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DID KUHN KILL LOGICAL EMPIRICISM?*

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In the light of two unpublished letters from Carnap to Kuhn, this essay examines the relationship between Kuhn's *The Structure of Scientific Revolutions* and Carnap's philosophical views. Contrary to the common wisdom that Kuhn's book refuted logical empiricism, it argues that Carnap's views of revolutionary scientific change are rather similar to those detailed by Kuhn. This serves both to explain Carnap's appreciation of *The Structure of Scientific Revolutions* and to suggest that logical empiricism, insofar as that program rested on Carnap's shoulders, was not substantially upstaged by Kuhn's book.

1. Introduction. The publication of Thomas Kuhn's *The Structure of Scientific Revolutions* (1970) is often regarded as a turning point in twentieth-century philosophy of science. Along with Stephen Toulmin's *Foresight and Understanding* (1961), Norwood Russell Hanson's *Patterns of Discovery* (1961) and others, this much more popular book ushered in the so-called "new philosophy of science" and—popular wisdom has it—the demise of logical empiricism. Clearly, *The Structure of Scientific Revolutions* rides atop a wave of reaction among many philosophers and historians of science to the logical empiricist program. Responding largely to Kuhn, philosophy of science in the 1960's and 1970's became less concerned with explicating the logical structure of theory, confirmation, and explanation and more concerned with actual scientific reasoning and the historical structure of scientific change. The very first sentence of Kuhn's book announces, "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (1970, 1). Judging from some of Kuhn's arguments, that "image" is one entertained by contemporary philosophers of science.

Yet, beyond this account of recent comings and goings in philosophical fashion, Kuhn's book is often suggested to amount to a refutation of logical empiricist views of science; in Kuhn's hands, history not only could but *did* produce "a *decisive* transformation" in philosophy of science.

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Arthur Danto (1985), for instance, has recently described such a transformation. For logical empiricism:

> [t]he philosophical task was to provide a formal reconstruction of the *language* of science, conceived of as a logical edifice, resting upon observational reports, overarched by sentences of increasing generality and scope. . . . This shimmering architecture was one of the great visionary ideals of modern philosophical thought, but it was destined soon to be dismantled, if not brutally demolished, through a quite different philosophical task and a wholly different conception of science itself. (Pp. x–xi)

After Hanson's attack on the observation-theory distinction, Danto says, and "the subsequent historicization of theory by Kuhn, the revolution was in all essentials complete" (ibid., xi). If the work of Hanson and Kuhn "demolished" the logical empiricist conception of theory as a linguistic-logical edifice, there would seem to have been a well-joined argument, one that Kuhn et al. won.[1]

In this essay, I will show that this argument is not as well-joined as Danto and others imply. Representing logical empiricism will be Rudolf Carnap, arguably its most creative and influential proponent. On the basis of his published writings and two unpublished letters sent to Kuhn, I will show that Carnap did not see *The Structure of Scientific Revolutions* as a challenge to his own philosophical views, and further that it should not be seen as such. 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. The following discussion of these points should give pause to those generalizing that Kuhn "did in" logical empiricism.

2. Two Letters from Carnap to Kuhn. Kuhn's book was originally commissioned as a monograph for the series *Foundations of the Unity of Science* (Neurath et al. 1955, 1970). This was a two-volume collection issued to introduce the *International Encyclopedia of Unified Science,*

[1]Frederick Suppe reviews logical empiricism as the "Received View" of scientific theory and examines the alternative views of Hanson, Kuhn, Toulmin, Feyerabend and others. To the same effect, he notes, ". . . these alternatives were such that their acceptance required or presupposed the rejection of the Received View" (1977, 119).

that brainchild of Otto Neurath which was intended to promulgate a logical empiricist view of the sciences as more or less unified by method and epistemological foundation. Except for Gerhard Tintner's "Methodology of Mathematical Economics and Econometrics" which appeared in 1968, Kuhn's monograph—volume 2, number 2—was the last to be published in this introductory series. Since volumes 3–9 comprising the *Encyclopedia* proper were never realized, *The Structure of Scientific Revolutions* was the *Encyclopedia*'s last publication—a seeming irony, or telling fact, given the book's reputation as the nemesis of logical empiricism.

