

CARNAP AND KUHN ON LINGUISTIC FRAMEWORKS AND SCIENTIFIC REVOLUTIONS*

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Abstract: Several recent works in history and philosophy of science have re-evaluated the alleged opposition between the theses put forth by logical empiricists such as Carnap and the so-called “post-positivists”, such as Kuhn. Although the latter came to be viewed as having seriously challenged the logical positivist views of science, recent authors (e.g., Friedman, Reisch, Earman, Irzik and Grünberg) maintain that some of the most notable theses of the Kuhnian view of science have striking similarities with some aspects of Carnap’s philosophy. Against that reading, Oliveira and Psillos argue that within Carnap’s philosophy there is no place for the Kuhnian theses of incommensurability, holism, and theory-ladenness of observations. This paper presents each of those readings and argues that Carnap and Kuhn have non-opposing views on holism, incommensurability, the theory-ladenness of observations, and scientific revolutions. We note at the very end – without dwelling on the point, however – that they come apart on other matters, such as their views on metaphysics and on the context of discovery/justification distinction.

Keywords: Holism. Scientific revolutions. Incommensurability. Theory-ladenness of observations. Linguistic frameworks.

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Introduction

A good deal of Carnap's work was dedicated to the analysis of the structure of knowledge and science. However, from the 1960s onwards, several criticisms of his views brought about a rather widespread understanding that his efforts had essentially missed the mark. In particular, the idea of a logic of science began to be seen with growing suspicion. The works of Popper, Quine, and Kuhn were mostly received as having dealt a fatal blow to Carnap's philosophy.² But in the last couple of decades a number of authors have been reassessing the neopositivist legacy. One of the topics on the agenda is how much of Kuhn's analysis of science actually contradicts Carnap's views. New documents and arguments have come up that suggest that, contrary to the received view, some of the theses usually associated with the post-positivist philosophy of science – especially Kuhn's – were in some way already at work within Carnap's philosophy.

This has spun up a debate on the matter, and the outcome has been that the most recent readings of Carnap tend to split up into two camps: authors such as Friedman (2002), Reisch (1991), Earman (1993), and Irzik and Grünberg (1995) claim that some aspects of Kuhn's analysis of science can be found in Carnap's work, even in Carnap's early work. On this reading, Carnap's philosophy contains versions of the thesis of incommensurability, semantic and confirmational holism, the theory-ladenness of observations, and the claim that there are two kinds of theory revision in science – the revision of statements within a linguistic framework and the revision of the framework itself. On the other hand, Oliveira (2007, 2010) and Psillos (2008) maintain the more traditional view according to which Carnap and Kuhn have quite different views on the structure of scientific knowledge, and that the

² See, e.g., John Passmore (1967, p. 57): “Logical positivism (...) is dead, or as dead as a philosophical movement ever becomes”. See also Irzik (2003, p. 328), Reisch (1991, pp. 264-265), Earman, (1993, p. 9), and Stein (1992, p. 275). For discussion, and further references, see Richardson (2007).

theses usually associated with Kuhn's philosophy could not even have emerged within Carnap's philosophy.

In (1991) Reisch made public two letters that Carnap sent to Kuhn on the occasion of the publication of *The Structure of Scientific Revolutions* [henceforth: *Structure*] in the *International Encyclopedia of Unified Science* [henceforth: *Encyclopedia*], praising him and showing interest for the work.³ Reisch and others see in the content of those letters a genuine manifestation of intellectual kinship among the two. Along similar lines, Earman (1993, p. 1) claims that "it does seem to me that many of the themes of the so called post-positivist philosophy of science are extensions of ideas found in the writings of Carnap and other leading logical positivists and logical empiricists". Friedman (2002, p. 181) concurs saying that "in Kuhn's theory of the nature and character of scientific revolutions" we find "an informal counterpart of the relativized conception of constitutive a priori principles first developed by the logical empiricists". Lastly, Irzik and Grünberg (1995, p. 293) claim that "[w]ithout semantic holism semantic incommensurability would be groundless; without theory-ladenness it would be severely restricted to the theoretical terms" and that these theses coherently make up Carnap's philosophy after 1932. Given Kuhn's reputation for having views on science inconsistent with the ones held by the logical positivists, it is surprising, says Reisch, that having read the manuscript of what would later be seen as one of the buriers of logical positivism Carnap not only invites Kuhn to discuss problems of common interest but also regards the book as having shed

³ *Structure* was first published in 1962 as a monograph in the *Encyclopedia*, which had Carnap as one of its editors. On the interesting history of this publication, see Kuhn's interview available at <http://www.stevens.edu/csw/cgi-bin/shapers/kuhn/info/transcript.htm>

light on his own views.⁴ According to Carnap, *Structure* helped him to “see clearly” what he “had in mind” (see Reisch 1991, p. 267).

Oliveira (2007, p. 150), on the other hand, objects to the suggestions made by Reisch and Earman, pointing out that Carnap could not have read *Structure* as an attack to his own philosophy because he would not have considered it as a work in philosophy of science, but rather as a work on the history of science – which would agree with Reichenbach’s corresponding distinction between context of discovery and context of justification.⁵ Psillos (2008), in turn, challenges the alleged kinship of Carnap and Kuhn on specific topics, saying that there is no semantic holism, incommensurability, or theory-ladenness of observations in Carnap.

Section 1 below lays out in more detail some aspects of Carnap’s and Kuhn’s works that can be viewed as containing similarities, or at least as not being incompatible; section 2 brings out differences among them; and section 3 assesses the debates on the topic. We claim that Oliveira’s remarks are historically correct but do not defeat completely the approximations put forth by Friedman, Reisch, Earman, Irzik and Grünberg; and that although Psillos’s criticism of the authors just mentioned are questionable, Carnap and Kuhn do come apart on their views on topics not yet sufficiently discussed in the literature, such as their views on metaphysics and its role in science and their views on the context of discovery/justification distinction.⁶

⁴ Unfortunately Kuhn set aside Carnap’s invitations, perhaps taking them as mere editorial formalities (see Kuhn 1993, p. 314).

⁵ See Reichenbach (1938, pp. 6-7). For further reading, see Hoyningen-Huene (1987 and 2006).

⁶ But see Uebel (2011) on this topic.

1. Similarities

Coffa, Friedman, Earman, Reisch, Irzik and Grünberg are some of the authors that have over the last couple of decades reinterpreted logical positivism, bringing into focus the historical context in which it flourished. Although there are significant differences among those authors, they all say that some aspects of some theses usually identified with the post-positivist philosophy were already organically present in Carnap's mature work.

1.1 Semantic holism

Holism has various formulations and has been used in various ways. Nowadays one usually distinguishes two kinds of holism: confirmational (or epistemological) and semantic. Some authors – for example, Harrell (1996, p. 63) – argue that one kind entails the other. Holism of both kinds can be found in Carnap. In *The Logical Syntax of Language* Carnap explicitly endorses confirmational holism:

There is in the strict sense no refutation of an hypothesis; for even when it proves to be L-incompatible with certain protocol-sentences, there always exists the possibility of maintaining the hypothesis and renouncing acknowledgment of the protocol-sentences. Still less is there in the strict sense of a complete confirmation of an hypothesis. When an increasing number of L-consequences of the hypothesis agree with the already acknowledged protocol-sentences, then the hypothesis is increasingly confirmed; there is accordingly only a gradually increasing, but never a final, confirmation. Further, it is, in general, impossible to test even a single hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences; hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must also be used. Thus the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses (Duhem, Poincaré) (1934, p. 318).

In the paragraph following the one above, Carnap draws the expected consequences of confirmational holism:

No rule of the philosophical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so. This applies not only to the P-rules [i.e. those that convey physical principles] but also to the L-rules [i.e. those that convey analytic truths] including those of mathematics. In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. (1934, p. 318)⁷

Semantic holism, on the other hand, can be found in Carnap's later works, that discuss the structure of the language of science. Carnap classifies the terms of a language as logical, observational (O-terms), and theoretical (T-terms).⁸ The sentences of a language are, accordingly, also split up into three groups: (1) logical sentences, which contain no observational or theoretical terms, (2) observational sentences, which contain observational terms but no theoretical terms, and (3) theoretical sentences, which in turn are split up into (3a) mixed sentences, which contain observational and theoretical terms and (3b) pure theoretical sentences, which contain theoretical terms but no observational terms. The total language of science is then divided into an observational part (L_o) and a theoretical part (L_t). Although "the choice of an exact line is somewhat arbitrary" (2000, p. 158), from a practical point of view Carnap thinks that the distinction is clear enough to tell apart most of the terms that designate observable objects, properties and relations (dog, blue, larger than, etc.) from most of the terms that designate non-observable objects, properties and relations (electrons, spin, electromagnetic field, etc.).

The meanings of the theoretical terms cannot be completely explained through observations, he says, because they need to be

⁷ This is strikingly similar to Quine's holism; on this matter, see Tomida (1994).

