relation in the domain can be in the scope of another predicate or relation, and this is just infinite type theory. Given the incompleteness of type theory, Carnap (1956b, 51, 62) relies on semantic entailment rather than syntactic deduction for his rules of inference.

The article that includes this construction was triggered by the first version (from 1954) of Hempel's discussion (Hempel 1963) of Carnap's philosophy of science (Carnap 1963b, §24.B). Accordingly, Carnap (1963b, §24.A, n. 38) refers to this work when he responds to Hempel's discussion. Carnap also points out some differences between his presentation and Hempel's, but none with respect to the use of type theory.

Carnap (1963b, §24.C, n. 41) also refers to another elaboration of his position. There, Carnap (1958, 237, my translation) is even more explicit about his logical assumptions:

> Let the structure of L_T be such that it contains a *type theory* with an infinite series of domains D^0, D^1, D^2, etc.; D^n is called n^{th}-level domain. Each variable and each constant belongs to a specific type. Each variable of type n has D^n as its domain and each constant of type n refers to an element of D^n.[4]

[4] "Die Struktur von L_T sei so, dass sie eine *Stufenlogik* mit einer unendlichen Folge von Bereichen D^0, D^1, D^2, usw. enthält; D^n heisst der Bereich n-ter Stufe. Jede Variable und jede Konstante gehört zu einer bestimmten Stufe. Jede Variable n-ter Stufe hat D^n als Wertbereich, und jede Konstante n-ter Stufe bezeichnet ein Element von D^n."

In his popular introduction to the philosophy of science, Carnap (1966, 253, n. 2) refers to this text as an elaboration of his position and repeats the construction.

There is no such very explicit endorsement of type theory in the works of Hempel, but he also does not dismiss it. Specifically, he does not distance himself from Carnap's use of type theory, even when he refers to the articles that contain the explicit statements quoted above (Hempel 1965f, 194–197; Hempel 1963, §I, n. 2).

There are some places in Hempel's writings where he analyzes axiomatizations given in first order logic, and he attaches quite some weight to the results. For example, in his discussion of the eliminability of theoretical terms, Hempel (1965f, 211f) states that Craig's theorem applies "provided that [the theory] satisfies certain extremely liberal and unconfining conditions". Elsewhere, Hempel (1963, 698) states that Craig's theorem applies "in a very comprehensive class of cases". But, as Hempel himself notes, Craig's theorem assumes first order logic.

On the other hand, Hempel (1973, 264f) states later, after having abandoned the Received View, that

> the precisely characterized languages by reference to which certain philosophical problems have been studied are often distinctly simpler than those required for the purposes of science. For example, Carnap's theory of reduction and confirmability, and his vast system of inductive logic are limited to languages with first-order logic, which certainly does not suffice for the formulation of contemporary physical theories. The same remark applies to various studies by Carnap and other empiricists that deal with the structure and function of scientific theories [...].

I confess that I do not know to which of Carnap's studies on the structure of scientific theories Hempel refers. Without being seriously mistaken, he cannot mean Carnap's expositions of the Received View that contain the above quotations. And the last conception of reduction and confirmability[5] in which Carnap (1937) relies on first order logic was quickly followed by a generalization in which Carnap (1938, 1939) allows correspondence rules of any logical kind for reduction (see also Carnap 1963a, 424).

This misrepresentation of Carnap's views notwithstanding, the passage shows that at least in 1973 Hempel is of the opinion that a restriction to first order logic puts more than an extremely liberal and unconfining condition on scientific theories. So either Hempel has changed his mind, or he is of the opinion that even without covering physical theories, first order logic suffices for a "very comprehensive class" of theories. Or, but this is somewhat far-fetched, he is of the opinion that those sentences of a theory in type theory that are entailed by, but not deducible from, the theory are not part of its observational content. Then the observational content of a theory described in type theory can be captured by transcribing the theory into first order logic (Henkin 1950, cf. Leivant 1994, §5.5) and applying Craig's theorem.

[5]See Gemes (1998, §1.4) for the curious relation between the two via the concept of empirical significance.

All the preceding claims by Hempel about first order logic are descriptive, that is, he only states what logics are or are not confining for the axiomatization of scientific theories. At no point does he *demand* that axiomatizations avoid type theory. Instead, Hempel (1965f, 201f) uses type theory himself to arrive at explicit definitions for real-valued measurement results in observational terms. He notes that using type theory "will be considered too high [a price] by nominalists", but adds that "it would no doubt be generally considered a worthwhile advance in clarification if for a set of theoretical scientific expressions explicit definitions in terms of observables can be constructed at all". And just like Carnap, Hempel is of the opinion that to be properly observational, a language must not have too strong a logical apparatus: He cautions that "the definiens will normally be teeming with symbols of quantification over individuals and over classes and relations of various types and will be far from providing finite observational criteria of application." Of course, he goes on to give the definition anyway, thereby showing that he did not consider exclusive reliance on first order logic a necessity.

Besides using type theory himself, Hempel also considers other axiomatizations that go beyond first order logic to be consistent with the demands of the Received View. Like Feigl (1970, 8), Hempel (1970, §3) lists the axiomatizations given by Suppes as acceptable within the Received View, and he explicitly notes that they use "set theory and mathematical analysis" which are "more powerful" than first order logic. While this may be questioned, since there are first order axiomatizations of set theory and (usually through the use of set theory) of mathematics in general, it shows that Hempel does not consider exclusive reliance on first order logic necessary. The clearest example, however, is the axiomatization of parts of cell biology by Woodger (1939). Both Feigl (1970, 8) and Hempel (1952, 687) accept his axiomatization as a possible reconstruction, and Woodger (1939, §III) explicitly uses type theory.

Finally, Hempel (1970, §2, n. 4) also gives a list of expositions of the Received View, and among them are some of those by Carnap (1956b, 1966) discussed above, and another one in which Carnap (1939, §14) explicitly discusses type theory.

There is thus no indication in Hempel's work that he thinks that axiomatizations have to rely on first order logic only, and there are multiple passages in which he either uses type theory himself, endorses reconstructions that do, or counts works that endorse type theory as expositions of the Received View. This should suffice to show that, while holding the Received View, Hempel did not mean to restrict reconstructions of theories to first order logic.

Of course, there is an abundance of first order logic formulas and simple first order theories in Hempel's and Carnap's writings. In discussing Carnap's studies of probability theory, which rely solely on first order logic, (and shortly after the above quote that notes the limits of first order logic), Hempel (1973, 265) describes and seems to agree with Carnap's view of these analyses:

> [...] Carnap often stressed that these studies are intended only as the first stage in the development of more comprehensive theories, and that the solutions they offer may well permit of extension to more complex situations.

In other words, many analyses in the philosophy of science focus on cases in which the problem is especially clear, often in the hope that the result will transfer to more complicated situations, or at least to arrive at a condition of adequacy for more general analyses. A nice example of such a generalization is described by Psillos (2000, §1, n. 7), who argues that Carnap rediscovered the Ramsey sentence approach when trying to generalize Craig's theorem to type theory. That Hempel shared this view is suggested by his discussion of the problems of confirmation. After noting that a criterion of confirmation should be applicable to hypotheses of any logical form, Hempel (1965e, §5) suggests that a criterion only applicable to laws of the form 'All ravens are black' might "still be considered as stating a particularly obvious and important sufficient condition of confirmation".