At the time when Kuhn was commissioned, Carnap and Charles Morris were associate editors. The following letters, reproduced here in their entirety, were written by Carnap acting in this editorial capacity. In the first, on the basis of certain manuscripts, Carnap expresses his approval of Kuhn's writing a monograph for the encyclopedia:

> Dear Professor Kuhn:
>
> Thank you very much for sending me your manuscripts. I have read them with great interest, and on their basis I am strongly in favor of your writing a monograph for the Encyclopedia, as you lined out in your letter to Morris of February 13th. I hope that you will find it possible to write your first draft this summer.
>
> I believe that the planned monograph will be a valuable contribution to the Encyclopedia. I am myself very much interested in the problems which you intend to deal with, even though my knowledge of the history of science is rather fragmentary. Among many other items I liked your emphasis on the new conceptual frameworks which are proposed in revolutions in science, and, on their basis, the posing of new questions, not only answers to old problems.
>
> I am returning your mss. as educational materials and I will send a copy of this letter to Morris.
>
> Sincerely yours. . . . (12 April 1960)

In the second letter, Carnap discusses his impressions of the completed manuscript:

> Dear Professor Kuhn:
>
> Simultaneously I am returning your manuscript "The Structure of Scientific Revolution". I am happy that it is now in final form and that the U. of Chicago Press has found a way of publishing it in its full length. I am especially gratified by the fact that we can incorporate this work into the Encyclopedia.
>
> I am convinced that your ideas will be very stimulating for all those

who are interested in the nature of scientific theories and especially the causes and forms of their changes. I found very illuminating the parallel you draw with Darwinian evolution: just as Darwin gave up the earlier idea that the evolution was directed towards a predetermined goal, men as the perfect organism, and saw it as a process of improvement by natural selection, you emphasize that the development of theories is not directed toward the perfect true theory, but is a process of improvement of an instrument. In my own work on inductive logic in recent years I have come to a similar idea: that my work and that of a few friends in the step for step solution of problems should not be regarded as leading to "the ideal system", but rather as a step for step improvement of an instrument. Before I read your manuscript I would not have put it in just those words. But your formulations and clarifications by examples and also your analogy with Darwin's theory helped me to see clearer what I had in mind.

From September on I shall be for a year at the Stanford Center. I hope that we shall have an opportunity to get together and talk about problems of common interest.

With best regards yours, . . .[2] (28 April 1962)

If Carnap had just read the monograph which doomed logical empiricism, we should certainly ask why these letters are so complimentary. However we must first consider why Kuhn (1970) is seen as a challenge to logical empiricism.

3. Kuhn's Challenge to Logical Empiricism. The reputation of Kuhn (1970) as a watershed in philosophy of science stems largely from its provocative thesis that competing theories can be "incommensurable". As "paradigms", theories are not only sets of propositions about nature, but rather ways of conceiving nature and natural phenomena. These different conceptual frameworks carry within them standards of what is to be explained, the very form explanation may take, and the sense and meaning of words. On this account, comparisons of theories have no recourse to paradigm-independent criteria of goodness. As Kuhn's historiography intends to show, the arguments and beliefs of many historical figures are accordingly constrained by their paradigmatic allegiances; only a rare genius, such as Galileo, is able to see beyond the confines of extant paradigms and synthesize a revolutionary successor.

To buttress this historical argument, however, Kuhn offered psycho-

[2]Lest Carnap's sincerity be doubted and his favorable comments be understood as editorial encouragment, he adds a note to Charles Morris on the carbon copy of the second letter: "Dear Charles, herewith my approval of Kuhn's ms., which is really a fine piece of work" (28 April 1962).