⁸ See Carnap (2000, p. 158) and (1956, p. 38). The former is a talk delivered at the 1959 meeting of the American Philosophical Association, Pacific Division, and published by Psillos (2000).

interpreted by means of their observational consequences. A term such as ‘electric charge’, ‘electrons’, etc. cannot be interpreted ostensively or by observations: “we cannot simply point and thereby learn it” (2000, p. 161), because “in addition to observational consequences, the content is too rich; it contains much more than we can exhaust as an observational consequence” (2000, p. 159). Theoretical terms therefore do not admit a complete interpretation. This is a liberalization that most logical empiricists began to accept from 1939 onwards.⁹ The theoretical terms introduced by theoretical postulates are only said to be partially interpreted when related with correspondence rules so as to produce an observational consequence. This is, according to Irzik and Grünberg (1995, p. 290), their ‘factual function’. However, some theoretical terms do not have a correspondence rule of their own relating them to observations (1956, p. 42). Hence, theoretical terms not directly interpreted by correspondence rules are connected through meaning postulates to other theoretical terms, which in turn have correspondence rules of their own and thus grant to the former their meaning (1956, p. 47-48) – their ‘semantic function’, in Irzik and Grünberg’s (1995, p. 290) terminology. So the meanings of the theoretical terms remain dependent on correspondence rules and their connections – through theoretical postulates – to other theoretical terms. Consider, for example, two material bodies ‘a’ and ‘b’, and a correspondence rule connecting the theoretical term ‘mass’ to the observational predicate ‘heavier than’, such as this one: “if ‘a’ is heavier than ‘b’, then the mass of ‘a’ is larger than the mass of ‘b’” (see 1956b, p. 48). In this case the very meaning of ‘mass’ turns out to be

⁹ Prior to that the possibility of defining theoretical terms in observational terms was thought to be viable or at least plausible (see Carnap 1956, pp. 39 and 48). In fact, in (2000) Carnap claims that he was able to give, for the first time, the explicit definition of a theoretical term in the observational language; for that purpose, he uses an alternative form of describing a theory with the use of Ramsey-sentences and the Hilbert ϵ -operator.

dependent on the correspondence rules and postulates with which it was introduced into the theory.

It is oftentimes remarked that for logical empiricists, and Carnap in particular, the meanings of the observational terms are fixed through direct observation, and therefore the observational language would be neutral and invariable from one theory to another.¹⁰ If correct, this poses a challenge for those who want to extend Carnap's holism all the way down to observational sentences. The issue was intensely debated in the dispute about protocol sentences in the early Vienna Circle. Observability, for Carnap, is always carefully qualified. He says that it varies in different contexts, philosophers and scientists may use it for different purposes, and that there is no single correct usage – as in other matters the choice of one usage or another is practical (1966, p. 226). In (1966) Carnap dedicated a whole chapter (23) to pointing out variations in the use of the term. The same point is made in (1963a):

... “observability” is a rather vague term which may be understood in a narrower or wider sense. I gradually preferred to exclude from the observational language more and more scientific terms, even some of those which many physicists regard as observation terms because they refer to magnitudes for which there are simple procedures of measurement, e.g., “mass” and “temperature”. (pp. 78-79)¹¹

¹⁰ This sounds like an idealistic absolutism acknowledged by Carnap as present in some formulations of the old Positivism. In the formulations of the Logic of Science of some logical positivists one finds a “refined form of an absolutism of the ur-sentence (‘elementary sentence’, ‘atomic sentence’).” But for Carnap (1932a), as we will see below, this is not strictly speaking absolutism, since the choice of where to place the protocols is governed by the Principle of Tolerance (one of the first formulations of that principle dates back to 1932).

¹¹ See also: “it is true that it is hardly possible to draw a clear-cut boundary line between O-terms and T-terms. The choice of an exact line is somewhat arbitrary” (2000, p. 158).

To be sure, Carnap says that theoretical terms are *related* to observational properties (1963a, p. 78), but, as Irzik and Grünberg (p. 292) have remarked, this is a semantic rule that indicates only the reference of the observational terms, i.e., their extension. In *The Unity of Science* (1932a) Carnap did describe science as “a system of statements based on direct experience, and controlled by experimental verification” (1932a, p. 42); and he does assign to Mach the view that “the simplest statements in the protocol language are protocol statements, i.e., statements needing no justification and serving as foundation for all remaining statements of science” (p. 45). But he immediately afterwards says that this formulation is regarded as “inadequate for most present-day critics” (p. 46). However that may be, Carnap does claim that from no collection of protocol statements one can deduce a singular statement, but that the converse is true:

Statements of the system constituted by science (statements in the language of that system) are not, in the proper sense of the word, derived from protocol statements. Their relation to these is more complicated. In considering scientific statements, e.g. in physics, it is necessary to distinguish in the first place between “singular” statements (referring to events at a definite place and time, e.g. “the temperature was so much at such and such a place and time”) and the so-called “laws of nature,” i.e. general statements from which singular statements or combinations of such can be derived (e.g. “the density of iron is 7.4 [always and everywhere]”). In relation to singular statements a “law” has the character of an hypothesis; i.e. cannot be directly deduced from any finite set of singular statements but is, in favourable cases, increasingly supported by such statements. A singular statement (expressed in the vocabulary of the scientific system) has again the character of an hypothesis in relation to other singular statements and in general the same character in its relation to protocol statements. From no collection of protocol statements, however many, can it be deduced, but is in the most favourable case continually supported by them. In fact deduction is possible but in the converse direction. For protocol statements can be deduced by applying the rules of inference to sufficiently extensive sets of singular statements (in the language of the scientific system) taken in conjunction with laws of nature. Now the verification of singular statements consists of performing such deductions in order to discover whether the protocol statements so

obtained do actually occur in the protocol. Scientific statements are not, in the strict sense, “verified” by this process. In establishing the scientific system there is therefore an element of convention, i.e. the form of the system is never completely settled by experience and is always partially determined by conventions. (1932a, p. 49)

This suggests that there is a sense in which protocol statements depend on more than mere experience, and this could be understood as a form of theory-ladenness of observations. If a protocol statement can be deduced by “applying the rules of inference to sufficiently extensive sets of singular statements (...) taken in conjunction with laws of nature” it follows that the theory may contribute to the content of at least some of those statements. Note that the passage quoted above is from *Unity of Science* (1932a), which is usually read as a book where Carnap allegedly defends foundationalism or a neutral observational language. The suggestion just mentioned remains true even when we take into account the subsequent changes introduced in “Testability and Meaning”, where verifiability is replaced by the weaker notion of confirmability, which is always a matter of probability. Because confirmation is probabilistic, there is always the possibility of revising any given singular statement, and that revision, in turn, can result from theoretical considerations.

In (1932b) Carnap remarked that protocol sentences may be viewed as lying either outside or inside the language of science, but he added that “this is a question, not of two mutually inconsistent views, but rather of two different methods for structuring the language of science both of which are possible and legitimate” (1932b, p. 457) and that

The questions of whether the protocol sentences occur outside or inside the system language and of their exact characterization are, it seems to me, not answered by assertions but rather by postulations. Although earlier (*Erkenntnis*, II, p. 438) I left this question open and indicated only a few possible answers, I now think that the different answers do not contradict each other. They are to be understood as suggestions for postulates; the task consists in investigating the

consequences of these various possible postulations and in testing their practical utility. (1932b, p. 458)

Viewing protocols statements as lying outside the system of science suggests that they are not be theory-laden, but this is only one of the possible ways of reconstructing the system of science. In his reply to Neurath (1932b), Carnap chooses to construe them as belonging to the language of science. He argues that doing so avoids the need for special rules of translation and any threats of misguided absolutism (1932b, pp. 469-470). (We shall return to this issue below, in the section on incommensurability and in the one on the theory-ladenness of observations.) What we can say already is that Carnap's mature philosophy does contain a statement of semantic holism in the sense that some changes in the theoretical postulates of a theory may bring about shifts in the meanings of theoretical terms.¹² When those changes happen in the most central terms of a theory, the ensuing shifts in the correspondence rules may produce changes in the whole structure of a theory, since it will then need to adapt to those modifications (see Carnap 1956 and 1959). Note that, and this is important for later discussion with Psillos, this is exactly the kind of semantic holism that Irzik and Grünberg attribute to Carnap: "By semantic holism we mean the doctrine that the theoretical postulates of a theory contribute to the meaning of theoretical terms occurring in them and that a change in the theoretical postulates results in a change in meaning. Contrary to Earman, we claim that Carnap is a semantic holist in this sense" (1995, p. 289).

An anonymous referee for this journal has pointed out that "according to Kuhn during the life of the paradigm (normal science) some laws may change without changing the meaning (...) of the

¹² As an anonymous referee has pointed out, however, this is not as strong as the holism we find in some passages of *Structure*.

theoretical terms”.¹³ But this is also the case in Carnap: revision of an indeterminate statement of a theory does not necessarily entail changes in meaning; changes in some laws may only refine the meanings of the terms it comprises without essentially changing them. Specific changes that produce radical alterations within a language, sometimes viewed as a revolution, are interpreted by Carnap as changes of one language to another. See, for example, Carnap’s reply to Quine in Carnap (1963b and section 1.4 below), where he fully agrees with most of Quine’s holism:

Quine shows (in his book, pp. 42-46) that a scientist, who discovers a conflict between his observations and his theory and who is therefore compelled to make a readjustment somewhere in the total system of science, has much latitude with respect to the place where a change is to be made. In this procedure, no statement is immune to revision, not even the statements of logic and of mathematics. There are only practical differences, and these are differences in degree, inasmuch as a scientist is usually less willing to abandon a previously accepted general empirical law than a single observation sentence, and still less willing to abandon a law of logic or of mathematics. *With all this I am entirely in agreement.* (1963b, p. 921; italics added)

¹³ Note that this is exactly the opposite of what Psillos says: “It does not follow (as it does on Kuhn’s holistic theory of meaning) that *any*, even the slightest, change in the theoretical web will result in meaning-change” (2008, p. 138) – this is clearly a misreading of Kuhn, however. In *Structure*, chapter III, Kuhn explicitly says that theoretical changes occur during a period of normal science. Some of these changes may in fact yield changes in the meaning of the terms used, but it is implausible to think that any theoretical change whatsoever will yield a change in meaning. Some theoretical changes aim at making more precise the predictions of a theory or articulating various portions of a theory – and most of these changes do not entail changes in meaning. In (1982, p. 670-671) Kuhn is much more explicit in this: “Most of the terms common to the two theories function the same way in both; their meanings, whatever those may be, are preserved; their translation is simply homophonic. Only for a small subgroup of (usually interdefined) terms and for sentences containing them do problems of translatability arise. The claim that two theories are incommensurable is more modest than many of its critics have supposed”.