First order axiomatizations also often appear as examples in Hempel's writings: When discussing the use of Ramsey-sentences, Hempel (1965f, §9, my emphasis) notes that their

> logical apparatus is more extravagant than that required by [the original theories]. *In our illustration, for example*, [the original theories] contain variables and quantifiers only with respect to individuals (physical objects), whereas the Ramsey-sentence [. . .] contains variables and quantifiers also for properties of individuals.

When discussing correspondence rules, Hempel (1963, 692, my emphases) again writes: "*For example*, the logical framework *might* be that of the first-order functional calculus with identity"; he adds, again ignoring all of the later publications by Carnap (1938, 1939, 1956b, etc.) on the topic: "This is, in fact, one of the principal cases with which Carnap's theory of reduction is concerned".

Although at this point I do not intend to engage in the history of the history of philosophy of science[6], I want to note that Hempel's utterly misleading comments about Carnap's reliance on first order logic may explain how this misconception became part of philosophical folklore. Suppes's switch from the discussion of the "standard sketch", which is the Received View, to the discussion of restrictions to "standard formalizations", which is not a part of the Received View, clearly also has contributed to the confusion, at least that of Beatty (1980) and Suppe (1974a). The latter popularized the mischaracterization through his canonical exposition. Another possible explanation stems from Carnap's exclusive reliance on first order logic in his explication of probability (Carnap 1950). This part of Carnap's work was possibly influential enough to overshadow his use of type theory in other works. Finally, Quine's stance that type theory is not proper logic but rather mathematics (Quine 1970) probably also contributed to the idea that proponents of the Received View were talking about first order logic when referring to logic.

The possible influence of Quine's stance and the logicians who followed him in this respect (cf. Leivant 1994, §5.2) finds a peculiar analogy in a critique of the Received View on

[6] HoHoPoS.

the grounds that it relies on a distinction between the theoretical and the observational vocabulary: Van Fraassen (1980, §3.6) remarks that "logicians attached importance to restricted vocabularies, and that was seemingly enough for philosophers to think them important too", and concludes that the Received View "focussed attention on philosophically irrelevant technical questions". This may very well hold for first order logic as well, except that the philosophers in question are not the proponents of the Received View, but their critics. Only under the strict distinction between metamathematics in first order logic on the one hand and mathematics and type theory on the other hand does Suppes's slogan "Mathematics for the philosophy of science, not meta-mathematics" (Muller 2010, §2; Van Fraassen cf. 1972, 309) make sense. Since neither Carnap nor Hempel subscribed to the distinction, they would have agreed with the slogan's first, but not its second half.

3 Exhaustive axiomatization

Even though the Received View is not overly cumbersome because it demands first order logic, it may still turn out to be overly cumbersome because it demands an exhaustive axiomatization of the scientific theory under consideration and all the mathematics it contains. With a reference to Van Fraassen (1980), who explicitly criticizes the Received View, Suppes (1992, 207f) criticizes philosophers of science who rely on "standard formalizations":

> Suppose we want to give a standard formalization of elementary probability theory. [Only] after stating a group of axioms on sets, and another group on the real numbers, [are we] in a position to state the axioms that belong just to probability theory as it is usually conceived. In this welter of axioms, those special to probability can easily be lost sight of.
>
> More important, it is senseless and uninteresting continually to repeat these general axioms on sets and on numbers whenever we consider formalizing a scientific theory. No one does it, and for good reason.

A standard formalization of a theory in Suppes's sense is thus not only restricted to first order logic, but also contains explicitly all axioms for all mathematical concepts that occur in the theory.

Suppes suggests instead a reconstruction in the Semantic View, which allows to leave much or even all of the needed mathematical axioms unmentioned, and accordingly leads to much simpler formalizations. Speaking in favor of the Semantic View, Stegmüller (1979, §1) argues that Carnap's approach to scientific theories could only be executed by philosophers with technical abilities far beyond even those of Montague (1962), who gave exhaustive axioms for Newtonian mechanics. And Muller (2010, §7), after suggesting improvements of both the Received and the Semantic View that bring them quite close together, concludes that

> the gap remains sufficiently wide to prefer the [improved Semantic View] over the [improved Received View], because the starting point for [the semantic] construal remains a set-theoretical predicate [. . .], and because the formal theories [. . .] need never be spelled out (the formalising labour that is mandatory in the [Received View] need not be performed).

Here, the overabundance of axioms Suppes describes for standard formalizations is explicitly assumed to accrue in a reconstruction of a theory within the Received View. Van Fraassen (1972, 306) makes the same assumption.

It seems indeed like an unacceptable demand on philosophical analysis to start with an exhaustive axiomatization of any concept that is employed by the theory under consideration. And the two most prominent works that take this route, Montague's and the axiomatization of parts of biology by Woodger (1939), certainly do not make for a relaxing read. This, however, is no reason to criticize the Received View, because it does not contain a demand for exhaustive axiomatizations. It is true that both Hempel (1952, 733, n. 24) and Feigl (1970, 8) mention Woodger's axiomatization as in keeping with the Received View, but that just means that exhaustive axiomatizations are allowed, not that they are demanded. Hempel and Feigl mention Woodger's axiomatization as one example among many; Hempel also mentions the axiomatization of the theory of relativity by Reichenbach and the axiomatization of game theory by von Neumann and Morgenstern; Feigl mentions the axiomatizations of Reichenbach and Suppes himself. Hempel (1970, 149f) later calls Reichenbach's treatment "axiomatically oriented, though not strictly formalized", but still considers Suppes's treatments full fledged axiomatizations.

Hempel (1974, 247, my emphasis) also calls Suppes's axiomatization "technically much *more* elegant and rigorous than Reichenbach's", suggesting that the exhaustiveness of axiomatizations comes in degrees. This stance is explicitly taken by Carnap (1939, §15), who notes that it would be "practically impossible to give each deduction which occurs the form of a complete derivation in the logical calculus [. . .]. But it is essential that this dissolution is theoretically possible and practically possible for any small part of the process". This means that obvious or uncontentious steps in derivations can be skipped, and with them the axioms on which they rely if those are not needed at other points in the deduction. Carnap (1939, §16) even gives a general description of this method which seems to describe Suppes's method to avoid the "welter of axioms" quite well:

> Each of the nonlogical calculi [which are applied in science] consists, strictly speaking, of two parts: a logical *basic calculus* and a *specific calculus* added to it. The basic calculus could be approximately the same for all those calculi; it could consist of the sentential calculus and a smaller or greater part of the functional calculus as previously outlined. The specific partial calculus does not usually contain additional rules of inference but only additional primitive sentences, called *axioms*. As the basic calculus is essentially the same for all the different specific

> calculi, it is customary not to mention it at all but to describe only the specific part of the calculus.

Since “the functional calculus as previously outlined” contains type theory and mathematics (Carnap 1939, §14), this allows a great deal of axioms to go unmentioned. The restriction of the exposition to the specific calculus is quite common according to Carnap (1939, §23), as he notes that “the customary formulation of a physical calculus is such that it presupposes a logico-mathematical calculus as its basis”.