logical arguments against the notion that theories can simply be judged according to how well they fit the facts, or, for logical empiricism, according to their measure of empirical confirmation. That is, for psychological reasons, he claimed, a neat and clean theory/observation distinction—perhaps the central feature of logical empiricist models of theory—simply does not exist. Taking inspiration from Gestalt and "New Look" psychology, he argued that visual perception is a perhaps unconscious but necessarily active interpretive process (1970, 112–113; and f.n. 1, 2). One lesson of experimental psychology is "that two men with the same retinal impressions can see different things . . . [and] that two men with different retinal impressions can see the same thing" (ibid., 126–127). For this reason, he ruled out the possibility of constructing a neutral observation-language "designed to conform to the retinal imprints that mediate what the scientist sees" (ibid., 125). But his analysis also ruled out a language whose terms designate "perceptual features" of nature, for "[t]hose features must obviously change with the scientist's commitments to paradigms" (ibid.). And, leaving no logical empiricist stone unturned, he dismissed an empirical language of "concrete operations and measurements that the scientist performs in his laboratory" (ibid.); they "are not 'the given' of experience but rather 'the collected with difficulty'" (ibid., 126). Phenomenalist, physicalist and operationist observation languages, he claimed, are necessarily non-theory-neutral. Observations and operations, and any languages of them, belong only within particular paradigms.

If it were true that all visual perception is necessarily interpretive, that one's beliefs or expectations invariably inform the content of one's perceptions, then Kuhn would have a good argument with logical empiricism. Logical empiricists assumed that some distinction between theory and observation was workable. A good part of Carnap's writings, for instance, concern the kind of language which will serve as an observation language and the kind of logical relationship that observational and theoretical terms exhibit.[3] After all, the empirical foundation of a theory is

[3]Carnap explored predominantly two ways by which this link between theoretical and observational terms was to be modeled: In Carnap ([1928] 1969), theoretical statements were to be constructed as "structure statements" (p. 29) describing formal, structural features abstracted from "elementary experiences" (p. 108). Like Kuhn, Carnap appeals to Gestalt psychology. He accepts its tenet that in perception "the total impression is epistemically primary" (p. 109). Since he requires that his constructional system "agree with the epistemic order of the objects [it constructs]", these "elementary experiences", as unanalyzed "total [perceptual] impressions", are taken as its basic elements (ibid., 108–109). Since theoretical statements are constructed out of these basic elements, the relation between theoretical and observational terms is one of transformability, "Each scientific statement can in principle be so transformed that it is nothing but a structure statement" (ibid., 29).

This relation of transformability between theoretical sentences and experience was mod-

manifest precisely in the way that it is supported by observable states of affairs. If the distinction were inadmissible, and a theory-independent observation-language was as well, the central epistemic goal of logical-empiricism—the development of models instrumental for clarifying the empirical justification of theoretical knowledge—would seem much less realizable.

But this psychological argument against the theory/observation distinction is not sound, as Fodor (1984, 1988) has recently shown. While Kuhn invokes gestalt illusions (of the duck-rabbit variety) to support the claim that observation is theory-laden, Fodor points out visual illusions (Muller-Lyer illusions, for instance) which refute the generality of that claim. We may see a figure as a duck or as a rabbit depending on our background beliefs about that figure, but the one Muller-Lyer line always looks longer than the other despite our beliefs about their lengths. Now the conditions under which background beliefs do or do not have access to and influence on perceptual mechanisms are not clear. But there is no question that sometimes the way things look is affected by what we believe about them, and sometimes it is not.

Granting this, Kuhn's challenge must fall back on the historical possibility that adherents to different paradigms typically did *in fact* perceive phenomena differently. If so, then any comparison of how well the theories in question are empirically supported will still be moot, regardless of the kind of confirmation function invoked. Of course, it would be just about impossible to demonstrate that a swinging stone, to use Kuhn's example, appeared to Galileo qualitatively differently than the way it would have looked to Aristotle; we cannot literally see things through any eyes but our own. *But it does not matter,* for here is where this argument with Carnapian logical empiricism is not well-joined. Carnap's views of revolutionary theory change do not rest on, nor stand or fall with, the theory/observation distinction. In fact, his account of what is involved in scientific revolutions is remarkably similar to Kuhn's. Carnap is well aware that if two or more theories are very different, choices between them can

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ified and weakened in Carnap (1936–37). Here, the meaning of some theoretical terms are *partially* specified by "reduction sentences" (pp. 441–444, 459–450) and are not translatable into statements about experiences (ibid., 464), hence, the Thesis of Physicalistic Confirmability (as opposed to translatability and even testability), "'Every descriptive predicate of the language of science is confirmable on the basis of observable thing-predicates'" (ibid., 468).