Carnap then goes on to distinguish two different kinds of modifications made when a conflict with experience occurs. A modification may be performed on the truth-values of an indeterminate sentence (i.e. a sentence that does not have its truth-value determined by the rules of the language) – these modifications do not change the meanings of the terms, only refine them – or it may occur in the language itself. In the latter case, “the structures of the two languages differ in essential points” (1936, p. 126). This, according to Carnap, “constitutes a radical alteration, sometimes a revolution” (1963b, p. 921).

Now if Carnap does accept semantic holism, as we have indicated, then we can immediately foresee in Carnap’s work another similar thesis often thought to have emerged only with the so-called post-positivists, namely, the thesis of incommensurability.

1.2 Incommensurability

In the “post-positivist” literature this thesis was put forth independently by both Kuhn and Feyerabend in 1962.¹⁴ In a later paper Kuhn says that he introduced the notion of incommensurability by analogy to mathematical incommensurability:

The hypotenuse of an isosceles right triangle is incommensurable with its side or the circumference of a circle with its radius in the sense that there is no unit of length contained without residue an integral number of times in each member of the pair. There is thus no common measure. (1983, p. 35)

In Kuhn’s work, the notion of incommensurability was used in support of the claim that there is no neutral language that can adequately convey the meanings of at least some of the more fundamental terms of the theories belonging to different paradigms

¹⁴ See *Structure*, chapter X, and Feyerabend (1962).

(see Kuhn, 1983, p. 36). In his later works, incommensurability is explicitly characterized as partial untranslatability of some central or fundamental terms or phrases of a theory. In *Structure*, Kuhn illustrates this thesis variously. At one point, he argues that the laws of Newton's dynamics cannot be understood as a special case of the laws of general relativity. If it could, then there would be a way of translating Newton's laws into the language of general relativity. One could imagine that translation being carried out roughly as follows:

Imagine a set of statements E_1, E_2, \dots, E_n , which together embody the laws of relativity theory. These statements contain variables and parameters representing spatial position, time, rest mass, etc. From them, together with the apparatus of logic and mathematics, is deducible a whole set of further statements including some that can be checked by observation. To prove the adequacy of Newtonian dynamics as a special case, we must add to the E_i 's additional statements, like $(v/c)^2 \ll 1$, restricting the range of the parameters and variables. This enlarged set of statements is then manipulated to yield a new set, N_1, N_2, \dots, N_m , which is identical in form with Newton's laws of motion, the law of gravity, and so on. Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions (*Structure*, pp. 101-2)

This derivation, Kuhn argues, is "spurious". The referents of the terms used in general relativity and the ones from which the Newtonian terms were derived are not the same. Newton's 'mass' equals 'force times acceleration', whereas 'mass' in Einstein is defined as equivalent to energy divided by the square of the speed of light. Newton's mass obeys the law of conservation and thus differs from Einstein's, which does not. Strictly, we cannot speak here of a derivation (see *Structure*, p. 102).

In "Truth and Confirmation" Carnap defends a thesis which at least resembles Kuhn's incommensurability, and uses a similar example:

In translating one language into another the factual content of an empirical statement *cannot always* be preserved unchanged. *Such changes are inevitable if the structures of the two languages differ in essential points.* For

example: while many statements of modern physics are completely translatable into statements of classical physics, this is not so or only incompletely so with other statements. The latter situation arises when the statement in question contains concepts (like, e.g., ‘wave-function’ or ‘quantization’) which simply do not occur in classical physics; the essential point being that these concepts cannot be subsequently included since they presuppose a different form of language. This becomes still more obvious if we contemplate the possibility of a language with a discontinuous spatio-temporal order which might be adopted in a future physics. Then, obviously, some statements of classical physics could not be translated into the new language, and others only incompletely. (This means not only that previously accepted statements would have to be rejected; but also that to certain statements – regardless of whether they were held true or false – there is no corresponding statement at all in the new language). (1936, p. 126; italics added)

“Needless to say”, Irzik and Grünberg comment (1995, p. 291), “this was exactly Kuhn’s point in his *Structure*”. In particular, it is striking that Carnap formulates the relation between different linguistic frameworks the same way Kuhn describes the relation between theories belonging to different paradigms: partial untranslatability. Earman draws from this same passage by Carnap even stronger conclusions: “Here we have two of the key theses of the ‘post-positivist’ philosophy of science: the nonexistence of neutral facts and incommensurability in the form of a failure of intertranslatability” (1993, p. 11).

We can now begin to see how incommensurability issues from semantic holism in Carnap: given that the (partial) interpretation of theoretical terms depends on theoretical postulates and correspondence rules, a shift in the latter may change the meanings of the former. Hence, the translation of one theory into the language of another might in some cases turn out to be impossible. If the two theories are couched in different linguistic frameworks, a full translation of the terms of one theory into the language of another might not be possible without residue. Carnap explicitly acknowledges this kind of possibility in (1936, p. 126, quoted above). This is not unlike what Kuhn says in

Structure, and this is very similar to what he later calls “local incommensurability” (see Kuhn 1983, p. 35).

Carnap’s notion of semantic incommensurability would be narrow, however, if it did not also apply to observational terms. It would be narrower and thus differ from Kuhn’s incommensurability thesis, which also includes observational terms.¹⁵ Irzik and Grünberg – following Friedman and Earman – state that this form of naïve empiricism often attributed to Carnap is more Humean than properly Carnapian. Carnap would never have come to the point of defending a complete interpretation of observational terms by observations themselves. What he does is to assume for methodological purposes that observational terms are to be understood non-problematically by all the speakers of a language, or linguistic community:

Let us *imagine* that L_o is used by a certain language community as a means of communication, and that all sentences of L_o are understood by all members of the group in the same sense. *Thus* a complete interpretation of L_o is given. (1956, p. 40; italics added)

Or:

... we *presuppose* that we are in the possession of a complete interpretation of the terms. That need not be done in an explicit way by semantical rules. You just ask somebody: ‘Is this part of the English language completely understood by you, do you know what you mean by the words which you use there?’ (2000, p. 160; italics added)

The full scope of incommensurability (which includes also observational terms) is rendered clearer in Carnap’s works on related topics, as we shall now see.

¹⁵ Nevertheless, it seems uncontroversial that Carnap accepts the incommensurability of at least some theoretical terms. So at least a weak version of the thesis of incommensurability is to be found in Carnap.

1.3 The theory-ladenness of observations

As mentioned above, Carnap never seems to have held that the meanings of the observational terms of a theory are *completely* determined by observations. Portions of their meanings come from theoretical sentences and correspondence rules. But what Carnap does assume is that observational terms are generally understood in the same way by the speakers of a language: fluent speakers understand the meanings of observational terms uniformly. Part of Carnap's apparently relaxed attitude towards observational language is motivated by his view that observational terms gave rise to very few significant philosophical issues, which were not the subject matter of serious disputes at the time (see Carnap 1956, p. 38).

Carnap's proposal that several linguistic frameworks are possible in science, in conjunction with his view that protocol statements can be deduced from a large set of singular statements and laws of nature – mentioned above – indicates that also the observational terms may gain their interpretation from theoretical postulates and correspondence rules. Already in the *Aufbau* (1928, chap. C; p. 51) Carnap leaves open the possibility of several possible basis for the analysis of knowledge. One possible basis is physicalistic, another one might be phenomenistic, and so forth; the choice between them rests upon a “methodological question of choosing the most suitable basis for the system” (1963a, p. 18). Carnap's later preference towards the physicalistic language is due to its intersubjective advantages (1963a, p. 19). The possibility of choosing various bases for constructing the total language of science indicates that an observational term can be interpreted in several ways; we can, for example, both describe colors through dispositions to behavior due to stimuli so and so, and by saying that light of a certain wave-length was captured by the eye, and so forth.

Oberdan (1990), while pointing out mistakes in Feyerabend's reading of Carnap, also maintains that Carnap accepts the theory-

ladenness of observations and that this is manifest in the development of his views on protocol sentences. Carnap claims that protocols sentences are to be considered as reporting facts, which either are to be translated into the language of science or are already part of that language.¹⁶ Carnap's argument in *The Unity of Science* (1932a) has the form of a *reductio*: if protocol sentences are not understood as correlates of physical facts, then they could not be used as evidence for the truth or falsehood of physical statements; and in that case physics as a whole would remain disconnected from our experiences. This would be rather disconcerting, since protocols sentences convey the empirical evidence on which all our scientific knowledge turns. Given that the initial assumption leads to contradictions, it follows that protocol sentences have physical facts as their correlates.