Carnap (1939, §16) gives as an example of a calculus a simple theory of thermal expansion, and accordingly uses, but does not axiomatize, logical and mathematical concepts. In a derivation within this calculus, mathematical transformations are inferred in one step and simply marked “Proved mathem. theorem”.

The description of the *additional* primitive sentences in a nonlogical calculus as ‘axioms’ may have led to the misconception that the Received View demands an exhaustive *logical* calculus, rather than at the most an exhaustive specific calculus. This ambiguity may have not been stated explicitly enough in the works of the Received View, even though Carnap (1950, 15f), for one, refers to the above quotation (Carnap 1939, §16) when discussing formalizations of probability theory, and states:

> The formalization (or axiomatization) of a theory or of the concepts of a theory is here understood in the sense of the construction of a formal system, an *axiom system* (or postulate system) for that theory. [. . .]
>
> In the discussions of this book we are [. . .] thinking of those semiformal, semi-interpreted systems which are constructed by contemporary authors, especially mathematicians, under the title of axiom systems (or postulate systems). In a system of this kind the axiomatic terms (for instance, in Hilbert’s axiom system of geometry the terms ‘point’, ‘line’, ‘incidence’, ‘between’, and others) remain uninterpreted, while for all or some of the logical terms occurring [. . .] and sometimes for certain arithmetical terms [. . .] their customary interpretation is—in most cases tacitly—presupposed.

This passage is a general description of how the tacit presupposition of axioms is to be understood, and it could well serve as a conclusion to the quote by Suppes (1992, 207f) at the beginning of this section.

Therefore, the Received View cannot be criticized for demanding an unwieldy and overly difficult formalization from all philosophers of science. As their examples show, Hempel and Feigl consider those axiomatizations explicit enough that do not list each and every axiom used in a derivation. And Carnap not only gives an axiomatization that leaves out all of mathematics, but also describes how in general a theory can be axiomatized with a specific calculus alone, while the basic calculus is presupposed. It is also possible to infer from his remarks when it is necessary to include an axiom: whenever it plays a contentious role in a derivation.

4 Models

While the Received View clearly does not demand exhaustive axiomatizations in first order logic, it does clearly demand axiomatizations. This emphasis on axiomatizations has been another source of criticism, since it suggests a dismissive attitudes towards scientific models. This presumption about the Received View is summarized well by Frigg and Hartmann (2008, § 4.1):

> Within [the syntactic view], the term model is used in a wider and in a narrower sense. In the wider sense, a model is just a system of semantic rules that interpret the abstract calculus [. . .]. In the narrower sense, a model is an alternative interpretation of a certain calculus [. . .]. Proponents of the syntactic view believe such models to be irrelevant to science. Models, they hold, are superfluous additions that are at best of pedagogical, aesthetical or psychological value [. . .].

As proponents of this attitude, Frigg and Hartmann cite Carnap (1939) and Hempel (1965b). Given the central role of models in science and recent philosophy of science, this attitude has provoked a lot of criticism, and has generally been seen as a reason to prefer the Semantic View over the Received View (see, for example, da Costa and French 1990; Morrison and Morgan 1999; Suppe 2000, Bailer-Jones 2003, §2; Muller 2010, §2). The reason is again neatly summarized by Frigg and Hartmann (2008, §4.1):

> The semantic view of theories [. . .] reverses this standpoint [of the Received View towards models] and declares that we should dispense with a formal calculus altogether and view a theory as a family of models. Although different version[s] of the semantic view assume a different notion of model [. . .] [,] they all agree that models are the central unit of scientific theorizing.

Note that the last sentence just means that all versions of the Semantic View agree that the central unit of scientific theorizing should be called 'models'. It is therefore by now means certain that all versions of the Semantic View assume that models in the narrower sense of Frigg and Hartmann are central to science.

In the following, I want to show that Carnap and Hempel only single out *one use* of models in the narrower sense as not essential to scientific theories, and are joined in this attitude by a major proponent of the Semantic View, Patrick Suppes. First, however, I note that one specific defense of the Received View with respect to its stance on models fails: Suppe (1974a, 90) claims that "Carnap and Hempel do make it clear at various places that independent nonobservational semantic interpretations [of theories] are permissible". And while it "must be admitted [. . .] that they tended to do so begrudgingly and also to belittle the importance of giving such interpretations" (Suppe 1974a, 90, n. 191), Suppe (1974a, 91) can then argue that theoretical terms can be

> interpreted as referring to electrons, electron emissions, and so on, where 'electron', 'electron emission', and so forth, have their normal meaning in scientific language. If we look at theoretical terms such as 'electron', we find that [...] much of the meaning concerns extra-observational associations—for example, for electrons there might include various features of the billiard-ball model, various classical intuitions about macroscopic point-masses, and so on.

Thus alternative interpretations of the calculus of a theory can play a role in the Received View.

However, one of the sources that Suppe cites for the support of his premises is a statement by Hempel about his new approach to scientific theories, after having given up the Received View (Achinstein et al. 1974, 260). And describing his new approach, Hempel (1974, IV) explicitly states that the "extensive theoretical use of antecedent terms appears to me to throw into question the conception of the internal principles of a theory as an axiomatized system whose postulates provide 'implicit definitions' for its extralogical terms". Hempel thus does not assert that direct interpretations of the calculus of the theory (through the "antecedent terms" used in the theory) are permissible in the Received View. Quite oppositely, he considers the need for (or factual use of) direct interpretations of theoretical terms a reason to *give up* the Received View.

Hempel's interpretation of the Received View seems correct in that Suppe's two other sources are very explicit about the impossibility of such non-observational interpretations: Carnap (1939, 204) notes the possibility to give the meaning of the theoretical terms in the metalanguage. But he adds that in order to enable someone "who does not know physics but has normal senses and understands a language in which observable properties of things can be described" to "apply [the theory] to his observations in order to arrive at explanations and predictions", "we have to give semantical rules for elementary terms only, connecting them with observable properties of things". This is not even a begrudging endorsement of non-observational interpretations. With reference to Carnap (1939), Hempel (1963, 696) also argues that interpreting theoretical terms in a metalanguage offers "little help towards an understanding of those expressions. For the criteria will be intelligible only to those who understand the metalanguage in which they are expressed". The argument for this conclusion is detailed by Rozeboom (1970, 204f). Carnap (1956b, 47, §V) is indeed very clear that there is no interpretation of the theoretical language L_T independent of the correspondence rules: "There is no independent interpretation of L_T. [...] The [theoretical terms] obtain only an indirect and incomplete interpretation by the fact that some of them are connected by the [correspondence rules] with observational terms".[7]

A good starting point for a defense of the Received View on models is rather the discussion of two different kinds of models given by Hempel (1965a, §6). He calls instances of the one

[7] Apart from this direct textual evidence, I should note that Suppe's premise is also incompatible with semantic empiricism (cf. Rozeboom 1962), a core assumption of logical empiricism.

kind “theoretical models”, noting that they are also known as “mathematical models” (Hempel 1965a, 445f). “Broadly speaking, and disregarding many differences in detail”, he continues, “a theoretical model of this kind has the character of a theory with more or less limited scope of application”. As examples, Hempel lists models of learning, conflict behavior, and in general models of social, political, and economic phenomena. Models of this kind, Hempel notes, are often idealizations in that they disregard factors relevant for the phenomenon under study, often oversimplify the relations of their parameters, and may be applicable only under very specific conditions.

