Synthese (2008) 164:385–400
DOI 10.1007/s11229-008-9356-0

Wissenschaftslogik: The role of logic in the philosophy of science

Michael Friedman

Received: 12 March 2008 / Accepted: 2 June 2008 / Published online: 1 July 2008

Abstract Carl Hempel introduced what he called "Craig's theorem" into the philosophy of science in a famous discussion of the "problem of theoretical terms." Beginning with Hempel's use of 'Craig's theorem," I shall bring out some of the key differences between Hempel's treatment of the "problem of theoretical terms" and Carnap's in order to illuminate the peculiar function of *Wissenschaftslogik* in Carnap's mature philosophy. Carnap's treatment, in particular, is fundamentally anti-metaphysical—he aims to use the tools of mathematical logic to dissolve rather solve traditional philosophical problems—and it is precisely this point that is missed by his logically-minded contemporaries such as Hempel and Quine.

Keywords Logic · Philosophy of science · Theoretical terms · Ramsey-sentences

Hempel's well-known paper, "The Theoretician's Dilemma" (1958), famously introduced what Hempel calls "Craig's theorem" into the philosophy of science. In particular, Hempel considers "Craig's theorem" and the method of Ramsey-sentences as the two main logical strategies for systematizing science without using theoretical terms. Hempel's discussion proceeds within a broadly Carnapian framework, and, as we know, Carnap himself began devoting particular attention to the Ramsey-sentence approach around the same time. Beginning with Hempel's use of "Craig's theorem," I shall bring out some key differences between Hempel's attitude towards the "problem of theoretical terms" and Carnap's in order to illuminate the peculiar function of *Wissenschaftslogik* in Carnap's mature philosophy.

M. Friedman (✉)
Department of Philosophy,
Stanford University, Stanford, CA 94305, USA
e-mail: mlfriedman@stanford.edu

Craig's original paper, "On Axiomatizability within a System" (1953), was presented as an "observation" in pure recursion theory which then has "applications" to a variety of formal systems. One such application, fastened on by Hempel, concerns subsystems of a given formal system T containing a privileged (recursive) subset K of T's non-logical constants: If the theorems of T are recursively enumerable, then there exists a (primitive) recursively axiomatizable system S whose theorems are exactly those theorems of T in which no constants other than those in K occur. To see this, let F be any such theorem of T containing only non-logical constants in K. Then the (primitive) recursive set of axioms of S is comprised of those formulas F & ... & F, where the number of conjuncts n is the Gödel number of a proof of F in T.

Craig's paper was originally received by the *Journal of Symbolic Logic* on November 21, 1951. A footnote added on May 18, 1952 to the statement of the application just given then explains the case where K is the set of observational predicates of an axiomatic theory T representing a "portion of a natural science."[1] In January of 1956 Craig published a second and much less formal presentation of his idea in the *Philosophical Review* entitled "Replacement of Auxiliary Expressions." Here, in particular, he considers a number of general "replacement programs"—favored, "rightly or wrongly," by "persons with empiricist leanings."[2] Prominent among these are empiricist-motivated program for dispensing with theoretical terms in natural science, and Craig makes a special point of thanking Hempel, in the first footnote of his paper (1956, p. 39), for encouraging him to present a less technical version of his earlier ideas here.

In any case, neither Hempel nor Craig takes Craig's re-axiomatization method for avoiding theoretical terms to be satisfactory from a philosophical point of view. For example, Craig (1956, p. 49) points out that the new axioms of the system S are constructed in a "mechanical and artificial way," so that this set, in particular, "is not more perspicuous than the set of theorems." Therefore (ibid.): "The axioms fail to simplify or to provide genuine insight. This failure seems to be the principal objection to the present method."[3] Hempel (1958, p. 78) echoes the point: "[T]he manner in which

[1] See Craig (1953, footnote 9, p. 31): "In particular, suppose that T expresses a portion of a natural science, that the constants of K refer to things or events regarded as 'observable', and that the other constants do not refer to 'observables' and hence may be regarded as 'theoretical' or 'auxiliary'. Then there exists a system which does not employ 'theoretical' or 'auxiliary' constants and whose theorems are the theorems of T concerning 'observables'."

[2] See Craig (1956, p. 38): "Rightly of wrongly, persons with empiricist leanings sometimes have misgivings concerning expressions which they regard as auxiliary. This leads them sometimes to proposes that such expressions be replaced, or at least shown to be replaceable, by expressions which they regard as non-auxiliary and thus somehow 'safer.' For example, some phenomenalists reject sentences about the external wold as 'meaningless' unless they can be translated into 'equivalent' sentences about sense perceptions. They therefore urge a program of showing that such translations are always possible. Also, for example, Bridgman and other physicists propose that words like 'electron' be 'operationally defined.' Proposals of this kind we shall call *replacement programs*."

[3] Craig emphasizes this failing in stating the purpose of his paper (while referring to the earlier 1953 paper and thanking Hempel in a footnote) at the very beginning (1956, Sect. 3, p. 39): "The main purpose of this paper is to describe in a less technical and condensed manner than has been done elsewhere[1] a method of solving certain replacement programs. The method seems applicable whenever the *formulation* of the program possesses two features (to be discussed below in Sects. 4 and 5) without which the program seems to have little change of success. It should be added at once, however, that the method is artificial and that the solutions it yields are philosophically quite unsatisfactory."

the axioms, or postulates, of [S] are specified by Craig's method is [very] intricate, and the resulting system would be practically unmanageable—to say nothing of the loss of heuristic fertility and suggestiveness which results from the elimination of the theoretical concepts and hypotheses. For empirical science, therefore, this method of dispensing with theoretical expressions would be quite unsatisfactory."