We have on the one side the content of experience, sensations, perceptions, feelings, etc., and on the other side constellations of electrons, protons, electro-magnetic fields, etc.; that is, two completely disconnected realms in this case [...]. Nevertheless an inferential connection between the protocol statements and the singular physical statements must exist for if, from the physical statements, nothing can be deduced as to the truth or falsity of the protocol statements there would be no connection between scientific knowledge and experience. (Carnap, 1932a, pp. 80-81)

Furthermore, if two statements are inferentially connected (if the truth-value of one is not logically independent of the truth-value of another), then they must have some common content. In this sense a protocol sentence that expresses an immediate experience can be translated into the language of physics, if it is not already formulated in that language. In either case protocol sentences are to be understood in light of our current theory.¹⁷

¹⁶ See Carnap's debate with Neurath and Schlick on this matter in: Carnap (1932a, 1932b), Neurath (1932), and Schlick (1934).

¹⁷ For discussion, see Oberdan 1990, p. 27.

Hence, the theory-ladenness of observations can affect also the most basic protocol sentences. This is not to say, however, that the kind of theory-ladenness of observations that we find in Carnap is the same as the one we find in Kuhn's. Kuhn's reasoning for the theory-ladenness of observations seems to rely not so much on the theoretical penetrability of explicitly stated rules and laws but mostly on the role that scientific education has on an individual's perception (see, for example, *Structure*, chapter V, and also what he says on "exemplars" in the Postscript). Nothing like the theory-ladenness of observation as stated in the theories of cognitive penetrability of perceptual experiences can be found textually in Carnap as it can in Kuhn (see *Structure*, chapter X). Our point here is not that their views are identical, but merely that their views are not incompatible and therefore Kuhn's views on these matters cannot be construed as objections to Carnap's. Perhaps they can even be seen as complementary. In the matter of theory-ladenness of observation, for example, Carnap was aware of the developments in Gestalt psychology already in *Aufbau* (1928, §67). But for Carnap the logic of science has the task of analyzing science as an ordered systems of the statements asserted by scientists, i.e., abstracting from the psychological and sociological conditions of those assertions – so the beliefs, images, and behavior induced by those statements are regarded as belonging to the study of psychology, sociology, and methodology of science (see Carnap, 1938, p. 393). So even though Carnap saw these studies as not belonging to philosophy (or "the logic of science") he did see them as complementing his own work.¹⁸ Kuhn's more psychological and methodological (or practical) formulation of

¹⁸ "These investigations of scientific activity may be called history, psychology, sociology, and methodology of science. The subject matter of such studies is science as a body of actions carried out by certain persons under certain circumstances. Theory of science in this sense will be dealt with at various other places in this *Encyclopedia*; it is certainly an essential part of the foundation of science." (Carnap, 1938, p. 393)

the thesis of the theory-ladenness of observations can thus be viewed as complementing Carnap's more linguistic formulation.

1.4 Scientific revolutions

Carnap's clearest reference to revolutions in science is in his reply to Quine in the Schilpp volume on his own philosophy (Carnap, 1963b), but there are also other passages that reveal the same (see Carnap, 1956, p. 51). Defending his concept of analyticity against Quine, who interprets it as implying that analytic sentences can be "held true come what may" (Quine 1951, p. 40), Carnap distinguishes two kinds of revision that may take place whenever there is a conflict between theories and observations: one kind is that of a change of language, another is that of an addition or modification of the truth-values of a given empirical statement.

A change of the first kind [change of language] constitutes a radical alteration, sometimes a revolution, and it occurs only at certain historically decisive points in the development of science. On the other hand, changes of the second kind occur every minute. A change of the first kind constitutes, strictly speaking, a transition from a language L_n to a new language L_{n+1} (Carnap, 1963b, p. 921)

So for Carnap a change of language – using Carnap's own words, a change in the linguistic framework – is sometimes a scientific revolution. But here it is necessary to make a distinction that is not clear in Carnap's works, between wide and narrow linguistic frameworks. In the *Unity of Science* (1932a) and *Logical Syntax of Language* (1934) Carnap suggests that the physicalist language is the only known universal language capable of unifying science. This however does not exclude the possibility of other languages that are not physicalist. The choice between a physicalist language and, say, phenomenalist language is a choice between wide linguistic frameworks. Scientific revolutions – the Copernican revolution, for example – are not changes of these

kinds, but rather changes within the physicalist framework; hence changes in the narrow linguistic frameworks (see Brendel, 2006). Furthermore, Carnap distinguishes the kind of criteria that govern changes within a linguistic framework from those that govern changes of the framework itself. In (1950), for example, Carnap insists that the former are internal to a framework and thus may yield yes-no answers to a given theoretical question, whereas the latter are external and pragmatic. These two features – change in the language and in the criteria for deciding between rival hypotheses – are roughly how Kuhn describes scientific revolutions, especially in his later works, when the notion of a ‘paradigm’ used in *Structure* is replaced by that of a ‘structured lexicon’.¹⁹ From the *Structure* onwards, Kuhn seems to have toned down his notion of incommensurability. Initially incommensurability was presented as a complex notion that was used to describe paradigms that have different theoretical, methodological, and metaphysical commitments, as well as different views of science, standards of evidence and scientific values. Over the years it came to be seen as a more restricted phenomena (dubbed ‘local incommensurability’), described in terms of lack of translatability of clusters of concepts belonging to different theories.²⁰ In (1983, p. 36), Kuhn says that: “the claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss”.

Likewise for Carnap: the addition of new theoretical terms and new correspondence rules for them does not happen all the time, but only in decisive moments in the development of the sciences. The most common activity of the scientific community is the “mere change in or addition of, truth-value”, which are “changes of second kind” that

¹⁹ See Carnap (1963, p. 921). Friedman makes the same comparison in his (2002, p. 181). See also Kuhn (1989, pp. 76-77).

²⁰ See Kuhn (1982, and 1989, p. 60 footnote, and p. 74).

“occur every minute” (Carnap 1963b, p. 921). These can also be viewed as the activities that Kuhn calls ‘normal science’, which is exactly what Reisch (1991) says, pointing out similarities between Kuhn’s normal science as puzzle-solving and Carnap’s activities within a scientific framework as additions or modifications of truth-values of indeterminate propositions. Reisch conveys this analogy and his general attitude towards Carnap and Kuhn in these terms:

If Kuhn debunked certain tenets of logical empiricism (namely, a theory/observation distinction and paradigm-independent criteria of theory goodness) partly by suggesting that they were impotent to capture the reasoning involved in episodes of revolutionary scientific change, the fact remains that these tenets do not ground Carnap’s view of revolutionary scientific reasoning. In choices between radically different theories, different conceptual frameworks, or (in his preferred philosophical idiom) different languages, he offers an account that is in fact distinctly analogous to that of Kuhn. (1991, p. 265)

It is true that scientific revolutions are not described by Kuhn as mere changes in linguistic frameworks, or in the language of a particular scientific discipline. There are also passages in Kuhn’s *Structure* which suggest that there can be changes in paradigms without changes in the language (“explicit rules”) shared by a scientific community.²¹ But given the way Kuhn construes paradigm changes it is not clear that in these cases (in which explicit rules shared by a community remain the same throughout a paradigm change) there are no changes in language. The way the members of a professional

²¹ “Explicit rules, when they exist, are usually common to a very broad scientific group, but paradigms need not be. The practitioners of widely separated fields, say astronomy and taxonomic botany, are educated by exposure to quite different achievements described in very different books. And even men who, being in the same or in closely related fields, begin by studying many of the same books and the achievements may acquire rather different paradigms in the course of professional specialization.” (*Structure*, p. 49)

subgroup use the relevant words changes and also their understanding of what those words mean.²²

This summarizes what the most recent literature on Carnap and Kuhn has to offer for the kinship between the Carnap and Kuhn's philosophies. On these readings Carnap appears as a precursor of several theses that became prominent only later, in the so-called 'post-positivist' literature. It is not clear, however, that Carnap did in fact assign to them the philosophical significance that they later acquired.

2. Differences

Against these attempts to draw Carnap and Kuhn closer together stand Oliveira (1998, 2002, 2004, 2007) and Psillos (2008). The argumentative strategies used by Oliveira vary, but they tend to challenge the historiography of the re-evaluation of Carnap's work more than the actual theses held by Carnap and Kuhn. In (1998) he speaks of a "revisionist" reading of Carnap's work, and distinguishes radical and moderate revisionists (see also Oliveira (2002)). The more moderate accept that the development of Carnap's work can be split up into a first, more "dogmatic" phase, in which he was a foundationalist, and a second, more liberalized phase, after *Aufbau*, also called by him as a "post-positivist" phase. Earman and Reisch are among the moderate revisionists; among the radicals, who deny that Carnap's work is split up into two phases and claim that he was never a foundationalist, are Friedman and Uebel.