Yet, in 1956, he reflects on this belief "that all scientific terms could be introduced as disposition terms on the basis of observation terms either by explicit definitions or by so-called reduction sentences" (1956b, 53) and says, "Today I think . . . that the connection between the observation terms and the terms of theoretical science is much more indirect and weak than it was conceived either in my earlier formulations or in those of operationism" (ibid.).

amount to choices between alternate conceptual frameworks, each of which has its own distinct means of representing scientific knowledge. And as in Kuhn's account, the choice often cannot be made in terms of any higher or more general systematic criteria. Let us now turn to Carnap's comments on revolutionary theory change.

4. Language Planning and Scientific Revolutions. Carnap typically treats scientific theories as languages. Generally, they comprise observational and theoretical vocabularies, rules of sentence formation, and are understood to invoke certain logical rules and certain bodies of mathematics. This conceptualization of theory runs throughout Carnap's philosophy of science, but his comments about contemporary and historical scientific practice are also couched in such terms. For Carnap, a scientist doesn't "refine or alter a theory" but rather "changes the truth value of an intermediate statement". As he admits in his first letter to Kuhn, he is not a historian of science. His published comments about scientific history and practice therefore reflect less historical study and more his own intuitions of what actual scientific reasoning was or is like. Nonetheless, his views about revolutionary scientific thinking are very much analogous to Kuhn's historically motivated picture of revolutionary science.

The substance of this analogy is most clearly stated in Carnap (1963b). Carnap distinguishes between two kinds of changes that a scientist typically makes in a theory when it conflicts with experience:

> . . . a change in the language, and a mere change in or addition of, a truth-value ascribed to an indeterminate statement. . . . A change of the first kind constitutes a radical alteration, sometimes a revolution, and it occurs only at certain historically decisive points in the development of science. . . . A change of the first kind constitutes, strictly speaking, a transition from a language L_n to a new language L_{n+1}. (P. 921)[4]

[4]To anticipate a query about my argument, it might be thought that this analogy between Kuhn's historical portrait of science and Carnap's philosophical intuitions about linguistic frameworks is an artifact: Perhaps Carnap conceived of scientific revolutions as wholesale changes in scientific language *only after reading Kuhn's book*. If so, not only may Kuhn have killed logical empiricism, he may have converted one of its founders to accept a kind of theoretical holism. This (at least) appears plausible since Carnap often did change his mind and openly criticize positions he had once taken on various issues. After all, his second letter reveals that he found parts of Kuhn's analysis "illuminating". This quote was published in 1963 and Carnap read the book no later than April of 1962, so Carnap possibly wrote it after that reading.

But Carnap alludes to this very connection between scientific revolutions and changes in theoretical languages in Carnap (1956b) in which he refers to "a radical revolution in the system of science . . . [made] especially by the introduction of a new primitive theoretical term and the addition of postulates for that term" (p. 51). The grounds for the analogy I draw do exist in Carnap's larger corpus, regardless of what he may have concluded about scientific revolutions from reading Kuhn.

One kind of change entails the evaluation of re-evaluation of individual statements, or sub-theories perhaps, which exist within a larger, stable theoretical language. The other, amounting to revolutionary change, sees the entire theoretical language itself being reworked as it becomes another language. In order to uncover Carnap's understanding of scientific revolutions, we must therefore examine his descriptions of the procedures involved in formulating and in choosing between alternate languages.

For Carnap, questions and problems involved in choosing and constructing languages belong to the context of "language planning" (1963a, 67–71). He describes this as a context embracing these questions as they occur in symbolic logic and in the construction of international languages. Through his study of Fregean, Intuitionist, typeless and other forms of logical systems he "became aware of the problems connected with the finding of language forms suitable for given purposes" (1963a, 68) and at the same time "gained the insight that one cannot speak of 'the correct language form', because various forms have different advantages in different respects" (ibid.). Language planning and the fact that languages need to be planned is based on this insight: Language forms are not right or wrong. They are better or worse depending on how they achieve their ends. Their evaluation must therefore turn upon *practical* considerations and it is the activity of language planning to address these.