Nevertheless, it is clear that Hempel thinks that a detailed consideration of Craig's method is very important for the problem about theoretical terms with which he is most concerned. And, more generally, it is clear that Hempel takes technical results in formal logic to illuminate the peculiar epistemic role fulfilled by theoretical terms and hypotheses in systematizing observational data—both deductively and inductively. In this context, Hempel takes the failure of the Craig replacement to provide a proper *inductive* systematization of the observable data to be an even more serious problem than the logical complexity and artificiality of its formal axioms.[4]

By contrast, Hempel takes the Ramsey-sentence of a given theory T to provide a much better systematization than the Craig replacement, both deductively and inductively. This is clear, for example, from the fact that the Ramsey-sentence of T (assumed to be finitely axiomatizable) has basically the same logical form as T itself, except that the theoretical terms of T are now replaced by existentially quantified variables. More formally, if we represent T itself by $\mathbf{T}(O_1, \ldots, O_m; T_1, \ldots, T_n)$, where $O_1, \ldots, O_m$ are the observational and $T_1, \ldots, T_n$ are the theoretical terms of T, then the Ramsey-sentence of T, R(T), is given by $\exists X_1, \ldots, \exists X_n \mathbf{T}(O_1, \ldots, O_m; X_1, \ldots, X_n)$.[5] For Hempel, the main problem with R(T), however, is that it continues to have the same existential commitments as T (1958, p. 81): "The Ramsey-sentence associated with an interpreted theory [T] avoids reference to hypothetical entities only in letter . . . rather than spirit. For it still asserts the existence of certain entities of the kind postulated by [T], without guaranteeing any more than does [T] that those entities are observable or at least fully characterizable in terms of observables. Hence, Ramsey-sentences provide no satisfactory way of avoiding theoretical concepts."

4 See Hempel (1958, pp. 78–80) for the problem of inductive systematization arising in the Craig method. The problem arises for any theory in which a number of observational predicates give necessary but insufficient conditions for the application of a given theoretical predicate. A particular individual may be observed to have some of the observational predicates, so that we then have inductive but not deductive grounds for attributing the given theoretical predicate to the individual in question; and it follows deductively from this (theoretical) attribution that the individual is characterized by the other observational predicates as well. The result is an inductive connection among observational predicates essentially mediated by the theory (for a concrete example see footnote 6 below); and the Craig replacement, which dispenses with the given theoretical predicate, fails to capture this inductive connection.

5 In particular, any existential instantiation of R(T) has exactly the same logical form as T itself—indeed, T is simply one such existential instantiation of R(T). In reference to the issue about inductive systematization raised in footnote 4 above, then, it would appear that R(T), via its existential instantiations, would effect the same inductive systematization of the observational data as does T. Hempel does not explicitly make this claim, however, but says only that R(T) provides an alternative, "inductively simpler, method of obtaining a functional equivalent, in observational terms, of a given interpreted theory" (1958, p. 80). In the context of the remainder of his paper this issue would appear to depend, for Hempel, on the question of how one regards the instantiations of R(T) semantically—for it is clear that they are completely on a par with T from a purely syntactic point of view.

It is precisely here, however, that Hempel's attitude towards the "problem of theoretical terms" diverges most clearly from Carnap's. Hempel's paper prominently cites an earlier paper of Carnap's, "The Methodological Character of Theoretical Concepts," published in 1956. This paper (appearing in the first volume of Minnesota Studies in the Philosophy of Science, immediately preceding the volume containing Hempel's paper) features Carnap's view of theoretical terms as only *partially* interpreted—which, in turn, is closely linked with Carnap's own use of the Ramsey-sentence in contemporaneous work. According to the partial interpretation view (which originated in Carnap's monograph on *Foundations of Logic and Mathematics* published in 1939), only the observational terms of a scientific theory are semantically interpreted, by specifying observable properties and relations as their designata. The theoretical terms, by contrast, are semantically uninterpreted, and are only implicitly defined, in the sense of Hilbert, by the axioms and postulates of the relevant theory (e.g., Maxwell's equations for the electromagnetic field). Among these axioms and postulates, however, are mixed sentences or *correspondence rules*, which set up (lawlike) relationships among theoretical and observational terms; and, in this way, the theoretical terms and sentences receive a *partial* interpretation in terms of the connections they induce among observables. For example, Maxwell's equations, in the presence of suitable correspondence rules relating values of the electromagnetic field to actual measurements (of electric and magnetic intensities, and the like), generate observable predictions and thus have empirical content.[6]

Are we thereby "ontologically committed" to the existence of a mysterious unobservable entity corresponding to our term for the electromagnetic field? Carnap explicitly considers this question and devotes considerable effort towards trying to defuse it. He stipulates, first of all, that the values of the variables of his theoretical language L_T range over a domain of entities including a denumerable sequence isomorphic to the natural numbers and closed over the formation of relations and classes. The domain therefore contains natural numbers, real numbers, sets of real numbers, and so on. "Now," Carnap (1956b, p. 43) continues, "we proceed to physics." We conceive space-time points as quadruples of real numbers which thereby belong to the (purely mathematical) domain D we have already constructed. Moreover, physical magnitudes (such as the electromagnetic field) are functions whose arguments are space-time points and values are real numbers or n-tuples of real numbers. Thus, all the entities needed for values of our variables have already been constructed within

[6] This procedure of generating empirical content in the form of observable connections among measurements amounts to an inductive but not deductive systematization of the observational data in the sense of footnote 4 above, for no set of empirical measurements, for example, *deductively* implies that the electromagnetic field has a particular value at a given space-time point. Hempel (1958) discusses Carnap's notion of partial interpretation at considerable length (as represented in both Carnap (1939) and Carnap (1956b)), and, as suggested in footnote 5 above, Hempel's apparent hesitations about the inductive systematization provided by R(T) are connected with his parallel hesitations about the notion of partial interpretation. By contrast, Carnap himself (in both (1939) and (1956b)) appears simply to take it for granted that a partially interpreted theory yields the same inductive systematizations that we ordinarily take the theory to provide.

our purely mathematical domain D.[7] And the same holds, Carnap adds, for the entities of biology, psychology, and the social sciences.[8]

Carnap then cautions the reader (1956b, pp. 44–45):

> We have considered some of the kinds of entities referred to in mathematics, physics, psychology, and the social sciences and have indicated that they belong to the domain D. However, I wish to emphasize here that this talk about the admission of this or that kind of entity as values of variables in L_T is only a way of speaking intended to make the use of L_T, and especially the use of quantified variables in L_T, more easily understandable. Therefore the explanations just given must not be understood as implying that those who accept and use a language are thereby committed to certain "ontological" doctrines in the traditional metaphysical sense. The usual ontological questions about the "*reality*" (in an alleged metaphysical sense) of numbers, classes, space-time points, bodies, minds, etc., are pseudo-questions without cognitive content.