Clearly, theoretical models are neither models in the narrower nor in the wider sense of Frigg and Hartmann. Nonetheless, they may be very well meant when the term ‘model’ is used in contemporary philosophy of science. A case in point is a recent discussion by Weisberg (2007) of a predator-prey model: Vito Volterra, in an effort to analyze the populations of Adriatic fish, stipulated certain properties of a predator-prey relationship between the populations to arrive at two coupled differential equations. These equations allowed him to predict the qualitative effects of fishing on the populations. Weisberg (2007, §2.1) notes that Volterra himself recognized that “his model was extremely simple and highly idealized with respect to any real world phenomenon”, in short, that it was a theoretical model as defined by Hempel. Furthermore, Hempel’s notion of a theoretical model is at least one way to make sense of the existence of incompatible models (cf. Frigg and Hartmann 2008, §5.1), for one because two theoretical models with disjunct scopes may be incompatible when taken as unrestricted in scope. More importantly, two theoretical models with overlapping scopes may disregard different relevant factors and oversimplify the relations of their parameters in different ways.[8] Thus, an important meaning of ‘model’ in science and philosophy of science may be that of ‘theoretical model’ in Hempel’s sense, and since theoretical models are simply theories, there is no question that they play an important role in the Received View.

Hempel calls the other kind of model “analogical”, and the underlying conception “syntactic isomorphism”. Two sets L_1 and L_2 of sentences are syntactically isomorphic if L_1 can be obtained from L_2 by renaming the nonlogical constants appearing in its sentences. Hempel then defines a system S_1 to be an analogical model of a systems S_2 with respect to the sets of laws L_1 and L_2 if L_1 is true of S_1, L_2 is true of S_2, and L_1 and L_2 are syntactically isomorphic. It is easy to see that, by renaming the nonlogical constants, the interpretation of L_1 by S_1 can be turned into an interpretation of L_2 and vice versa. Therefore, an analogical model of this kind is an alternative interpretation of a calculus, and thus for Frigg and Hartmann a model “in the narrower sense”.

Hempel (1965a, 440f) lists three ways in which analogical models can be useful. First, an analogical model “may make for ‘intellectual economy’” because “all the logical consequences of the [one system’s laws] can be transferred to the new domain by simply replacing all extra-logical terms by their counterparts”. A set of mutually analogical models also allows

[8] I thank Christopher Belanger for impressing the importance of incompatible models on me.

the development of one "general mathematical theory" for all the systems at once, "without distinguishing between the different subject matters to which the resulting theory can be applied". The intellectual economy is straightforward: Investigate the logical or mathematical features of the laws once, and apply the results to all systems with syntactically isomorphic laws.

Second, analogical models can "facilitate one's grasp" of the laws in a new domain by "exhibiting a parallel with [laws] for a more familiar domain" (Hempel 1965a, 441). The wave equations of electrodynamics, for example, can be qualitatively analyzed by simply thinking about waves in solid bodies, because those are sometimes governed by syntactically isomorphic equations.

"More important", Hempel continues, "well-chosen analogies or models may prove useful 'in the context of discovery', i. e., they may provide heuristic guidance in the search for new explanatory principles". Hempel (1965a, 445) elucidates:

> Considering the great heuristic value of structural analogies, it is natural that a scientist attempting to frame a new theory should let himself be guided by concepts and laws that have proved fruitful in previously explored areas. But if these should fail, he will have to resort to ideas that depart more and more from the familiar ones.

Hempel adduces the development of quantum mechanics, which started out close to classical mechanics but became considerably less analogical, thereby gaining in scope.

The first two uses of analogical models are, in a sense, of psychological value, just as Frigg and Hartmann contend. The third, heuristic use of analogical models is not so, and neither is its main value pedagogical or aesthetic. It is nonetheless unsurprising that it does not play a major role in the Received View on scientific theories, because the Received View was not developed for analyses in the context of discovery, but for analysis in the context of justification (cf. Feigl 1970, 3f, 13f). Even for critics of this distinction it should be acceptable that neither Hempel nor Carnap worked on the role of analogical models in theory development, since a division of labor cannot be considered hostility towards analogical models.

Hempel (1965a, 438f) only dismisses the use of analogical models as essential for explanation. His argument is straightforward: Assume that some feature of a system is to be explained. To find an analogical model of the system, the system's laws have to be established. But the laws are all that is needed to give an explanation of the feature, and so no analogical model is needed. This argument rests on the D-N-schema of explanation, and it is not obvious how it translates to other explications of explanation. But whether it translates or not, Hempel does establish that an analogical model (in his sense) is not needed to derive statements about a system from the system's laws.

Carnap's dismissal of models is similarly confined. Discussing 'understanding' in physics (Carnap 1939, 209), he notes that

> [w]hen abstract, nonintuitive formulas, as, e. g., Maxwell's equations of electromagnetism, were proposed as new axioms, physicists endeavored to make them "intuitive" by constructing a "model", i. e., a way of representing electromagnetic microprocesses by an analogy to known macroprocesses, e. g., movements of visible things.

But not only did these attempts fail, Carnap states, they are also not necessary:

> It is important to realize that the discovery of a model has no more than an aesthetic or didactic or at best a heuristic value, but is not at all essential for a successful application of the physical theory.

Like Hempel, Carnap does not dismiss theoretical models, nor does he question the usefulness of analogical model in the context of discovery (which he does not even mention). He only objects to the demand that every formalism be supplied with an analogical model in terms of macroprocesses, that is, he objects to analogical models as *necessary* for the theory's application. Assuming that the theory is successfully applied by deriving true statements from it, this objection is justified by Hempel's argument against the need for analogical models in explanations.

When Carnap (1966, 232f) does mention the context of discovery, he accepts a prominent role for analogical models:

> [I]magine that we are [...] preparing to state for the first time some theoretical laws about molecules in a gas. [...] We do not know the exact shape of molecules, so let us suppose that they are tiny spheres. How do spheres collide? There are laws about colliding spheres, but they concern large bodies. Since we cannot directly observe molecules, we assume their collisions are analogous to those of large bodies [...]. These are, of course, only assumptions; guesses suggested by analogies with known macrolaws.

Carnap (1966, 174f) only cautions that visualizability (which, arguably, is a central motivation for many analogical models) is neither necessary nor sufficient, but can be helpful. And models can turn out to be true:

> A physicist must always guard against taking a visual model as more than a pedagogical device or makeshift help. At the same time, he must also be alert to the possibility that a visual model can, and sometimes does, turn out to be literally accurate. Nature sometimes springs such surprises. [...]
>
> A theory may move away from models that can be visualized; then, in a later phase, when more is known, it may move back again to visual models that were previously doubted.

Thus Carnap, like Hempel, considers analogical models important, but not necessary for the application of the theory to the system.

Let me compare this stance with the one taken by Suppes (1960) in one of the articles that cemented the Semantic View's position as the account of scientific theories especially hospitable to models (see Muller 2010, §2). Suppes (1960, 289) argues that

> the concept of model in the sense of Tarski may be used without distortion and as a fundamental concept in [physics, social science, and mathematical statistics]. In this sense I would assert that the meaning of the concept of model is the same in mathematics and the empirical sciences. The difference to be found in these disciplines is to be found in their use of the concept.