²² "An investigator who hoped to learn something about what scientists took the atomic theory to be asked a distinguished physicist and an eminent chemist whether a single atom of helium was or was not a molecule. Both answered without hesitation, but their answers were not the same. For the chemist the atom of helium was a molecule because it behaved like one with respect to the kinetic theory of gases. For the physicist, on the other hand, the helium atom was not a molecule because it displayed no molecular spectrum. (...) Undoubtedly their experiences had much in common, but they did not, in this case, tell the two specialists the same thing." (*Structure*, p. 50-51)

Oliveira (2007, p. 155) claims that Carnap did not read Kuhn's work as a work in the philosophy of science, thus reasserting Reichenbach's (1938) distinction between contexts of discovery and contexts of justification. He argues that this is corroborated by the fact that although Carnap became acquainted with *Structure* and with Kuhn's work in general, he never quoted him as someone who shared his views on science, even though he did write a book specifically on philosophy of science (1966). Oliveira holds that this would explain Carnap's accepting the publication of *Structure* in the *Encyclopedia*, since it had been planned by the editors – Carnap among them – to include also works in “history, psychology, sociology, and methodology of science” (2007, p. 151). According to Oliveira (2007, p. 150), this renders implausible the “revisionist” interpretation which says that Carnap would have taken Kuhn's *Structure* as congenial to his own work.

On the famous 1962 letters that became public with Reisch (1991) – where Carnap praises Kuhn's work saying that it is “very illuminating” – Oliveira (2007) argues that they provide little if any evidence of a meeting of minds. He claims that the “revisionists” were too hasty in drawing that conclusion, given that other, more plausible motives can explain the same facts: Carnap may have thought that Kuhn's work was illuminating in that he made significant contributions to the history, sociology, and psychology of science.

On the radical revisionists, Oliveira offers two main criticisms (1998, pp. 7-22): “neglect of contrary evidence” (1998, p. 12) to the claim that Carnap was never a foundationalist and excessive ingenuity in their explanation about the “origin and persistence of the misreading of which logical positivism would have suffered since its beginning” (1998, p. 14). Evidence of Carnap's foundationalism can be found in his “Intellectual Autobiography” (see Carnap 1963a, pp. 50-57), where Carnap openly admits his commitment to foundationalism in the *Aufbau* period. When the revisionists mention those passages (e.g., Friedman 1999, p. 4), they construe them as if Carnap were

misinterpreting the historical development of his own thought – and this, according to Oliveira, is too much of an *ad hoc* explanation.

On the alleged persistent misreading of logical positivism disseminated initially by Ayer and Quine, in which it is portrayed as a continuation of classical British empiricism, Oliveira says that the radical revisionists use “bold” methods to prove their point and turn away from “completely unfavorable evidence” such as the mention of Hume and Mill in the Vienna Circle manifesto (1929), and Carnap’s permission to include papers of his own in *Logical Positivism* (1959), edited by Ayer. If Carnap had disagreed with Ayer’s way of presenting logical positivism, he would most likely have made his reservations clear at the time or later on; but he did not (cf. 1998, p. 14).

On the passage of “Truth and Confirmation” (pp. 125-126) quoted above, Oliveira (2002 and 2004) disagrees with the revisionist reading based on Carnap’s response to Cohen on “Truth and Confirmation” in the Schilpp volume and “Testability and Meaning”. There, Cohen describes Carnap as a conventionalist, and Carnap replies that no logical empiricist ever defended a pure kind of conventionalism; and adds:

Cohen believes that my principle of tolerance in the logical syntax contains a ‘doctrine of conventionally-chosen basic-truths’. But this is not the case. The principle referred only to the free choice of the structure of the language, and not to the content of synthetic sentences. I emphasized the non-conventional, objective component in the knowledge of facts, e.g. in (1936-5). There I also pointed out that the first operation in the testing of synthetic statements is the confrontation of the statement with observed facts. Thereby I took a position clearly opposed to a pure conventionalism and to any coherence theory of truth (...). At any rate, there cannot be any doubt that Neurath never held this conception. Still less can it be attributed to me or to “the physicalists” in general, as critics have sometimes done. (1963b, p. 864)

Oliveira then asks whether it would “make sense for Carnap to draw attention to this passage in order to emphasize ‘the non-

conventional, objective component in the knowledge of facts' and the fact that his principle of tolerance referred 'only to the free choice of the structure of the language, and not to the content of synthetic sentences' if this text should be interpreted as suggested by revisionists?" (Oliveira 2002, p. 5). Regarding "Testability and Meaning", Oliveira (2002, p. 4) highlights passages where Carnap mentions conventional components that play a role in deciding the truth of synthetic sentences only to immediately discredit them as being of little significance on many occasions.

The passage (Carnap 1936, pp. 125-126) quoted by Irzik and Grünberg, Coffa, and Earman, if read with revisionist eyes might raise the suspicion of a Carnapian thesis of incommensurability due to failure at intertranslatability and lack of a neutral language. The passage is very brief – Carnap is calling attention to the use of the word 'comparison' (as in 'comparison of propositions with facts'), and saying that he considers more adequate the word 'confrontation'. Carnap makes several qualifications throughout; he says, for example, that "the answer to a question concerning reality (...) depends not only upon that 'reality' or upon the facts but also upon the structure of the language", and that "in translating one language into another the factual content of an empirical statement cannot always be preserved unchanged", and "while many statements of modern physics are completely translatable into statements of classical physics, this is not so or only incompletely so with other statements" (Carnap 1936, p. 126). These qualifications suggest a more prudent reading of the passage. Is it really the case that Carnap is asserting a thesis of incommensurability and the lack of a neutral language?

Oliveira goes on to other passages by Carnap in which he says that there are no substantial theoretical differences among scientists – say, among two geographers or two zoologists – that cannot be resolved empirically through observations (Carnap 1928, pp. 333-334;

1935, pp. 19-20; and 1932, pp. 64-65). These passages suggest a reading of Carnap that differs from the one offered by the revisionists.

2.1 A reply to Oliveira

Regarding the question of Carnap's conventionalism, Brendel (2006) has pointed out that Carnap's linguistic frameworks can be wide or narrow. In the *Logical Syntax of Language* (1934), for example, Carnap dwells on "the question of range of the definitions" (Brendel 2006, p. 4):

[W]hether in the construction of a language S we formulate only L-rules or include also P-rules, and, if so, to what extent, is not a logico-philosophical problem, but a matter of convention and hence, at most, a question of expedience. If P-rules are stated, we may frequently be placed in the position of having to alter the language; and if we go so far as to adopt all acknowledged sentences as valid, then we must be continuously expanding it. (Carnap, 1934, p. 180)

Commenting on this paragraph, Brendel (2006, p. 4) says that it indicates that linguistic frameworks can be extended or shrunk down: "if the range of the definition is wide, we get a wider framework, a wider language, which means that only bigger (theory) changes are to be regarded as language changes"; on the other hand, if the scope is narrow, "smaller or fewer changes cause language changes". What matters here – regardless of Brendel's conclusion, which we omit here – is that the scope of a linguistic framework may vary. At the beginning of Part I of *Logical Syntax of Language* (1934, p. 11), Carnap explains the method of extending a language and the purposes of doing so. He then constructs Language II, which is wider in scope than Language I; indeed, Language I is then a subset of II when the latter also includes the P-rules. At other occasions Carnap explicitly states the possibility of constructing linguistic frameworks of wider and narrower scope. Sometimes he speaks of "extending" a linguistic framework, for example (see, e.g., Carnap 2000, pp. 158-159).

This possibility (of wider and narrower frameworks) opens up alternative interpretations of some passages by Carnap. Brendel (2006) has shown that on some occasions Friedman (1993, 1998, 1999) and Irzik (2003) have misinterpreted Carnap, but I will focus here on mistakes made by Oliveira. We noted that in the passage of “Truth and Confirmation” quoted above Carnap makes several qualifications, which suggest a more prudent reading of the text. But now, aided by the distinction between wider and narrower linguistic frameworks, we can return to that passage. Oliveira calls attention to Carnap’s reply to Cohen in the Schilpp collection, in which Carnap refers to “Truth and Confirmation” so as to emphasize “the non-conventional, objective component in the knowledge of facts”, thus rejecting the accusation of having a “doctrine of conventionally-chosen basic-truths”. But Carnap also says that “the principle referred only to the free choice of the structure of the language, and not the content of synthetic sentences” (1963b, p. 864). For Oliveira, this is perhaps a refutation of the revisionist interpretation of Carnap, because on the revisionist reading this passage would indicate Carnap’s conventionalism, which is exactly the reading Carnap rejects in his reply to Cohen.

It seems clear that for Carnap science is impossible without some confronting of empirical statements with facts. In *The Unity of Science* he advances this idea, already quoted above:

[A]n inferential connection between the protocol statements and the singular physical statements must exist for if, from the physical statements, nothing can be deduced as to the truth or falsity of the protocol statements there would be no connection between scientific knowledge and experience. (1932, p. 81)

In “Truth and confirmation” Carnap points out that the word ‘comparison’ is inadequate and may bring about confusion: we might assume, for example, that we are in search of an “absolute reality” whose nature is fixed and independent of the language used to describe it. This is not the case, however: The “answer to a question concerning

reality (...) depends not only upon that ‘reality’”, but also on it insofar as we confront our descriptions of it with facts. “Thereby I took a position clearly opposed to a pure conventionalism and to any coherence theory of truth”, Carnap replies to Cohen.