By contrast, questions about the reality of entities as asked and answered within science—a question, for example, about the reality of the electromagnetic field—can be given a "good scientific meaning" (1956b, p. 45) if, for example, we understand the acceptance of the reality of the electromagnetic field "as the acceptance of a language L_T and in it a term, say 'E,' and a set of postulates T which includes the classical laws of the electromagnetic field (say, the Maxwell equations) as postulates for 'E'." Carnap continues (ibid.): "For an observer X to 'accept' the postulates of T, means here not simply to take T as an uninterpreted calculus, but to use T together with specified correspondence rules C for guiding his expectations by deriving predictions about future observable events from observed events with the help of T and C."[9]

In his reply to Hempel in the Carnap Schilpp volume (published in 1963, but likely written in the late 1950s), Carnap returns to this question and explicitly considers the passage from Hempel (1958, p. 81), quoted above, which complains about the existential commitments of the Ramsey-sentence (1963, p. 963):

---

[7] See Carnap (1956b, p. 44): "When a physicist describes a physical system or a process occurring in it or a momentary state of it, he ascribes values of physical magnitudes (e.g., mass, electric charge, temperature, electromagnetic field intensity, energy, and the like) either to the space-time region [occupied by the system] as a whole or to its points. The values of a physical magnitude are either real numbers or n-tuples of such. Thus a physical magnitude is a function whose values are either real numbers or n-tuples of such. Thus, on the basis of our conventions, the domain D contains space-time points and regions, physical magnitudes and their values, physical systems and their states. A physical system itself is nothing else than a space-time region characterized in terms of magnitudes. In a similar way, all other entities occurring in physical theories can be shown to belong to D."

[8] See Carnap (1956b, p. 44): "Psychological concepts are properties, relations, or quantitative magnitudes ascribed to certain space-time regions (usually human organisms or classes of such). Therefore they belong to the same logical types as concepts of physics, irrespective of the question of their difference in meaning and way of definition. . . . Thus the domain D includes also all entities referred to in psychology. The same holds for the social sciences."

[9] Although Carnap does not explicitly cite this paper here, the point he is making about "ontological commitment" is clearly linked to Carnap (1950)—which, in turn, is Carnap's response (among other things) to Quine (1948). We shall return to this matter below.

> I agree with Hempel that the Ramsey-sentence does indeed refer to theoretical entities by the use of abstract variables. However, it should be noted that these entities are not unobservable physical objects like atoms, electrons, etc., but rather (at least in the form of the language which I have chosen in [Carnap (1956b)]) purely logical-mathematical entities, e.g., natural numbers, classes of such, classes of classes, etc. Nevertheless [the Ramsey-sentence of T] is obviously a factual sentence. It says that the observable events in the world are such that there are numbers, classes of such, etc., which are correlated with the events in a prescribed way and which have among themselves certain relations; and this assertion is clearly a factual statement about the world.

Of course Carnap's response will strike anyone seriously concerned with the general question of the "reality" of theoretical entities (as Hempel apparently is) as something of a cheat—and this will be so whether one favors a "realist" or "instrumentalist" answer to the question. Carnap's point, however, is that all such general "ontological" questions—in the sense in which they are raised and answered within traditional philosophy (including traditional philosophy of science)—are metaphysical pseudo-problems, in principle incapable of genuinely "scientific" answers.[10]

This becomes especially clear in the monograph *Philosophical Foundations of Physics*—which was originally composed from transcripts of Carnap's lecture course at UCLA in the Fall of 1958 and published in 1966; a second edition then appeared in 1974 with the title (formerly subtitle) *An Introduction to the Philosophy of Science*. Here Carnap explicitly considers the general issue between "realism" and "instrumentalism" about theoretical entities in the context of a lengthy discussion of Ramsey-sentences (1974, p. 256):

> It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here, is essentially this. I believe that the question should not be discussed in the form: "Are theoretical entities real?" but rather in the form: "Shall we prefer a language of physics (and of science in general) that contains theoretical terms, or a language without such terms?" From this point of view the question becomes one of preference and practical decision.[11]

[10] We are therefore concerned, once again, with the topic of Carnap (1950). Hempel (1958, p. 85, footnote 69) explicitly invokes Quine (1948) while raising the issue of the "ontological commitment" of the Ramsey-sentence, but he does *not* discuss Carnap (1950)—either here or in Hempel (1963).

[11] A footnote added to this last sentence finally explicitly refers the reader to Carnap (1950). As Martin Gardiner explains in his Forward to the second edition (1974, v–vi): "In response to a friendly letter from Grover Maxwell, Carnap agreed (shortly before his death in 1970) that his all-too-brief comments on the conflict between instrumentalism and realism, with respect to the nature of scientific theory, be clarified. With this in mind, he made certain alterations on the two pages [255–256], and added a new footnote referring to a 1950 paper which gives his views in more detail." The original passage in the first edition reads (1966, p. 256): "It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here, is that the conflict between the two approaches is essentially linguistic. It is a question of which way of speaking is to be preferred under a given set of circumstances. To say that a theory is a reliable instrument—that is, that the predictions of observable events that it yields will be confirmed—is essentially the same as saying that the theory is true and that the theoretical, unobservable entities it speaks about exist. Thus, there is no incompatibility between

Moreover, it clear from the context that the two languages in question, for Carnap, are a standard language containing (constant) terms for electrons, the electromagnetic field, and so on, and a Ramsified version of this language where these same terms are replaced by existentially quantified variables. Finally, Carnap's own preference, as he states in the reply to Hempel (immediately following his response concerning the existential commitments of the Ramsey-sentence quoted above), is to retain the standard language using (constant) theoretical terms (1963, p. 963): "I do not propose to abandon the theoretical terms and postulates, as Ramsey suggests, but rather to preserve them in L_T and simultaneously to give an important function to the Ramsey-sentences. . . . Their function is to serve in the explication of experiential import and, more importantly, in the explication of analyticity."