Suppes's claim is thus that in all three disciplines 'model' means what Frigg and Hartmann call 'model in a wider sense'. Therefore, Suppes argument cannot possibly establish that the Semantic View is more hospitable to analogical models than the Received View. Suppes (1960, 289) also explicitly excludes Hempel's theoretical models from his discussion by cautioning against

> one very common tendency, namely, to confuse or amalgamate what logicians would call the model and the theory of the model. It is very widespread practice in mathematical statistics and in the behavioral sciences to use the word 'model' to mean the set of quantitative assumptions of the theory, that is, the set of sentences which in a precise treatment would be taken as axioms [. . .].

Since theoretical models are theories, not models of theories, they are excluded from Suppes discussion like analogical models. Note that Suppes here in effect claims that many of the so-called models used in the sciences are actually theories, and thus play an important role in the Received View.

Suppes (1960, 290) does acknowledge that "many physicists want to think of a model of the orbital theory of the atom as being more than a certain set-theoretical entity. They envisage it as a very concrete physical thing built on the analogy of the solar system". It is not exactly clear whether Suppes has here exactly Hempel's analogical models in mind, or simply thinks of the "concrete physical thing" as the thing that contains the actual nucleons and electrons, that is, the intended interpretation of the formalism. In the latter case, this is not a model in either the wide or the narrow sense. But Suppes (1960, 291) evaluates what is clearly an analogical model in his discussion of

> Kelvin's and Maxwell's efforts to find a mechanical model of electromagnetic phenomena. Without doubt they both thought of possible models in a literal physical sense, but it is not difficult to recast their published memoirs on this topic into a search for set-theoretical models of the theory of continuum mechanics which will account for observed electromagnetic phenomena. Moreover, it is really the

> formal part of their memoirs which has had permanent value. Ultimately it is the mathematical theory of Maxwell which has proved important, not the physical image of an ether behaving like an elastic solid.

Kelvin and Maxwell are thus trying to find the regularities that govern electrodynamic phenomena by using an analogy with mechanical phenomena. Therefore, they used analogical models in the context of discovery, as discussed by Hempel and Carnap. But Suppes's interest lies in the possibility to describe electrodynamics by a mathematical model, that is, to give a model in the wider sense. And the permanent value of Maxwell's theory is, I take it, its applicability, for which Suppes considers the analogical model irrelevant. Overall, Suppes's attitude towards scientific models seems to be at most as hospitable towards scientific models as that of Hempel and Carnap. To assume that the Semantic View is directly related to scientific models because it uses model theory is nothing but a fallacy of equivocation, and whether scientific models are better formalized in predicate logic or model theory is a matter of contention (cf. Lutz 2010, §6).

Finally, while I am mainly interested in Hempel's and Carnap's formulations of the Received View, it should be noted with Suppe (1974a, §IV.D) and Hempel (1970, §5) that one clear proponent of the Received View, Nagel (1961, §§5.II.3, 6.1), considered analogical models part of theories.

5 The concept and object of explication

Suppe (1974a, §III, p. 58) develops a direct argument against the Received View that is closely related to the question of axiomatizability of scientific theories. As the first premise, he "take[s] it as being reasonably clear from Carnap's and Hempel's writings that they intend their analysis to provide an *explication* of the concept of a scientific theory". Carnap (1950, §§2–6) gives a detailed discussion of the idea behind and the structure of an explication, which he takes to consist of a preliminary identification of the typically vague notion to be explicated, the explicandum, and the development of a new concept, the explicatum, that is to take the explicandum's place in some analyses. The explicatum has to fulfill four requirements: some similarity to the explicatum, precision in the rules for its use, fruitfulness in the development of theories, and simplicity to the extent allowed by the previous three requirements.

The requirements, Suppe (1974a, 59) notes, are "rather vague as to the relationship in which the explicatum should stand to the explicandum", and he therefore introduces as his second premise an "adequacy criterion" stated by Chomsky (1957, §2.1), which "seems to be in accord with Carnap's position" (Suppe 1974a, 59). According to this criterion, an explication is adequate only if "the explicatum denote[s] all the clear-cut instances, and none of the clear-cut noninstances" of the explicandum. In other words, the explicatum has to concur with the explicandum within the boundaries of the explicandum's vagueness, so that explication is a kind of precisification. Suppe (2000, S104) later repeats these two premises.

This criterion of adequacy allows to test the Received View, because if it "can be demonstrated that there are clear-cut examples of scientific theories which do not admit of the required canonical formulation [of the Received View], or else show[n] that certain clear-cut examples of nonscientific theories fit their analysis", then this shows "the inadequacy of the Received View" (Suppe 1974a, 60). And it is not difficult for Suppe (1974a, 65) to find, for his third premise, scientific theories that cannot be axiomatized. For example, there are

> Darwin's theory of evolution, Hoyle's theory on the origin of the universe, [and] Freud's psychology [...]. Furthermore, it is manifest that most theories in cultural anthropology, most sociological theories about the family; theories about the origin of the American Indian [...] are all such at present that any attempts at axiomatization would be premature and fruitless [...].

Since "[s]ome theories do admit of fruitful axiomatization, however", Suppe (1974a, 63) concludes that "the Received View is plausible for some but not all scientific theories". Similarly, Beatty (1980, appendix 1) considers the impossibility to axiomatize "evolutionary theory, Freudian psychology, theories of the origin of the universe, and many others" to be a major problem of the Received View (and curiously traces this impossibility to the Received View's perceived restriction to first order logic).

First, note that Suppe's conclusion, that the Received View is plausible for some, but not all theories, does not follow from his premises; the correct conclusion is that the Received View is false. Second, note that '$\{\forall x(Px \vee Qx), Oc \leftrightarrow Pc\}$' is axiomatizable and fulfills all the further requirements of the Received View if 'P' and 'Q' are theoretical predicates, 'O' an observational predicate, and 'c' an observational constant. Since some such sets of sentences are clearly not called 'scientific theory' in ordinary language, the Received View is false independently of Suppe's third premise. Given the ease of this disproof of the Received View, it should come as no surprise that Suppe's first and second premises are false.

5.1 The concept of explication

Suppe's sole source for the first premise is Chomsky (1957, §2.1), who references neither Carnap nor Hempel for his criterion of adequacy for explication. He does mention Goodman (1951, 5–6), but Goodman (1951, §I,1) lists precisification only as one kind of explication (he speaks of 'constructional definition'). Furthermore, contrary to Suppe's first premise, Goodman notes that for explications in general, scientists and philosophers often "trim and patch the use of ordinary terms to suit their special needs, deviating from popular usage even where it is quite unambiguous".