Carnap’s position is ‘dual’, so to speak. On the one hand the confrontation of statements with facts settles the truth-values we assign to empirical sentences; on the other, choosing between linguistic frameworks is a practical matter “decided by their efficiency as instruments” (1988, p. 221). When that decision does not involve empirical elements – e.g., when choosing between wide linguistic frameworks – the principle of tolerance governs the choice; when it involves empirical elements, it is based on non-conventional, objective criteria, guided by the overarching linguistic framework that encompasses it. To be sure, this is not to say that Carnap’s views are the same as Kuhn’s. As we mentioned above (section 1.3) the way that a paradigm ladens theoretically the sentences of a theory involves non-linguistic elements. Our point in this paper here is merely that Kuhn’s views are not incompatible with Carnap’s on this matter, and can be taken to be complementary. Oliveira highlights passages of Carnap’s works that suggest differences between Carnap and Kuhn, and that the “revisionist” reading of Carnap is wrong. Our claim here is that those same passages can be interpreted otherwise.

Having that in mind, we can now read at least two of the citations offered by Oliveira (2002) and interpret them uniformly. This is the full passage from “Testability and Meaning”:

Suppose a sentence S is given, some test-observations for it have been made, and S is confirmed by them in a certain degree. Then it is a matter of practical decision whether we will consider that degree as high enough for our acceptance of S, or as low enough for our rejection of S, or as intermediate between these so that we neither accept nor reject S until further evidence will be available. Although our decision is based upon the observations made so far, nevertheless it is not uniquely determined by them. There is no general rule to determine our decision. Thus the acceptance and the rejection of a

(synthetic) sentence always contains a conventional component. That does not mean that the decision – or, in other words, the question of truth and verification – is conventional. For, in addition to the conventional component there is always the non-conventional component – we may call it, the objective one – consisting in the observations which have been made. And it must certainly be admitted that in very many cases this objective component is present to such an overwhelming extent that the conventional component practically vanishes (Carnap 1953, p. 49).

Here Carnap highlights a non-conventional component and rejects it as the sole determining factor in deciding a synthetic sentence, i.e., when deciding whether to accept a synthetic sentence that was tested by observations, the number of confirmations by observations – which itself involves a practical question as to how many should count as enough – does not determine alone its acceptance. There is still here a conventional component, but this alone does not entail that the decision is purely a matter of convention; often when the observations are evident the conventional component is less influential. But if the observational tests are found lacking, conventional factors kick in. This agrees with Carnap's confirmational holism mentioned above (1934, p. 318).

Our 'dual' interpretation of Carnap also explains his reply to Cohen where he speaks of a "complete conventionalism" (1963b, p. 864). Only a partial conventionalism can be attributed to Carnap, namely, conventionalism regarding the choice of the widest possible frameworks, such as the ones discussed, e.g., in "Empiricism, Semantics, and Ontology". This same interpretation also explains the motivations behind Cohen's criticisms: a failure to distinguish between wider (full coverage) and narrower linguistic frameworks made some commentators feel inclined to criticize Carnap either for being a complete conventionalist (see 1963b, p. 864), or a naïve empiricist.

Now we can read this other passage quoted by Oliveira (2002) as evidence against the revisionists under a new light:

The scruples here advanced regarding the assertion that statements are to be compared with facts (or reality) were directed not so much against its content but rather against its form. The assertion is not false – if only it is interpreted in the manner indicated – but formulated in a potentially misleading fashion. Hence, one must not, in repudiating the assertion, replace it by its denial: “Statements cannot be compared with facts (or with reality)”; for this negative formulation is as much open to objection as the original affirmative one. In repudiating the formulation one must take care not to reject the procedure which was presumably intended, viz., the confrontation with observation. Nor must the significance and indispensability of such confrontation be overshadowed by exclusive attention to the second operation. (Besides, the phrase ‘Comparison of statements with each other’, instead of ‘confrontation’, seems open to the same objections.) He who really repudiates the first operation – I do not think that anyone in scientifically oriented circles does – could not be considered an empiricist (Carnap 1936, p. 126)

To be sure, Carnap can be considered an empiricist, and as such he would not want to deny that empirical statements can be confronted with facts. His views on conventionalism can be seen clearly in the *Philosophical Foundations of Physics* (1966) where he defends Poincaré and his own version of conventionalism. The discussion there is on the choice between two theories on the structure of space.

It is important to understand the nature of this choice thoroughly before asking what the geometrical structure space is. I believe that the ambiguity of this question and the elliptical phrasing of various answers by Poincaré and others led to some misinterpretations of their position (by Reichenbach, for instance). Poincaré said that the physicist can freely choose between a Euclidean geometry and any form of non-Euclidean geometry. Because Poincaré said the choice was a matter of convention, his view became known as the conventionalist view. In my opinion, Poincaré meant that the choice was made by the physicist before he decided which method to use for measuring length. After making the choice, he would then adjust his method of measurement so that it would lead to the type of geometry he had chosen. Once a method of measurement is accepted, the question of the structure of space becomes an empirical question, to be settled by observations (...) once an appropriate method of measurement is adopted, the question of the geometrical structure of space becomes an empirical problem, to be answered by making observations. (1966, p. 160)

On this particular point, Brendel's interpretation is very accurate:

There may be a question of choosing very wide parts of linguistic frameworks, which are purely analytical matters. Carnap is conventionalist in this respect: one can choose whatever analytical matters. But this is completely rational, since these decisions are not concerning the synthetical part, they mean nothing regarded to "reality". Regarding the choice about the synthetical part of a theory, Carnap is not a conventionalist. All this is completely rational in my opinion and although conventionalist in a restricted sense, not relativist at all. (2006, p. 7)

The distinction between narrow and wide linguistic frameworks is not always explicit in Carnap, and very few commentators have paid attention to it. To the best of my knowledge, only Brendel has highlighted its significance. It seems essential for a proper understanding of the passages where Carnap seems to oscillate between a view that seems strictly empiricist and a more conventionalist view. Failure to see this has led commentators to overstate either Carnap's conventionalism (Friedman), or his empiricism (Oliveira). Oliveira believes that Carnap's reply to Cohen is a refutation of the revisionist interpretation. The distinction of framework scopes, however, ends up saving both interpretations. The conventionalism claimed by the revisionists remains relative to linguistic frameworks, and it is not the pure conventionalism that Carnap explicitly rejects; the empiricism claimed by Oliveira remains relative to the decisions taken after the framework has been chosen.²³

²³ Our view is in harmony with the use of the term "logical empiricism" or "logical positivism" precisely because it indicates the two components. See, for example, Carnap (1928, p. vi).

2.2 On Psillos's criticisms

Psillos (2008), in turn, criticizes the content of some of the specific approximations of Carnap and Kuhn made in the “revisionist” literature. He denies that in Carnap there is any kind of semantic holism, incommensurability, or theory-ladenness of observations. Regarding holism, Psillos maintains that Carnap's views are better described as ‘local holism’, which differs from the more generalized kind of holism we find in *Structure*.

According to Psillos, even on his later works Carnap was always concerned with finding a criterion of meaningfulness for individual theoretical terms, and not for a theory as a whole: a term is said to be meaningful if it contributes to the observational content of a theory (Carnap 1956, p. 49). Carnap wants a meaningfulness criterion neither too restrictive – that excludes theoretical terms useful for science – nor too broad – that allows for the meaningfulness of speculative metaphysics. Psillos thinks that this motivation indicates that Carnap is trying to avoid any commitment to generalized semantic holism, given that even if the meaningfulness criterion is relative to a language, determining whether a theoretical term is meaningful is something to be done case by case: one term at a time. Psillos explains that Carnap remained faithful to this view even after Hempel (1963) pointed out that his attempts are to be found lacking: in science there are theoretical terms which are introduced with the sole purpose of connecting other terms, and which on their own carry no empirical content.

Regarding the theory-ladenness of observations, Psillos argues that Carnap understands the concept of analyticity as unproblematic for observational language and that the meanings of observational terms are determined by “analytic semantic rules”, and concludes that “it is not theory that informs their meaning, nor indeed any synthetic truths about the world” (Psillos 2008, p. 138). Because Psillos thinks that Carnap maintains neither semantic holism nor the theory-

ladenness of observations, and given that these two theses would be the main reason for incommensurability, he concludes that Irzik and Grünberg's claim that Carnap endorsed or assented independently to incommensurability is untenable. Psillos acknowledges that Carnap admits that shifts in meaning may happen in a given historical period – a revolution – but for Psillos those shifts do not entail a widespread and radical change in meanings, as they would in Kuhn:

Irizik and Grünberg are not justified in concluding that Carnap endorsed (or, worse, independently accepted) Kuhn's thesis that competing paradigms in physics are incommensurable. To be fair to them (and to Carnap) he did admit that there will be meaning changes “when a radical revolution in the system of science is made, especially by the introduction of a new primitive term and the addition of postulates for such term” (1956, 51). This claim, however, does not entail radical meaning variance — worse, incommensurability. It does not follow (as it does on Kuhn's holistic theory of meaning) that *any*, even the slightest, change in the theoretical web will result in meaning-change. (Psillos 2008, p. 138)

Furthermore, he claims that Carnap's resistance to changes in meaning is grounded on his view that the theoretical vocabulary is an incomplete and open interpretational calculus. This means that the theoretical vocabulary can always receive the addition of new theoretical postulates and new correspondence rules consistent with the ones already in place, but that this process does not change the meanings of the theoretical terms already there, except insofar as they are rendered more precise.