To see what is at stake here, for Carnap, recall that the Ramsey-sentence R(T) of T is given by $\exists X_1, \ldots, \exists X_n \mathbf{T}(O_1, \ldots, O_m; X_1, \ldots, X_n)$. The Carnap sentence C(T) of T is then given by the conditional with R(T) as antecedent and T itself as consequent: i.e., $C(T) = R(T) \rightarrow T$. T is then obviously logically equivalent to the conjunction of C(T) and R(T), where Carnap views C(T) as the analytic part of T and R(T) as the empirical or synthetic part. It is for this reason (the precise force of which we shall explore below) that Carnap's own preference or proposal is to adopt the "realist" language where we retain the (constant) theoretical terms of T—and therefore, in effect, add C(T) to R(T).[12]

A footnote to the deflationary remarks about the choice between "realism" and "instrumentalism" in Carnap (1974, p. 256) refers the reader to his paper "Empiricism, Semantics, and Ontology" (1950), where, of course, Carnap famously adopts a deflationary attitude towards all traditional "ontological" questions quite generally (compare footnote 11 above). In particular, Carnap (1950) articulates a general distinction between "internal" questions, which can be raised and settled within a given linguistic framework introducing this or that type of entities as values of its variables (numbers, physical things, space-time points, and so on), and what Carnap calls "external questions, i.e., philosophical questions concerning the existence or reality of the total system of the new entities" (1950/1956a, p. 214). Concerning the latter Carnap remarks (ibid.): "Many philosophers regard a question of this kind as an ontological

Footnote 11 continued
the thesis of the instrumentalist and that of the realist. At least, there is no incompatibility so long as the former avoids such negative assertions as, '. . . but the theory does not consist of sentences which are either true or false, and the atoms, electrons, and the like do not really exist'."

[12] Thus, the practical choice between "realist" and "instrumentalist" languages corresponds to the choice between using only R(T) alone or the conjunction of R(T) and C(T) (= T itself). In the former case our language contains no (constant) theoretical terms; in the latter, of course, it does. What Carnap is saying in his reply to Hempel is that he in fact prefers the latter alternative. The terminology is somewhat delicate, however. In the first edition (1966, pp. 254–255), immediately after explaining the "Ramsey point of view," Carnap adds (p. 255): "This point of view is sometimes called the 'instrumentalist' view of theories." In the corresponding passage from the second addition, however, after explaining the "Ramsey point of view" in exactly the same words, Carnap instead says (1974, p. 255): "With respect to the nature of theories and the entities referred to in theories, there are at present two main views, often labeled 'instrumentalism' and 'realism'." The reason Carnap makes this change, in my opinion, is simply that he is now explicitly proposing to *replace* the question "Are theoretical entities real?" with the question "Shall we prefer a language of physics (and of science in general) that contains theoretical terms, or a language without such terms?".

question which must be raised and answered *before* the introduction of the new language forms." Carnap's view, on the contrary, is that, although there is certainly a practical question of which such linguistic framework to adopt, there is absolutely no corresponding theoretical question (ibid.): "Above all, it must not be interpreted as referring to an assumption, belief, or assertion of 'the reality of the entities'. There is no such assertion. An alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content."[13]

This paper, in turn, represents Carnap's most detailed and systematic account of an attitude towards characteristically philosophical questions he had been developing from at least the *Logical Syntax of Language* (1934) onwards. What was especially salient for Carnap at this time (the late 1920s and early 1930s) was the "foundations crisis" then afflicting modern logic and the foundations of mathematics precipitated by Brouwer's articulation of intuitionism and Hilbert's development of proof-theory in response to Brouwer. Carnap (1934) then attempted to diffuse *this* "metaphysical" issue, which, from his point of view, appeared to threaten the properly scientific status of modern mathematics and logic.[14]

In conformity with the metamathematical method of Hilbertian proof-theory we view any formulation of logic and mathematics as a syntactically described formal system. In light of Gödel's recently published incompleteness theorems, however, we do not pursue the Hilbertian project of constructing a proof of the consistency of classical mathematics using finitary means acceptable to the intuitionist. Instead, we formulate both a formal system or calculus conforming to the strictures of intuitionism and a much stronger system adequate for full classical mathematics. For both systems, moreover, we define a notion of logical truth (analyticity) intended formally or syntactically to express their essential independence from all factual content. Finally, and most importantly, Carnap formulates the principle of tolerance: *both* types of system should be syntactically described and investigated, and the choice between them, if there is one, should then be made on practical or pragmatic grounds rather than prior, purely philosophical commitments.[15]

In particular, contrary to Quine's well-known portrayal of Carnap's position—as presented, for example, in Quine (1963)—Carnap's own emphasis on the importance of the analytic/synthetic distinction is by no means derived from a foundational epistemological program aiming to explain how logical and mathematical certainty is possible in terms of truth-by-convention or truth-in-virtue-of-meaning. Rather, according to precisely the principle of tolerance, the point of regarding the statements of logic and mathematics as analytic lies in *our freedom to choose* which system of logic and mathematics best serves the formal deductive needs of empirical science.[16] Classical

[13] Compare footnotes 9 and 10 above, together with the paragraphs to which they are appended.

[14] For more detailed discussion of the issues sketched here and in the next several paragraphs see Friedman (2006, 2007).

[15] See Carnap (1934, Sect. 17): "*In logic there is no morality*. Everyone may construct his own logic, i.e., his own form of language, as he wishes. Only, if he wants to discuss it with us, he must clearly indicate how he wishes to construct it, [and he must] give syntactic rules instead of philosophical arguments."

[16] The point is developed in Friedman (2006, 2007). From this perspective (unlike Quine's more "foundationalist" reading) the problem of giving a satisfactory formal explication of analyticity is less

mathematics, for example, is much easier to apply, especially in physics, than intuitionistic mathematics, while the latter, being logically weaker, is less likely to result in contradiction.[17] The choice between the two systems is therefore purely practical or pragmatic, and it should thus be sharply separated, in particular, from all traditional philosophical disputes about what mathematical entities "really are" (independent "Platonic" objects or mental constructions, for example) or which such entities "really exist" (only natural numbers, for example, or also real numbers—that is, sets of natural numbers). Carnap aims to use the new tools of metamathematics definitively to dissolve all such metaphysical disputes and to replace them, instead, with the much more rigorous and fruitful project of language planning, language engineering—a project which, as Carnap understands it, has no involvement whatsoever with any traditional epistemological program. Indeed, as Carnap clearly and emphatically states in *Logical Syntax*, the new discipline he here calls *Wissenschaftslogik* (the logic of science) "*takes the place of the inextricable tangle of problems one calls philosophy*" (1934, Sect. 72).