The practical concerns of language planning must be contrasted with "internal" or "theoretical" questions which occur within a linguistic structure and whose answers are generally determined in light of that structure. Carnap (1956a) makes this distinction explicit when he aims to counter the accusation that his semantic theory "hypostatizes" abstract entities. The charge is that in making statements like "The word 'five' designates a number" (ibid., 216), Carnap mistakenly presumes that numbers have some kind of empirical ontological status—a status such that one of them can be designated by the term "five". Carnap responds by emphasizing "a fundamental distinction between two kinds of questions concerning the existence or reality of entities" (ibid., 206). Since statements about some entity or entities exist necessarily within a linguistic framework, we can direct existential questions directly to the entities or, very differently, to the framework as a whole. That is, we must distinguish between:

> [F]irst, questions of the existence of certain entities . . . *within the framework;* we call them *internal questions;* the second, questions concerning the existence or reality *of the system of entities as a whole,* called *external questions*. (Ibid.; Carnap's emphasis)

He admits that in the internal sense his semantics trivially grants "reality" to the abstract entities of which it makes use. But this introduction of terms for abstract entities in no way carries with it a *theoretical* assertion

of their absolute or framework-independent reality. Consequently:

> [T]he decisive question is not the alleged ontological question of the existence of abstract entities but rather the question whether the use of abstract linguistic forms . . . is expedient and fruitful for the purposes for which semantical analyses are made. . . . (Ibid., 220–221)

Justification for the use of these abstract terms does not lie in their reference, but in how well they contribute to the aims of the theory in which they operate.

Similar considerations inform Carnap's choices of observation bases in logico-linguistic models of theory. One of his stated reasons for employing a phenomenalist basis was his "intention to have the constructionist system reflect not only the logical-constructional order of the [constructed] objects, but also their epistemic order" ([1928] 1969, 101). Yet, by Carnap (1936–37), he had changed his mind to prefer a physicalist language of "things" to facilitate the operation of his then introduced reduction sentences and also because of the intersubjectivity provided by a completely and commonly understood physical language (pp. 463–467; 1963a, 51–52). This freedom to adopt linguistic forms according to one's purposes is the substance of Carnap's "principle of tolerance",[5]—a principle based on this decoupling of ontological claims from the use of linguistic forms and this "insight" about the various strengths different languages exhibit. Introducing a phenomenalistic observation language, for instance, does not entail the claim that visible things really are sensations, or constructed out of them; nor does the use of a physicalist thing-language commit one to the view that visible physical things are the ultimate constituents of reality.

Although he does not say so explicitly, clearly Carnap also sees science itself as an activity that, like philosophy, involves its own share of language planning. His comment above that scientific revolutions are tran-

[5]Carnap notes he adopted this principle in his ([1934] 1937) and that while writing the *Aufbau* ([1928] 1969) he was developing "a more and more neutral attitude with respect to the language forms used by the various philosophical schools" (1963a, 44).

In his ([1935] 1963b) Carnap upholds a similar idea: "The relativity of all philosophical theses in regard to language" (p. 451). He writes, "Suppose two philosophers get into a dispute, one of them asserting: 'Numbers are classes of classes,' and the other: 'No, numbers are primitive objects, independent elements.' They may philosophize without end about the question what numbers really are, but in this way they will never come to an agreement" (ibid., 450). But if the two theses are rendered in the formal rather than material mode, as respectively "'in L1 numerical expressions are elementary expressions'" (ibid., 451) and "'in L2 numerical expressions are class expressions of the second order'", (ibid.) then their being embedded in different languages becomes transparent. Since each statement refers properly to its language, the illusion that the two statements make competing metaphysical claims as to the reality and character of numbers in themselves is dispelled. Each properly refers only to the status of "numerical expressions" in different language systems. Carnap concludes, "the controversy has ceased to exist" (ibid.).