In his discussion of Goodman's view on explication, Carnap (1963a) does not take issue with the possible deviation of the explicatum from the explicandum. This is unsurprising given that Carnap (1950, §3) himself allows for such deviation, and even uses the explication of 'fish' as an example, just like Goodman:

> [O]ne might perhaps think that the explicatum should be as close to or as similar with the explicandum as the latter's vagueness permits. However, it is easily seen that this requirement would be too strong, that the actual procedure of scientists is often not in agreement with it, and for good reasons. [...] In the construction of a systematic language of zoölogy, the concept Fish designated by this term has been replaced by a scientific concept designated by the same term 'fish'; let us use for the latter concept the term 'piscis' in order to avoid confusion. When we compare the explicandum Fish with the explicatum Piscis, we see that they do not even approximately coincide. The latter is much narrower than the former; many kinds of animals which were subsumed under the concept Fish, for instance, whales and seals, are excluded from the concept Piscis.

This settles the question of Carnap's (and Goodman's, for that matter) stance on Chomsky's criterion of adequacy.

Hempel's position, on the other hand, is more ambiguous. On the one hand, see seems to be in accord with Carnap: In a monograph on concept formation, Hempel (1952, 663) refers to Carnap's exposition without taking exception to the possible deviation of the explicatum from the explicandum, and states:

> Explications, having the nature of proposals, cannot be qualified as being either true or false. Yet [...] they have to satisfy two major requirements: First, the explicative reinterpretation of a term or—as is often the case—of a set of related terms must permit us to reformulate [...] at least a large part of what is customarily expressed by means of the terms under consideration. Second, it should be possible to develop, in terms of the reconstructed concepts, a comprehensive, rigorous and sound theoretical system.

Note that Hempel does not demand that the explicatum coincide with the explicandum to the extent that the latter's vagueness permits. He only demands that what can be expressed with the explicandum can also be expressed with the explicatum (or the explicata), and even this only in in a large part of the cases, but not in all. Most importantly, according to Hempel explications cannot be false, which they could be if there was a condition of adequacy for the relation of explicandum and explicatum.

Another passage by Hempel also suggests agreement with Carnap. In a review of Goodman's exposition of explication, Hempel (1953, 113f) states:

> It seems to me important to note [...] that the stage of rigorous construction in philosophy [...] presupposes a preconstructional clarification of the explicanda under investigation. [...] But in the pursuit of its objective, analysis cannot be content with a purely descriptive account of linguistic behavior patterns: it has to point out the pitfalls inherent in the various modes of usage [...]. And from here

> on, it is only a short step [. . .] to [. . .] explicitly proposing certain modifications of existing usage which will enhance clarity and which promise to be theoretically fruitful. Once this last step has been taken, the stage is set for the development of a constructional system for the readjusted explicanda [. . .].

It is of no relevance in this context that Hempel here, in a quite puzzling shift of terminology, allows a deviation of the explicanda from actual usage, rather than a deviation of the explicata from the clear cases of the explicanda. The important point is that Hempel allows that the explicata deviate from clear cases of the actual usage.

However, Hempel (1950, §6) also answers the question of "how [. . .] to judge the adequacy of a proposed explication, as expressed in some specific criterion of cognitive meaning" with two criteria, the first of which relies on the fact that

> there exists a large class of sentences which are rather generally recognized as making intelligible assertions, and another large class of which this is more or less generally denied. We shall have to demand of an adequate explication that it take into account these spheres of common usage; hence an explication which, let us say, denies cognitive import to descriptions of past events or to generalizations expressed in terms of observables has to be rejected as inadequate.

Unfortunately, Hempel is not explicit about whether an explicatum always has to coincide with the explicandum's clear cases, but speaks of an explication "taking into account" common usage, which is determined by "rather general" agreement of positive instances and "more or less general" agreement on negative ones.

In a defense of his and Oppenheim's explication of scientific explanation, Hempel (1965a, §11) similarly states:

> Like any other explication, the construal here put forward has to be justified by appropriate arguments. In our case, these have to show that the proposed construal does justice to such accounts as are generally agreed to be instances of scientific explanation, and that it affords a basis for a systematically fruitful logical and methodological analysis of the explanatory procedures used in empirical science.

The phrase 'do justice to such accounts' is not as clear as one might wish, but nonetheless this is possibly the passage in which Hempel comes closest to demanding that the explicatum includes all clear positive instances of the explicandum (note that he does not demand agreement on negative instances). It was written in the years of 1963 and 1964 at the Center for Advanced Study (Hempel 1965d), about eighteen years after his monograph on concept formation was written or at least researched (Hempel 1952, 731, n. 1), and one year before he gave a lecture that would form the core of his rejection of the Received View (Hempel 1970, 142, n. 1). Maybe Hempel had simply changed his mind. At the Center, he also met Thomas Kuhn for the first time, whose ideas "certainly contributed to [his] shift from an antinaturalistic stance to a naturalistic one" in the following years (Hempel 1993). Maybe the passage marks the first tiny step in this direction.

On the other hand, the passage sounds suspiciously similar to the preceding one published in 1950, which precedes the publication of Hempel's monograph on concept formation, though not the research that led to it. The dissonance may therefore just be the result of loose language, which leaves—barely—enough room to go conform with Carnap on the relation of explication and precisification.

In summary, Carnap clearly did not conceive of explication as a specific kind of precisification, and unless Hempel was very confused about Carnap's stance, neither did Hempel. Considering that Hempel's position was at least at some points unclear, I want to note that explication is a core concept of ideal language philosophy (cf. Carnap 1963b, §19; Maxwell and Feigl 1961, 488; Lutz 2009, §2). And in a defense of ideal against ordinary language philosophy, Maxwell and Feigl (1961, 491) are very explicit about the possible deviation of the explicatum from the explicandum:

> [W]e see absolutely no reason to believe that examination of ordinary use in the "paradigm", normal cases can provide us with definitive rules for "proper" use in the unusual and novel cases. [...]
>
> Furthermore—and this is of crucial importance—consideration of atypical cases often points up possible inadequacies and may suggest improvements in our conceptualization of the "normal" cases.

Because Maxwell and Feigl consider philosophical problems to be often linked to unusual and novel cases, the first point is an explicit rejection of the viability of ordinary language philosophy: To tackle philosophical problems, ordinary language at least has to be precisified, but ordinary language itself gives no clue on which precisification is right. The second point extends this rejection to the areas where ordinary language is unambiguous: Even a precisified language might be inadequate, and therefore even the use in the clear cases might have to be changed to solve philosophical problems. Thus rendering explication as precisification means giving it up the core of ideal language philosophy.

5.2 The object of explication

Feigl certainly considered the Received View to be about explication, not just precisification: Maxwell and Feigl (1961, 489f) call explication also 'rational reconstruction', and in a defense of the Received View, Feigl (1970, 13) speaks of the Received View as being concerned with "the rational reconstruction of theories". But this does not mean that Feigl agrees with Suppe on his second premise. This is because Feigl does not speak of the rational reconstruction of 'theory', or the rational reconstruction of the concept (or notion) of a theory, but the rational reconstruction of theories. In the same text, he does speak of the "analysis of the *notion of* evidential support" and the "*concept of* probability" (Feigl 1970, 9, my emphases), so it is improbable that when speaking about theories, Feigl just ignores the use-mention distinction. Indeed, earlier in the text he remarks that "logicians of science [...] analyze *a given theory* in regard to its logical structure" (Feigl 1970, 8, emphasis changed), that is, a specific one, not the term denoting all of them. My thesis is therefore that the Received View is not meant to explicate 'theory', but rather provide a framework in which to explicate specific theories like the general theory of relativity or evolutionary theory. Again in the words of Feigl (1970, 13): "[T]he 'orthodox' view of scientific theories can help in clarifying their logico-mathematical structure, as well as their empirical confirmation (or disconfirmation)".