It is precisely here, in fact, that the true philosophical radicalism of Carnap's position clearly emerges. "Von der Erkenntnistheorie zur Wissenschaftslogik" (from epistemology to the logic of science), published in 1936, argues that *all* traditional epistemological projects, including his own earlier project in the *Aufbau*, must now be renounced as "unclear mixtures[s] of psychological and logical components."[18] Whereas, for example, the broadly pragmatic and holistic epistemology Quine develops in "Two Dogmas of Empiricism" (1951) is intended as a replacement for what Quine takes to be the epistemology of logical empiricism (i.e., the *Aufbau*), Carnap is breaking decisively with the entire epistemological tradition. What Carnap now calls the logic

Footnote 16 continued
philosophically urgent: we assert that analytic sentences are empty of content and therefore subject only to free (purely pragmatic or conventional) choice, but not that their possession of this feature somehow explains the special epistemic status of logic and mathematics. (This is not to say, however, that the problem of giving a satisfactory formal explication is not important at all.) It is also true (as Stathis Psillos has emphasized to me) that synthetic sentences can be subject to free (pragmatic or conventional) choice as well, in so far as we still have a choice whether or not to reject a particular empirical hypothesis when, in conjunction with other hypotheses, it comes into conflict with observations: see Carnap (1934, Sect. 82). The crucial point, however, is that analytic sentences, in Carnap's view, constitute the inferential framework of empirical science rather than any part of its content, whereas synthetic hypotheses, by contrast, are themselves linked to observations by precisely such a framework—albeit in a holistic fashion.

17 Carnap (1939, pp. 192–193) puts this point especially clearly: "[I]f we regard interpreted mathematics as an instrument of deduction within the field of empirical knowledge rather than as a system of information, then many of the controversial problems are recognized as being questions not of truth but of technical expedience. The question is: Which form of the mathematical system is technically most suitable for the purpose mentioned? Which one provides the greatest safety? If we compare, e.g., the systems of classical mathematics and of intuitionistic mathematics, we find that the first is much simpler and technically more efficient, while the second is more safe from surprising occurrences, e.g., contradictions." Carnap (1934, Sect. 16) takes his Language I—a version of primitive recursive arithmetic—to represent the point of view of intuitionism. This system, being quantifier free, is of course much weaker than Heyting arithmetic: in particular, it follows from Gödel's double-negation translation that Heyting arithmetic and Peano arithmetic are equi-consistent relative to one another.

18 See Carnap (1936, p. 36): "It seems to me that *epistemology*, in the form it has taken until now, is *an unclear mixture of psychological and logical components*. This holds even for the works of our Circle, my own earlier work not excepted."

of science is in no way concerned with either explaining or justifying our scientific knowledge by exhibiting its ultimate basis; it is rather concerned with developing a new role for philosophy vis-à-vis the empirical sciences that will maximally contribute to scientific progress while, at the same time, avoiding all the traditional metaphysical disputes and obscurities which have constituted (and, according to Carnap, continue to constitute) serious obstacles to progress in both the sciences and scientific philosophy.

Thus, armed with the new logico-mathematical tools of modern logic, the Carnapian logician of science can participate, together with the scientists themselves, in the articulation, clarification, and development of formal inferential frameworks for articulating empirical theories and testing them by experimental methods. For example, beginning in the mid to late 1940s, and continuing throughout the last twenty-five years of his long and fruitful career, Carnap did just this with respect to the probabilistic and statistical inferential frameworks now being applied with ever-increasing frequency in the physical, biological, and social sciences.[19] However, unlike the empirical scientists themselves (the physicists, biologists, and social scientists), the logician of science, as such, is not concerned with then actually testing empirical theories within such inferential frameworks. Moreover, unlike the applied mathematician (for example, the statistician), who also develops formal methods for use in the empirical sciences, the logician of science has a characteristically philosophical interest in developing a systematic method for defusing persistent and unresolvable metaphysical controversies which, in Carnap's view, constitute an ever-present obstacle to scientific progress.[20] In the case of his work on the logical foundations of probability and statistical inference, for example, Carnap was especially concerned with defusing the traditional philosophical debate about the "true nature" of probability—subjective or objective—by drawing a sharp distinction between two different concepts of probability: logical or epistemic (degree of confirmation) and empirical or physical (long-run relative frequency).

Carnap's stance vis-à-vis the empirical sciences is therefore quite different from the perspective we are familiar with in either traditional philosophy or most twentieth-century philosophy of science. Carnap does not attempt to answer any general questions about the possibility or ultimate justification of scientific knowledge; nor,

[19] See Zabell (2007) for discussion of how Carnap's work on the logical foundations of statistical inference, although somewhat outside of the mainstream, did in fact interact fruitfully with that of working statisticians.

[20] A particularly clear and eloquent statement comes at the conclusion of Carnap (1950/1956a, p. 221) (where the particular metaphysical issue in question concerns the legitimacy of postulating "abstract entities"): "The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. To decree dogmatic prohibitions of certain linguistic forms instead of testing them by their success or failure in practical use, is worse than futile; it is positively harmful because it may obstruct scientific progress. The history of science shows examples of such prohibitions based on prejudices deriving from religious, mythological, or other irrational sources, which slowed up the developments for shorter or longer periods of time. Let us learn from the lessons of history. Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. *Let us be cautious in making assertions and critical in examining them, but tolerant in permitting linguistic forms*."

a fortiori, does he attempt to give any general answer to the question of why (or whether) we should take the entities postulated by empirical science to be "real." Carnap does not—in the style of Russell or Quine (or in the style of Hempel)—use modern mathematical logic as a new kind of philosophical tool for shedding light on these general epistemological and ontological questions. Instead, *Wissenschaftslogik*, for Carnap, has both a positive and negative role in the ongoing practice of empirical science itself.[21]

On the positive side, as I have said, the Carnapian logician of science participates, together with the scientists themselves (especially applied mathematicians), in the development and clarification of formal inferential frameworks for articulating empirical theories and testing them by experimental methods. On the negative side, however, the Carnapian logician of science is also concerned with a systematic method for defusing persistent and unresolvable metaphysical controversies. It is this last task, in particular, that makes Carnapian *Wissenschaftslogik* characteristically philosophical and explains why, for Carnap, the formal tools we employ must be drawn from mathematical logic (and not simply from informal mathematics—as employed, for example, by statisticians). Whereas formal logic, throughout much of the modern philosophical tradition (beginning with Leibniz and culminating in the twentieth-century mathematical philosophy articulated by Frege and Russell) has had a fundamental importance for epistemology and metaphysics, mathematical logic, in Carnap's hands, has a fundamental importance for *anti*-epistemology and *anti*-metaphysics instead: its role is precisely to safeguard our ongoing practice of developing empirical scientific theories within formal mathematical frameworks from epistemological and metaphysical contamination.