2.3 A reply to Psillos

Psillos's criticisms, much like Oliveira's, points out that Carnap would be inclined to reject the Kuhnian theses assigned to him in the recent “revisionist” literature, if we are to take at face value his stated motivations. But let us put aside those motivations for a moment and look into the actual arguments put forth by Psillos. On semantic

holism, Psillos (p. 137) argues that “Carnap preferred to treat as meaningless terms that did not contribute to the empirical content of the theory, instead of adopting the (holistic) view that they acquired some meaning by ‘fusion’ with other meaningful terms”, and acknowledges that the meaningfulness criteria are judged relative to a theory. He concludes that Carnap is not committed to semantic holism, because the meaningfulness of a term is determined one case at a time. According to Psillos (p. 138), Carnap’s position is better described as local semantic holism.

But if we now look back to Irzik and Grünberg’s paper, which is the target of Psillos’s criticism, we will see that they do not say that there is any stronger version of holism in Carnap. On p. 289, they characterize Carnap’s holism in terms that are clearly compatible with what Psillos calls local semantic holism: “by semantic holism we mean the doctrine that the theoretical postulates of a theory contribute to the meaning of theoretical terms occurring in them and a change in the theoretical postulates results in a change in meaning”. This kind of weak version of holism (“local semantic holism”) is enough to justify the attribution of (local) incommensurability to Carnap, since in Carnap the theoretical terms are interdefined and are not necessarily translatable into the language of an alternative linguistic framework.²⁴ The fact that

²⁴ As is well-known, the notion of incommensurability in Kuhn’s (1962) spurred an intense debate. This led Kuhn to reformulate the notion more precisely in later works (e.g. Kuhn (1982 and 1989)), mainly to avoid the charges against incomparability (1982, p. 669). In the later works incommensurability is explicitly defined in terms of partial untranslatability (see also 1989, p. 60 footnote), which brings the notion closer to Carnap’s. It is true however that Kuhn’s earlier versions of the thesis of incommensurability are not always explicitly formulated in terms of partial untranslatability. Those earlier versions rely on a less linguistic notion of incommensurability, which we do not find in Carnap. Nevertheless they are compatible with Carnap’s views and can be seen as fleshing out in historical, sociological and psychological terms the more abstract notion that we do find in Carnap. In this sense, again, Kuhn’s work is not at odds with Carnap’s, but complements it.

the Carnap's meaningfulness criterion is meant to apply to theoretical terms one by one does not entail that the meanings of the terms that pass the meaningfulness test can also be determined one by one. Psillos seems to be confusing here the criteria for meaningfulness with how one determines the meaning of a term.

Regarding Irzik and Grünberg's claim that the meanings of observational terms are also theory-laden, Psillos puts forth a more direct objection:

Carnap (1952; 1974, 261-4) took the concept of analyticity to be entirely unproblematic for an observational language. He therefore thought the meanings of observational terms are fixed by analytic semantic rules. So, it is not theory that informs their meaning, nor indeed any synthetic truths about the world. It is this very fact that made him insist that the comparison of theories at the observational level is possible. (Psillos 2008, p. 138)

Psillos refers here to two works by Carnap: "Meaning Postulates" (1952) and *Philosophical Foundations of Physics* (1966). In them Carnap attempts to lay out the concept of analyticity semantically, using what he calls "meaning postulates" (1952, p. 66). In the 1952 paper, he does that formally; in the 1966 book, informally. A meaning postulate is statement that stipulates how a given set of terms are logically related (whether one implies the others, whether they are incompatible, etc.). Carnap uses the notion to explain analytic truths that are not logical truths in a given language system (for example, "no bachelors are married"). A given 'semantical language system' comprises logical connectives, individual variables, quantifiers, signs for individual constants (*a*, *b*, *c*, etc.) and primitive predicate signs (in the example given by Carnap in (1952, p. 66), *B* = bachelor, *M* = married, *R* = raven and *Bl* = black):

- (1) $Bl a \vee \sim Bl a$ (Fido is black or Fido is not black)
- (2) $B b \supset \sim M b$ (If Jack is a bachelor, then he is not married)

The first sentence is a logical truth (*L*-truth): there is no need to know the meanings of the descriptive terms in order to explain its truth; knowing the meanings of the logical particles ('is', 'or', 'not') suffices. The second sentence, however, is not a logical truth. So as to show that it too is true, first one needs to know what kind of relation the predicate signs (*B* and *M*) have among themselves in the language in question. In this particular language system, it turns out that *B* and *M* are logically connected according to the following meaning postulate:

$$(P_1) \quad '(x)(B x \supset \sim M x)'$$

Notice that this postulate does not give rules of designation for the predicate signs (or "descriptive constants", as Carnap calls them), but merely states that *B* and *M* are incompatible in that system. Given this postulate, (2) comes out an analytic truth (*A*-truth). Meaning postulates can thus be used to determine logical relations between observational terms. So we can agree with Psillos that Carnap "took the concept of analyticity to be entirely unproblematic for an observational language". But we cannot completely agree with his conclusion that Carnap "therefore thought the meanings of observational terms are fixed by analytic semantic rules. So, it is not theory that informs their meaning, nor indeed any synthetic truths about the world" (p. 138). Meaning postulates, as we indicated above, merely fix some of the logical relations that hold among a set of terms in a given language system. They determine "as much about the meanings [...] as necessary for analyticity", that is, in no way Carnap says that they completely determine meanings. The rules for the range of values of all variables and rules for the values of all descriptive constants are presupposed in the explication of analyticity. "We do not give rules of designation for 'B' and 'M'. They are not necessary for the explication of analyticity, but only for that of factual (synthetic) truth" (Carnap 1956, pp. 66-67).

Furthermore, the terms may have logical relations other than those determined by the meaning postulates.

Moreover, it would be odd if Carnap thought that no reference to a theory or to some synthetic truths are needed for determining the meanings of the terms in an observational language, which is exactly what Psillos seems to imply, in fact these relations are presupposed. Indeed, in (1932a, p. 49) Carnap does say that from laws of nature (in conjunction with sets of singular statements) one can deduce protocol statements (as we saw in section 1.3 above), which suggests the opposite to what Psillos is claiming, since some of the terms that are used in the protocol statements must also appear in the theoretical statements. To be sure, this does not settle the matter and there is room for discussion here. In any case, for Carnap there is a decision to be made as to which meaning postulates to include into a given language system. This is a decision to be made in accordance with the intentions and motivations for constructing the language system. And this is a point at which the theory held to be true affects the choice of the meaning postulates. Carnap uses meaning postulates to show how we might define analyticity for artificial languages, but this does not entail that those languages are detached from the world or the theories we have about it. Rather the choice about what meaning postulates to include into a language system is directly affected by the theory of the world one holds true.

To sum up: there are two main reasons for thinking that Carnap's views entail some kind of theory-ladenness of observational terms. The first is that observational terms can be deduced from laws of nature in conjunction with a set of singular statements, and the fact that some of the terms that are used in the deduced statements must also appear in the theoretical statements of a theory. The second (as we saw in section 1.3) is that observational terms can be construed both as a part of the language of science or as lying outside the language of science, but Carnap preferred the former view. Now, none of this *proves*

that there is theory-ladenness of observational terms in Carnap, although it does suggest that there is. One may, if one wishes, go along with Psillos and say that there isn't. But at best this is an interpretation which cannot exclude the alternative reading of Carnap. Hence, at least we can say that Kuhn's views on this matter cannot be taken as contradicting any explicit claim by Carnap.

On the matter of incommensurability Psillos seems to have misinterpreted Kuhn, by attributing to him a thesis stronger than the one he actually held. It is true that Carnap never held the stronger version of the thesis that Psillos assigns to Kuhn. But if follow what Kuhn actually says about incommensurability, then we will find similar views in Carnap. Psillos says that the radical theory changes described by Carnap do not imply radical meaning variance. He quotes from Carnap's (1956) "The methodological character of theoretical concepts", which says that a theoretical term is significant only relative to a theory *T*. The significance of a term "cannot possibly be decided without taking into consideration the postulates by which it is introduced" (p. 50). Carnap toyed with the idea that a discovery or observation of a new fact can render one term that is taken as significant to be nonsignificant. He then denied that possibility by considering the character of the theory *T*: If *T* contains only theoretical postulates (the fundamental laws of nature) and no singular statements describing single facts, new facts cannot interfere with the class of significant theoretical terms. This, of course, does not render them eternally significant: "This class will generally be changed only when a radical revolution in the system of science is made, especially by the introduction of a new primitive theoretical term and the addition of postulates for that term" (1956, p. 51).

This agrees with the kind of holism considered above: "by semantic holism we mean the doctrine that the theoretical postulates of a theory contribute to the meaning of theoretical terms occurring in them and a change in the theoretical postulates results in a change in

meaning” (Irzik and Grünberg, 1995, p. 289). So, what Carnap states in (1956) is that only a change in the postulates – or addition of a new one – can modify the meanings of the theoretical terms. This also agrees with the notion of incommensurability as partial untranslatability (put forth by Kuhn), and is confirmed by Carnap’s “Reply to Quine” (Carnap, 1963b, p. 921) and by “Truth and Confirmation” (1936, p. 126), where the impossibility of completely translating one language into another due to the presence or absence of new concepts (and new postulates for that concepts) is considered: “these concepts cannot be subsequently included since they presuppose a different form of language”.