One piece of evidence for this view is that there is, to my knowledge, no text by Hempel, Carnap, or Feigl in which either mentions a successful explication of the term 'theory'. Quite to the contrary, when Hempel (1983, §6) discusses "Carnap's views on the analytic elaboration of methodological concepts and principles [to which he] refers [...] as *explication*", he notes that among those philosophical issues that Carnap so elaborated are "standards for a rational appraisal of the credibility of empirical hypotheses", but does not include the concept of an empirical hypothesis or theory itself. He writes:

> Explication plays an important role in analytic philosophy, where it has often been referred to as logical analysis or rational reconstruction. All the accounts proposed by analytic empiricists for such notions as verification, falsification, confirmation, inductive reasoning, types of explanation, theoretical reduction, and the like are instances of explication.

This is a rather comprehensive list of concepts at the core of logical empiricism's philosophy of science. If 'theory' had been explicated as well, it would be very surprising that Hempel does not mention such a central concept at all.

After giving up the Received View and presenting a competing account, Hempel was actually asked whether his new view was meant as a description of the actual use of 'theory', in a discussion published in the conference proceedings edited by Suppe (1974b) himself. Sylvain Bromberger asks (Achinstein et al. 1974, 261):

> Exactly what is the status of this analysis of a theory? [...] As an analysis of the concept of a theory, that is, the concept that is embodied in our use of the word 'theory' in English or equivalent ones in other languages, I think it is demonstratively false. [...] The view that a theory consists of an uninterpreted calculus and rules of interpretation [...] was a program which, if successful, would have made explicit the rules that govern or that ought to govern acceptance and rejection of theories. [...] Now do you envisage your new [analysis] as entailing a program that will show how the theories that we in fact have might ultimately be analyzed?

Bromberger here claims that Hempel's new account does not capture the use of the term 'theory' in ordinary language, and then asks whether it is meant to achieve the goal of the Received

View. He takes this goal to be the successful explication of the rules for acceptance and rejection of theories.

Hempel's reply is telling:

> Professor Bromberger is right in stressing the programmatic side of the standard conception. One of its objectives was to explicate, and appraise from the point of view of an analytic-philosophical conscience, the principles governing concept formation in scientific theories. Another objective was similarly to exhibit and appraise the principles governing the testing of scientific theories. [. . .]
>
> My paper was intended principally as a criticism of the basic assumptions by means of which the standard construal tackles its task; I did not put forward a properly developed alternative.

Again, Hempel does not mention the explication of the term 'theory' as a goal of the Received View. Instead, he agrees with Bromberger that one goal was the explication of the rules of acceptance and rejection, that is, of the testing of scientific theories. He adds that another goal was the explication of the rules of concept formation. Furthermore, Hempel seems to agree with Bromberger that his new account fails in capturing the use of the term 'theory' and that his new account might nonetheless achieve the goals of the Received View. Therefore, he cannot be of the opinion that the goal of the Received View was to capture the use of the term 'theory'.

In a critical discussion of the Received View, Hempel (1970, 148) addresses explicitly the relation between the Received View and explication: "[T]he standard construal [. . .] was intended [. . .] as a schematic explication that would clearly exhibit certain logical and epistemological characteristics of scientific theories". Since the Received View is not an explication but an explication schema, it is incomplete until it is applied to something. The explication of a scientific theory would then be an instantiation of this schema. In other words, the Received View provides a framework for explicating specific scientific theories.

Beyond the general impression of Carnap's and Hempel's writings, Suppe (1974a, §IV, A, n. 126) cites an early article by Carnap (1931a, §§5, 7) as evidence for the claim that a theory that "does not admit of a canonical reformulation meeting the conditions [. . .] of the Received View [. . .] is not a genuine scientific theory". This article, he states, contains a "very explicit version of the claim for the initial version of the Received View", where the "initial version" is that at the time of the Vienna Circle. That this claim still holds for the final version is borne out by the addendum to the article's English translation, in which Carnap (1963a) "reaffirms this claim in its essential form".

In the two sections to which Suppe refers, Carnap (1931a, 453, all translations are mine) argues for physicalism, the thesis that "every scientific sentence can be translated into the physicalistic language"[9] and that through this, "the whole of science becomes physics"[10] (Carnap 1931a, 463, emphasis removed). His argument for these conclusions relies on the explicit definability of all scientific terms in observational terms, and the explicit definability of all observational terms in physical terms. Because explicit definability is transitive, the explicit definability of all scientific terms in physical terms follows immediately from these two claims. In the addendum, Carnap (1963a) notes that the explicit definability of all scientific terms in observational terms has to be given up and substituted by "reducibility through a kind of conditional definitions" (cf. Carnap 1936, 1937) or relations "still more flexible" (cf. Carnap 1956b). For a then current presentation of physicalism, Carnap refers to two discussions by Feigl (1958, 1963) and a work of his own (Carnap 1963b, §7). Both of Feigl's work discuss the reducibility of mental states to physical states.

It is surprising to me that Suppe sees in these discussions a justification of his second premise. Clearly, the texts are about what terms can be reduced to physical language, and the thesis of physicalism that Carnap holds is that any meaningful term can be reduced to physical terms. But to infer from this position that the Received View explicates 'theory' would at least need additional premises, for example that all physical theories can be axiomatized according to the Received View, that all and only sets of sentences reducible to a theory are themselves theories, and that all and only scientific theories can be reduced to physics with the help of the reduction statements. And the latter premise is explicitly denied by Feigl (1963, 241–245) and Carnap (1963b, 883) at the time of the final version of the Received View, and also in the very article Suppe uses to support his second premise. There, Carnap (1931a, 449, my translation) states with respect to the reducibility of biology to physics that "the thesis of the universality of the physical language [...] is not about the reducibility of the biological *laws* to the physical, but the reducibility of the biological *terms* to the physical [...]. And this reducibility can, in contradistinction to the former, be easily shown."[11] In conclusion, Suppe's textual evidence fails to make his case for the second premise.

Since the Received View is a framework intended to help in the explication of theories, it is no problem if it also allows to explicate things that are not scientific or no theories. It is more problematic if the Received View relies on assumptions that not all theories fulfill, so that it cannot help in their explications. Like Suppe, Carnap (1939, 202) was clearly aware that some scientific theories could not, at that point, be axiomatized, and hence a fortiori not axiomatized according to the Received View. He writes:

> Any physical theory, and likewise the whole of physics, can [...] be presented

[9]"Unsere Überlegungen [...] führen somit zu dem Ergebnis, daß jeder wissenschaftliche Satz in die physikalische Sprache übersetzbar ist."

[10]"Dadurch [...] wird die gesamte Wissenschaft zu Physik."

[11]"[Bei der] These von der Universalität der physikalischen Sprache [...] handelt es sich nicht um die Zurückführbarkeit der biologischen *Gesetze* auf die physikalischen, sondern um die Zurückführbarkeit der biologischen *Begriffe* [...] auf die physikalischen. Und diese Zurückführbarkeit kann, im Unterschied zu der ersteren, leicht erwiesen werden."