Let us return, as a final illustration of this point, to Carnap's treatment of the "problem of theoretical terms." Carnap makes it very clear when he first introduces his conception of partial interpretation that this perspective on theoretical terms takes its starting point from an increasing use of the abstract (Hilbertian) axiomatic method in modern physics (1939, p. 209):

> The development of physics in recent centuries, and especially in the past few decades, has more and more led to that method in the construction, testing, and application of physical theories which we call *formalization*, i.e., the construction of a calculus supplemented by a [partial—MF] interpretation. It was the progress of knowledge and the particular structure of the subject matter that suggested and made practically possible this increasing formalization. In consequence it became more and more possible to forego an "intuitive understanding" of the abstract terms and axioms and theorems formulated with their help.

[21] Carnap's point of view is therefore quite incompatible with a principled distinction between the task of the empirical scientist, on the one side, and that of the logician of science, on the other. By contrast, Hempel appears to be committed to just such a principled distinction while replying to Braithwaite's criticism of operationalism for entailing continual revisions in the definitions of our theoretical terms (1958, p. 69): "For clearly, the procedure of expanding a theory at the cost of changing the definitions of some theoretical terms is not logically faulty; nor can it even be said to be difficult or inconvenient for the scientist, for the problem at hand is rather one for the methodologist or logician, who seeks to give a clear 'explication' or 'logical reconstruction' of the changes occurring in an expansion of a given theory."

Carnap sees the theories of relativity and quantum mechanics as the culmination of this development—where the use of highly abstract terms introduced by something like Hilbertian implicit definitions (terms such as 'electron,' 'electromagnetic field,' 'metric-tensor,' 'psi-function,' and so on) has become a pervasive and essential feature of physical practice.[22]

Empiricism, for Carnap, means that we always begin our theorizing within our best current mathematically formulated physical theories, and so there can be no room for empiricist doubts, from this point of view, about the use of theoretical terms in general. For Carnap, as we have seen, the only choice remaining is that between taking our theoretical terms as *constants*, following his own conception of partial interpretation, or rather taking them as (existentially quantified) *variables*, following Ramsey. Moreover, Carnap's suggested proposal, in response to this choice (which, as we have seen, represents his reformulation of the choice between "realism" and "instrumentalism"), is to retain theoretical terms as genuine primitive constants, while relegating the use of Ramsey-sentences to a subsidiary role—that of allowing us clearly to distinguish between the analytic and properly empirical components of our axiomatically formulated physical theories.[23]

Why, however, is Carnap dissatisfied with the Ramsey-sentence itself as a formulation of our scientific theory? The problem, from Carnap's point of view, is purely one of actual deductive practice. Suppose we were to attempt to make deductions from the Ramsey-sentence R(T) of the axioms of T. We would need to proceed by an instantiation of all the existentially quantified variables, followed by ordinary logico-mathematical reasoning on the basis of this existential instantiation, and concluding with an existential generalization whereby all the existential quantifiers are then reintroduced at the end. This procedure is very complex and cumbersome, and, most importantly, it does not correspond to the way in which we in fact make deductions from axioms in scientific practice—where, in effect, we treat the axioms of T as an Hilbertian implicit definition of the *constant* theoretical terms of T, and we

[22] For the case of quantum mechanics, in particular, see Carnap (1939, pp. 210–211): "If we demand from the modern physicist an answer to the question what he means by the symbol 'ψ' of his calculus, and are astonished that he cannot give an answer, we ought to realize that the situation was already essentially the same in classical physics. There the physicist could not tell us what he meant by the symbol 'E' in Maxwell's equations. ... The situation of the modern physicist is not essentially different. He knows how to use the symbol 'ψ' in the calculus in order to derive predictions which we can test by observations. (If they have the form of probability statements, they are tested by statistical results of observations.) Thus the physicist, although he cannot give us a translation into everyday language, understands the symbol 'ψ' and the laws of quantum mechanics. He possesses that kind of understanding which alone is essential in the field of knowledge and science."

[23] See footnote 12 above, together with the paragraph to which it is appended and the two preceding paragraphs. Carnap (1963, pp. 962–963) makes it clear that he fully agrees with Hempel's point that the *inductive* systematization of observational data provided by scientific theories is lost on a method (like Craig's) which simply dispenses with theoretical terms; and it also seems clear, from the context, that Carnap takes the method of Ramsey-sentences to be successful in this respect (compare footnotes 4–6 above, together with the paragraphs to which they are appended). We shall consider Carnap's reasons for nevertheless preferring to add C(T) to R(T) immediately below.

then proceed to engage in ordinary logico-mathematical reasoning from these axioms without worrying about existential instantiation and generalization.[24]

From this point of view, Carnap's proposal for factoring T into a conjunction of R(T) and C(T) is actually quite ingenious and even rather deep. For the Carnap sentence is now seen to take over the role of precisely existential instantiation from the Ramsey-sentence, and it then allows us to proceed with ordinary mathematical reasoning in the style of Hilbert without worrying about cumbersome restrictions on existential variables in natural deduction. Whereas existential instantiation, of course, is not a logically valid inference, the Carnap sentence $C(T) = \exists X_1, \ldots, \exists X_n \mathbf{T}(O_1, \ldots, O_m; X_1, \ldots, X_n) \rightarrow \mathbf{T}(O_1, \ldots, O_m; T_1, \ldots, T_n)$, taken as a *non-logical* axiom of T, is now seen, nonetheless, as an *analytic* postulate—a conventional choice of (constant) names arbitrarily given to a sequence of values of the variables $X_1, \ldots, X_n$, which, by the Ramsey-sentence, must (synthetically) exist.[25] Carnap is thereby able, in the context of axiomatically formulated physical theories, to reconcile the use of Hilbert-style implicit definitions with a Fregean insistence that what is actually defined in this way is a higher-order logico-mathematical structure. The Hilbert-style implicit definition, in this context, is given by T itself; the existence of an appropriate higher-oder logical-mathematical structure is (synthetically) asserted by R(T); and the two viewpoints are reconciled by Carnap's at first sight quite trivial trick of factoring T into a conjunction of R(T) and C(T).[26]

Carnap's treatment of the "problem of theoretical terms" within his version of the axiomatic method again has both a positive and a negative dimension. On the negative side, it frees us from the intractable philosophical debate between "instrumentalism" and "realism." For the only "ontological" question that now matters concerns the existence of an appropriate mathematical structure into which the observable

[24] Carnap makes this clear in *Philosophical Foundations of Physics* (in both editions). Suppose we wish to formulate a simple statement attributing a definite mass to a certain object. Such a statement, in the method of Ramsey-sentences, would ultimately require "an immensely long sentence, which contains the formulas corresponding to all the theoretical postulates, all the correspondence postulates, and their existential generalizations" (1966/1974, p. 254); and it is for precisely this reason, in particular, that "it would be inconvenient to substitute the Ramsey way of speaking for the ordinary discourse of physics in which theoretical terms are used," and "physicists find it vastly more convenient to talk in the shorthand language that includes theoretical terms" (ibid.). The Carnapian logician of science, in this respect, therefore simply agrees with the practice of working physicists: compare footnote 21 above.