The reader may remember here the notion of a no-overlapping principle, developed by Kuhn in “The Road since *Structure*” (1990, p. 4): “no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus”:

There are no dogs that are also cats, no gold rings that are also silver rings, and so on: that’s what makes dogs, cats, silver, and gold each a kind. Therefore, if the members of a language community encounter a dog that’s also a cat (or, more realistically, a creature like the duck-billed platypus), they cannot just enrich the set of category terms but must instead redesign a part of the taxonomy. (p. 4)

Now this redesign of a part of the taxonomy can be viewed as a change of the language as described by Carnap. The new fact of observing a dog that is also a cat cannot, like Carnap’s description, directly change the meaning of “dog” or “cat”, but it does requires a change in the taxonomy (the introduction of a new kind).

In interpreting Kuhn’s holistic theory of meaning, Psillos seems to draw only from Kuhn’s initial formulations found in *Structure*, apparently ignoring later formulations – strangely, since he does note them on p. 145 – which do not assume that any theoretical change whatsoever will yield meaning changes: “Incommensurability thus becomes a sort of untranslatability, localized to one or another area in

which two lexical taxonomies differ” (Kuhn, 1990, p. 5). This is a weaker notion of incommensurability than the one Psillos assigns to Kuhn, as we saw above (section 2.2). So one of Psillos’s reasons for wanting to say that Carnap and Kuhn differ on this point rests on a misreading of Kuhn. If Kuhn’s thesis were as strong as he claims, then he would be right, but it isn’t. Moreover, Psillos seems to think that Kuhn’s thesis of incommensurability entails some kind of incomparability between theories:

There has been a lot of work on whether incommensurability implies incomparability. What is certain, I think, is that translatability implies comparability. Hence, incomparability implies untranslatability. What Carnap’s move in effect secures is that there cannot be conditions of general incomparability among theories. Hence, there cannot be conditions sufficient for untranslatability. Besides, Kuhn’s dictum that “if two theories are incommensurable, they must be stated in mutually untranslatable languages” (1983, 669-670) would be the major premise of a Carnapian *modus tollens* of its antecedent, the minor premise being that the theories of physics are not stated in mutually untranslatable languages, but in one and the same language Lt. (Psillos 2008, p. 146)

This passage by Psillos indicates that he thinks that because there is no ground for attributing general incomparability to Carnap, he could not have held any kind of untranslatability between theories. But this is clearly an invalid inference. Partial untranslatability between two theories can occur at the theoretical level even when they remain comparable and translatable at the observational level. Furthermore, incommensurability is *not* the thesis (“Kuhn’s dictum” according to Psillos) that incommensurable theories are *completely* untranslatable. Rather, Kuhn’s thesis is merely that *some* of the central terms of those theories are not intertranslatable: “Most or all discussion of incommensurability have depended upon the literally correct but regularly overinterpreted assumption that if two theories are incommensurable, they must be stated in mutually untranslatable

languages” (1982, pp. 669-670):

No more in its metaphorical than its literal form does incommensurability imply incomparability, and for much the same reason. Most of the terms common to the two theories function the same way in both; their meanings, whatever those may be, are preserved; their translation is simply homophonic. *Only for a small subgroup of (usually interdefined) terms and for sentences containing them do problems of translatability arise.* The claim that two theories are incommensurable is more modest than many of its critics have supposed. I shall call this modest version of incommensurability 'local incommensurability'. Insofar as incommensurability was a claim about language, about meaning change, its local form is my original version. (1982, pp. 670-671; italics added)

The kind of overinterpretation mentioned by Kuhn is exactly the one we see in Psillos. In his later works, Kuhn says that overlapping lexical structures render possible communication and comparison between theories even while conceptual changes are being made:

Whether the communities in question are displaced in time or in conceptual space, their lexical structures must overlap in major ways or there could be no bridgeheads permitting a member of one to acquire the lexicon of the other. Nor, in the absence of major overlap, would it be possible for the members of a single community to evaluate proposed new theories when their acceptance required lexical change. Small changes, however, can have large-scale effects. The Copernican Revolution provides especially well-known illustrations. (1990, p. 12)

This, of course, does not rule out cases of untranslatability. But even in these cases comparability can occur through the acquisition or learning of new taxonomies without actually translating it:

Faced with untranslatable statements, the historian becomes bilingual, first learning the lexicon required to frame the problematic statements and then, if it seems relevant, comparing the whole older system (a lexicon plus the science developed with it) to the system in current use. Most of the terms used within either system are shared by both, and most of these shared terms occupy the same positions in both lexicons. Comparisons made using those terms alone ordinarily provide a

sufficient basis for judgment. But what is then being judged is the relative success of two whole systems in pursuing an almost stable set of scientific goals, a very different matter from the evaluation of individual statements within a given system. (1989, p. 77)

These suggestions might perhaps be seen as removing from incommensurability any serious or interesting consequences. This is not the case. In the postscript to his (1982), Kuhn ponders if the scientist experiences the same sudden recognition of new patterns as the historian does, and remarks that the scientist can only perceive small changes, contrary to the historian, to whom global revolutionary change pop out. But no matter how they are perceived, at least some of those changes will have a holistic character. If a change is made in the most central concepts of a theory, that will produce a change in the lexical structure, or, in a Carnapian terms, a change of language, a “radical alteration, sometimes a revolution”.

3. Assessments

Carnap and Kuhn indeed do seem to hold at least four compatible and complementary theses, as we have shown in section 1 above. But their relevance to their general accounts of science seems to differ. Carnap, although very much aware of scientific revolutions, does not dwell on its implications in his writings nor does he explicitly take them as guiding points for his research on science, as Kuhn does. Yet at the very least Kuhn’s theses mentioned above do not seem *prima facie* incompatible with Carnap’s philosophy of science. Kuhn uses a historiographical methodology in his analysis of science, whereas Carnap’s account is based on the logical analysis of the language of science (or the logic of science), and it is not at all strange that the two authors should come to agree on some general theses, despite their differing starting points. One striking fact is that when Kuhn begins to analyze more closely the formal uses of language, his views begin to resemble Carnap’s more closely. See, e.g., his shift from the term

“paradigm” to “structured conceptual lexicon” in Kuhn (1989) or in (1990) and (1993, p. 316).

Recent authors commenting on these matters are split up into two main camps, as we have seen. Oliveira (2007) argues that as a historical point of fact, the revisionist reading of Carnap is untenable. His reasoning, even if correct, little says against attributing to Carnap some of the theses associated with Kuhn.²⁵ The letters exchanged between Kuhn and Carnap are an historical curiosity that do not render untenable the revisionist reading of Carnap. They are, as Uebel says (2011, p. 3), the “icing on the cake”, not the cake itself.

Psillos (2008), on the other hand, argues directly against a point-by-point approximation of Carnap and Kuhn, but his reasons do not settle the matter either: (1) Even if in Carnap we are not to find a generalized form of semantic holism, local semantic holism is all that is needed to approximate Kuhn and Carnap, because this is in fact the version of holism that we find explicitly in Kuhn’s later works (and, according to Kuhn, also in some of his earlier works – see Kuhn 1989, p. 60 footnote). Additionally, Psillos seems to disregard passages where Carnap explicitly defends confirmation holism and semantic holism (e.g., Carnap 1934 mentioned above). (2) The fact that Carnap has a meaningfulness criterion applicable to individual terms in (1956) does not entail that the meaning of those terms can be determined in isolation from other terms. Terms that are meaningful on Carnap’s criterion, and therefore belong legitimately to a theory, acquire their meaning (even if only partially) relative to the theoretical postulates and correspondence rules of that theory – and this is, again, semantic holism. (3) Regarding the theory-ladenness of observations, Psillos’s argument that observational terms are “fixed” by analytic semantic rules disregards passages in Carnap’s (1932a). Lastly, (4) Psillos seems to

²⁵ Oliveira (2004) does discuss the motivations behind the revisionist reading of Carnap on some specific theses, especially the ones in Carnap (1936), but we shall leave that aside here and take it on at another more detailed paper.

overinterpret the notion of incommensurability in Kuhn and based on this infers that Carnap and Kuhn have incompatible views on the matter.

What has been said here suggests that Carnap and Kuhn might indeed have been quite akin on some matters. But one ought not to forget that Carnap and Kuhn differ significantly on at least two further issues that the literature on the topic seems to have mostly neglected: the relations between science and metaphysics and the context of discovery / context of justification distinction. According to Kuhn (at least in *Structure*), scientific paradigms are, among other things, ways of seeing the world, and for that reason there is a metaphysical element in them (something that is assumed about the most basic elements and principles of nature, but which is not subject to empirical confirmation or refutation during the periods of normal science). It seems, then, that some metaphysical elements are constitutive of science. Carnap, on the other hand, always distinguishes clearly between metaphysical claims and scientific claims, always arguing that the former makes no sense, as opposed the latter. Regarding the context of discovery / context of justification distinction, it seems that at least prima facie Kuhn's work can be interpreted as calling into question its tenability, whereas Carnap seems to have upheld it throughout. These seem to be significant differences between Carnap and Kuhn, but which we shall leave aside here and explore in another paper.

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