> in the form of an interpreted system, consisting of a specific calculus (axiom system) and a system of semantical rules for its interpretation [...]. It is, of course, logically possible to apply the same method to any other branch of science as well. But practically the situation is such that most of them seem at the present time to be not yet developed to a degree which would suggest this strict form of presentation.

So Carnap held the view that in principle, all theories can be axiomatized, that is, there is no logical inconsistency involved in this assumption. He plausibly also held the view that the better developed a theory is, the closer it comes to being axiomatizable. But Carnap also was of the opinion expressed by Suppe in his third premise, that many theories cannot be fruitfully axiomatized "at present".

Clearly, then, Carnap was not of the opinion that all theories can be explicated in the Received View. I think it is most plausible that Carnap's attitude to the Received View was like his attitude to first order axiomatization as communicated by Hempel: It is the first stage in the development of more comprehensive methods of explication, and it may permit generalizations that can deal with theories that cannot be axiomatized. The explications based on the Received View, for example of theory testing or concept formation, then have the form of conditionals: If a theory can be reconstructed according to the Received View, then the respective explication or analysis is applicable.

Carnap (1923) in fact expresses this view in his earliest paper on the Received View. He introduces an ideal physics, consisting of a completely axiomatized theory, a set of correspondence rules, and a complete description of the physical world at two points in time. Carnap (1923, 96) describes the value of such a fiction thusly:

> To determine the direction that physics should take on any level, the fiction of a completed construction of physics can be of great help, as it were, as a target at infinite distance.[12]

Later in the text, Carnap actually loosens one of his assumptions about the completed construction of physics, the assumption that at two points in time, the state of all physical magnitudes is known at all points in space. By considering what the restriction to observable phenomena means for the knowledge of the physical magnitudes, Carnap tries to arrive at a more general theory about scientific theories starting from a special case, just as in his remark about first order logic. Note that this restriction to information about observable states effects that the theoretical terms are interpreted only through the observational terms and the correspondence rules. Thus this generalization becomes a core feature of the later Received View.

Shimony describes this search for generalizations as indeed fundamental to Carnap's way of working. In an homage after Carnap's death, he writes that Carnap "took particular delight

[12]"Für die Feststellung der Richtung, in der die Physik auf irgendeiner Stufe weiterschreiten soll, kann die Fiktion eines vollendeten Aufbaues der Physik, gewissermaßen als Zielpunkt im Unendlichen, gute Dienste leisten."

in technical advances which permitted him to widen the scope of his investigations without loss of precision" (Feigl et al. 1970, XXVI). And indeed Carnap (1956b, 49) considers another generalization of the Received View. After giving examples of correspondence rules (*C*-Rules), he states:

> In the above examples, the *C*-rules have the form of universal postulates. A more general form would be that of statistical laws involving the concept of statistical probability [. . .]. A postulate of this kind might say, for example, that, if a region has a certain state specified in theoretical terms, then there is a probability of 0.8 that a certain observable event occurs [. . .]. Or it might, conversely, state the probability for the theoretical property, with respect to the observable event. Statistical correspondence rules have so far been studied very little.

If the Received View was meant as a definitive account of theory explication, such a generalization would not make sense. Hence I think it is clear that Carnap considered the Received View's reliance on axiomatization in logic a restriction that could be shed in further generalizations. Accordingly, Carnap spent the rest of his life studying probability.

To summarize, the Received View does not fail in explicating the notion of 'theory', first, because it was never meant to explicate it. Rather, it was meant to give a framework in which to explicate individual theories that are formulated precisely enough to be axiomatized. Second, not all theories were considered explicable in the Received View, but the Received View was, at least by Carnap, seen as a first step in a series of generalizations that allow to explicate more and more theories. Third, even if a reconstruction of a specific theory were to be not just a precisification, but actually deviate from the original theory, this would not be in itself a problem because this is a core idea of explications.

6 Final remarks

> [Heraclitus's] words, like those of all the philosophers before Plato, are only known through quotations, largely made by Plato or Aristotle for the sake of refutation. When one thinks what would become of any modern philosopher if he were only known through the polemics of his rivals, one can see how admirable the pre-Socratics must have been, since even through the mist of malice spread by their enemies they still appear great.
>
> (Russell 1961, 64)

While Russell attributes the mischaracterizations of opponents to malice, I think honest opponents (and even neutral expositors like, arguably, Russell) may very well make mistakes in the exposition of a philosophical position. Thus I do agree with Russell that it is always dangerous to assume a position to be accurately presented by its opponents. The principle of charity is flouted often enough to justify giving it a name and teaching it to philosophy students, and sometimes even crystal clear positions get distorted.

One such crystal clear position is the Received View's reliance on type theory. Neither Carnap, Hempel, nor Feigl ever restrict reconstructions of theories to first order logic, Carnap uses type theory in all of his expositions of the Received View, and Hempel refers to Carnap's expositions without taking issue with the use of type theory. Hempel himself uses type theory in his exposition of the relation between observational terms and measurement terms, and, like Feigl, lists explications of theories that use type theory as compatible with the Received View. That first order logic is cumbersome to use and unable to describe specific mathematical structures up to isomorphism can therefore not be an argument against the Received View.

Second, the proponents of the Received View did not demand exhaustive axiomatizations of all of the mathematics that appears in a scientific theory. Carnap describes how mathematical and logical constants with a standard interpretation can be used without mentioning their axiomatizations, and Hempel and Feigl consider reconstructions of theories that are not exhaustive to be compatible with the Received View. Hence the difficulty of arriving at exhaustive axiomatizations cannot count against the Received View.

Third, of the many meanings and uses of the term 'model', Hempel and Carnap doubt only the necessity of one use of models under one meaning: Neither considers it *necessary* that the laws of a theory be given an alternative, visualizable interpretation to determine the consequences of the theory. Nonetheless, Hempel describes a host of conveniences that come with such analogous models, and both Carnap and Hempel see much value in analogous models in the context of discovery. The Received View in fact is at least as hospitable to analogical models as the Semantic View as described by Suppes. There is, then, no obvious reason to dismiss the Received View or prefer the Semantic View because of the relevance of scientific models.

Fourth, not all explications are precisifications, so it is prima facie not a problem when the explicatum of a term does not conform to the explicandum in all clear cases. Carnap explicitly takes this stance, and Hempel relies on Carnap's account of explication. To demand that explications be precisification would furthermore undermine the basic tenet of ideal language philosophy.

Fifth, the Received View is not meant as an explication of the term 'theory'. Rather, it is meant as a framework in which to explicate specific scientific theories. Furthermore, it is arguably meant as a precise framework that allows to explicate some theories, while the explication of other theories would require either their further development or a generalization of the Received View.

Because of the last two points, it is not a problem for the Received View that it does not capture the use of 'theory' in ordinary language.

These results counter some of the criticism of the Received View, but by no means all of them. It is noteworthy, however, that it was not even necessary to engage with the content of the criticism, but only with their presumptions about the Received View. Given this comparatively easy defense, there is hope that the Received View can be resurrected as a viable method in the philosophy of science. In Carnap's spirit, one would hope that such a resurrection brings with it improvements and generalizations not only of the Received View itself, but also of the explications that rely on it.

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