[25] The connection between the Carnap sentence and existential instantiation comes out even more explicitly in a reformulation of his approach using Hilbert's ε-operator. For the ε-operator selects an arbitrary sequence of values of the theoretical variables that satisfies the theory, and the crucial analytic postulate then becomes an *explict definition* of the theoretical terms via this particular (arbitrarily selected) sequence of values. For discussion of the ε-operator reformulation, together with Carnap's 1959 Santa Barbara lecture on theoretical terms, see Psillos (2000). More generally, see Psillos (1999, chapter 3) for an extended discussion of "Carnap's neutralism" in the context of a detailed account of the historical development of Carnap's views. I intend to discuss the relationship between my interpretation and Psillos's in future work.

[26] It is precisely here that Carnap goes beyond the practice of working physicists (even working *mathematical* physicists). For Carnap's factorization of T into R(T) and C(T) shows, on the one hand, that the entire (synthetic) content of T is given by R(T), and, on the other, that the additional factor C(T) is a purely analytic "meaning-postulate." Hence, since the entities whose existence is (synthetically) asserted by R(T) are purely logico-mathematical entities, "we can now avoid all the troublesome metaphysical questions that plague the original formulation of theories" (1966/1974, p. 252): compare again footnote 21 above, together with the paragraph to which it is appended and the following paragraph.

phenomena are to be embedded—and this question, in turn, is answered within the ongoing practice of modern physics itself. That is, the progress of modern physics, from Carnap's point of view, consists precisely in the discovery of appropriate systems of abstract axioms (and correspondence rules) characterizing the mathematical structures in question. On the positive side, therefore, it then allows us seriously to address the axiomatic foundations of modern physics (together with mathematical physicists) entirely free of all such metaphysical distractions.[27]

In particular, Carnap discusses the axiomatic foundations of quantum mechanics in the final chapter of *Philosophical Foundations of Physics*.[28] It is not yet clear, he says, how the language of physics must change in response to the fundamentally non-classical character of quantum probabilities, but Carnap is sure, nonetheless, that the modern axiomatic method represents our very best hope for progress (1966/1974, p. 291):

> I am convinced that two tendencies, which have led to great improvements in the language of mathematics during the last half century, will prove equally effective in sharpening and clarifying the language of physics: the application of modern logic and set theory, and the adoption of the axiomatic method in its modern form, which presupposes a formalized langauge system. In present-day physics, in which not only the content of theories but the entire conceptual structure of physics is under discussion, both those methods could be of an enormous help.

[27] In focussing on the question of the existence of an appropriate mathematical structure into which the observable phenomena are to be embedded Carnap's position is similar to the "constructive empiricism" defended in Van Fraassen (1980). The crucial difference, however, is that, whereas van Fraassen's whole point is to defend (philosophical) "instrumentalism" against (philosophical) "realism," Carnap aims at leaving this issue entirely behind in favor of what he takes to be a much more productive engagement with the ongoing practice of (mathematical) physics itself. Demopoulos (2007) provides an outstanding discussion of the motivations for Carnap's view of theoretical terms and its relationship, in particular, with the Hilbertian axiomatic method. In the course of his discussion Demopoulos criticizes Carnap's use of the Ramsey-sentence for requiring only the existence of an appropriate mathematical structure—which, as Demopoulos shows, is "almost analytic" in so far as it logically follows from the totality of the observational consequences of T together with a cardinality assumption. Carnap's own view, however, is that the synthetic content of T does *not* exceed its empirical content, and he aims to defend this view, moreover, against the metaphysical excesses of both "realism" and "instrumentalism." Demopoulos, from this point of view, is relying on a fundamentally "realist" intuition about what the (synthetic) content of a scientific theory should be taken to be.

[28] As we saw in footnote 22 above, the case of quantum mechanics was an especially important part of Carnap's motivations for originally proposing the partial interpretation view of theoretical terms in 1939. In *Philosophical Foundations of Physics* Carnap takes Einstein's distinction between mathematical and physical geometry to show that Carnapian *Wissenschaftslogik* (with its analytic/synthetic distinction) provides a fruitful perspective on the general theory of relativity, and he urges us, accordingly, to extend this perspective to quantum mechanics as well (1966/1974, pp. 257–258): "In my opinion, a sharp analytic/synthetic distinction is of supreme importance for the philosophy of science. The theory of relativity, for example, could not have been developed if Einstein had not realized that the structure of physical space and time cannot be determined without physical tests. He saw clearly the sharp dividing line that must always be kept in mind between pure mathematics, with its many types of logically consistent geometries, and physics, in which only experiment and observation can determine which geometries can be applied most usefully to the physical world. This distinction between analytic truth (which includes logical and mathematical truth) and factual truth is equally important today in quantum theory, as physicists explore the nature of elementary particles and search for a field theory that will bind quantum mechanics to relativity." For futher discussion of Carnap's relation to Einstein in this context see Friedman (2006).

> Here is an exciting challenge, which calls for close cooperation between physicists and logicians—better still, for the work of younger men who have studied both physics and logic. The application of modern logic and the axiomatic method to physics will, I believe, do much more than just improve communication among physicists and between physicists and other scientists. It will accomplish something of far greater importance: it will make it easier to create new concepts, to formulate fresh assumptions.

Thus, it is precisely here, for Carnap, that his conception of *Wissenschaftslogik* promises to bear perhaps its most significant fruit.

The main examples Carnap gives of how "[t]he revolutionary nature of the Heisenberg uncertainty principle has led some philosophers and physicists to suggest that certain basic changes be made in the language of physics" (1966/1974, p. 288) are proposals for adopting some kind of non-classical logic in quantum mechanics—including Birkhoff and von Neumann's (1936) proposal to adopt a non-distributive logic.[29] Carnap concludes this discussion with the open-minded and sensible assessment (1966/1974, p. 290): "Again, my feeling is that, if it were necessary to complicate logic in this way for the language of physics, it would be acceptable. At present, however, I cannot see the necessity of such a radical step. We must, of course, wait to see how things go in the future development of physics." In the years since Carnap made this assessment (originally in 1958) we have seen a striking increase in logically-oriented work on the mathematical foundations of quantum mechanics and the non-classical character of quantum probabilities by both mathematical physicists and philosophers of physics. In this sense, although no satisfying consensus has yet been attained, Carnap's hopes have indeed been realized. Perhaps a better appreciation of the fundamentally anti-metaphysical ambitions of Carnapian *Wissenschaftslogik* will contribute to even more progress in the future.

References

Birkhoff, G., & von Neumann, J. (1936). The logic of quantum mechanics. *Annals of Mathematics, 37*, 823–843.

Carnap, R. (1934). *Logische Syntax der Sprache* (Wien: Springer); translated as *The logical syntax of language*. London: Kegan Paul, 1937.

Carnap, R. (1936). Von der Erkenntnistheorie zur Wissenschaftslogik. In Actes du Congrès international de philosophie scientifique, fasc. 1: Philosophie scientifique et empirisme logique (pp. 36–41). Paris: Hermann.

[29] Carnap (1966/1974, pp. 289–290) is clear, in particular, that Birkhoff and von Neumann are proposing a change in "the laws of distribution in propositional logic," and, as such, their proposal is therefore quite different from Reichenbach's (1944) suggestion to a adopt a (three-valued) logic where the classical law of excluded middle fails. By contrast, in the course of his famous argument—ostensibly directed *against* Carnap—that "no statement is immune from revision," Quine (1951/1953, p. 43) focusses exclusively on a revision of the law of excluded middle; and Quine (1970, pp. 85–86) goes so far as to associate such a revision with Birkhoff and von Neumann. What this shows, from our present point of view, is that Quine appeals to possible revisions of logic in physics only as part of a very general epistemological argument carried out from a frankly philosophical (and even "metaphysical") perspective: Quine, unlike Carnap, has no serious interest in the ongoing practice of contemporary mathematical physics (and Quine thus has no interest at all in what *Carnap* means by *Wissenschaftslogik*).

Carnap, R. (1939). Foundations of logic and mathematics. In O. Neurath, R. Carnap, & C. Morris (Eds.), *International encyclopedia of unified science* (Vol. I, pp. 139–213). Chicago: University of Chicago Press.

Carnap, R. (1950). Empiricism, semantics, and ontology. *Revue Internationale de Philosophie, 11*, 20–40 (Reprinted in Carnap, 1956, pp. 205–229).

Carnap, R. (1956a). *Meaning and necessity* (2nd enlarged ed.). Chicago: University of Chicago Press

Carnap, R. (1956b). The methodological character of theoretical concepts. In H. Feigl & M. Scriven (Eds.), *Minnesota studies in the philosophy of science, vol. I: The foundations of science and the concepts of psychology and psychoanalysis* (pp. 38–76). Minneapolis: University of Minnesota Press.

Carnap, R. (1963). Replies and systematic expositions. In P. Schilpp (Ed.), *The philosophy of Rudolf Carnap* (pp. 859–1013). La Salle: Open Court.

Carnap, R. (1966). *Philosophical foundations of physics: An introduction to the philosophy of science*. New York: Basic Books.

Carnap, R. (1974). *An introduction to the philosophy of science*. New York: Basic Books.

Craig, W. (1953). On axiomatizability within a system. *Journal of Symbolic Logic, 18*, 30–32.

Craig, W. (1956). Replacement of auxiliary expressions. *Philosophical Review, 65*, 38–55.

Demopoulos, W. (2007). Carnap on the rational reconstruction of scientific theories. In M. Friedman & R. Creath (Eds.), *The Cambridge companion to Carnap* (pp. 248–272). Cambridge: Cambridge University Press.

Friedman, M. (2006). Carnap and Quine: Twentieth-century echoes of Kant and Hume. *Philosophical Topics, 34*, 35–58.

Friedman, M. (2007). Introduction: Carnap's revolution in philosophy. In M. Friedman & R. Creath (Eds.), *The Cambridge companion to Carnap* (pp. 1–18). Cambridge: Cambridge University Press.

Hempel, C. G. (1958). The theoretician's dilemma: A study in the logic of theory construction. In H. Feigl, M. Scriven, & G. Maxwell (Eds.), *Minnesota studies in the philosophy of science, vol. II: Concepts, theories and the mind-body problem* (pp. 37–98). Minneapolis: University of Minnesota Press.

Hempel, C. G. (1963). Implications of Carnap's work for the philosophy of science. In P. Schilpp (Ed.), *The philosophy of Rudolf Carnap* (pp. 685–709). La Salle: Open Court

Psillos, S. (1999). *Scientific realism: how science tracks truth*. London & New York: Routledge.

Psillos, S. (Ed.). (2000). Rudolf Carnap's theoretical concepts in science. *Studies in History and Philosophy of Science, 31*, 151–172.

Quine, W. V. (1948). On what there is. *Review of Metaphysics, 2*, 21–38 (Reprinted from *From a logical point of view*, pp. 1–19, by Quine, Ed.,1953).

Quine, W. V. (1951). Two dogmas of empiricism. *Philosophical Review, 60*, 20–43 (Reprinted from *From a logical point of view*, pp. 20–46, by Quine, Ed., 1953).

Quine, W. V. (1953). *From a logical point of view*. Cambridge, Mass.: Harvard University Press.

Quine, W. V. (1963). Carnap and logical truth. In P. Schilpp (Ed.), *The philosophy of Rudolf Carnap* (pp. 385–406). La Salle: Open Court.

Quine, W. V. (1970). *Philosophy of logic*. Englewood Cliffs, NJ: Prentice Hall.

Van Fraassen, B. C. (1980). *The scientific image*. Oxford: Clarendon Press.

Zabell, S. L. (2007). In M. Friedman & R. Creath (Eds.), *Carnap on probability and induction* (pp. 273–294).