sitions from one scientific language to another suggests this, as does a discussion found in Carnap ([1935] 1963b). At the time of this essay, Carnap held that "the analysis of the formal structure of language as a system of rules, is the only method of philosophy" (p. 459). Applying this syntactical method to the issue of physical causality, Carnap contends that the matter of physical laws being statistical or deterministic is really a practical matter of the form in which they are stated. Anticipating the objection that experimental results and not linguistic practicalities should decide the form of physical laws, he says:

> . . . we must bear in mind the fact that the empirical results at which physicists arrive by way of their laboratory experiments by no means *dictate their choice* between the deterministic and the statistical form of laws. The form in which a law is to be stated has to be decided by an act of volition. This decision, it is true, depends upon the empirical results, but not logically, only practically. (Ibid., 454)

Carnap admits that "this decision . . . depends upon the empirical results" and that the "practical connection between the empirical results and the form of physical laws" may be very "close" (ibid., 455). Still, he appears to suggest, between empirical results on the one hand and the linguistic form of physical theory on the other, there is an ineliminable space in which scientists become language planners. Taking experimental results into account, they must choose which formulation of a theory is "more suitable with regard to the whole system of physics" which would result (ibid., 454–455).[6] In Carnap (1956a), he makes a similar point:

[6]Even though Carnap writes this well before relatively recent theoretical and experimental studies concerning the possibility of microphysical theories with local hidden-variables, he nonetheless alludes to experimental results which suggest that "one mode of formulation would be more suitable than another" ([1935] 1963b, 454–455). Assuming with Carnap that experiment can capture the deterministic or alternatively the statistical behavior of nature, there would not seem to be much "practical" leeway in choosing which form of physical law "would be more suitable". Consequently, the "act of volition" (ibid., 454) entailed in this choice seems trivial and this claim that problems of causality are syntactical problems seems misconceived. The choice may not be logically entailed by experimental results, but it should depend on them more than practically; it should depend on them crucially. Carnap's emphasis on practicalities in this issue seems inappropriate—even more so in light of recent experimental verdicts against local hidden-variable theories (see Aspect et al. 1981, 1982; and Aspect, Dalibard and Roger 1982).

On the other hand, however, the point that practical decisions help determine the very form and structure of physical laws rings true, especially considering that when Carnap wrote about the issue it was "still a matter of discussion"—a "discussion about the future form of physical language and especially the form of fundamental physical laws" ([1935] 1963b, 455). The Einstein-Bohr dialogue, for instance, can be seen as a contribution to this discussion through which the language of physics was planned. In philosophy of science, Carnap says, "[Language] planning means to envisage the general structure of a system and to make, at different points in the system, a choice among various possibilities, theoretically an infinity of possibilities, in such a way that the various features fit together

> The acceptance or rejection of abstract linguistic forms [in semantical theory], just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. (P. 221)

Here, the connection between philosophical language planning and scientific activity is clear. In both cases, the adequacy of languages is to be evaluated practically, without recourse to overarching criteria.

How then, in the case of scientific revolutions, are competing theories or alternate scientific languages judged? Since he takes scientific activity to be a species of language planning, Carnap would answer that the theories in question are to be judged in this practical sense. One must consider the purposes for which they are constructed, to what extent they achieve their ends and how well they do it. If choosing between frameworks for the very expression of scientific knowledge, such criteria must be as vague as these. There can be no algorithmic procedure in such cases, precisely because different theories, as different languages, may well have various strengths and weaknesses. The best will be that which is best only relative to a given set of purposes.

5. Carnap on Kuhn. These considerations explain why Carnap appears to have been at home with the kinds of issues *The Structure of Scientific Revolutions* raises. He "liked [Kuhn's] emphasis on the new conceptual frameworks which are proposed in revolutions in science, and, on their basis, the posing of new questions, not only answers to old problems". This should not be surprising, for such framework questions—in the form of *linguistic* framework questions—constitute the "practical" half of Carnap's philosophy of science. Carnap's evident delight with Kuhn's Darwinian picture of scientific progress can also be readily explained in this light. Carnap says he found the Darwinian analogy "illuminating" not for his understanding of the history of science but rather for his understanding of his own and others' work "in the step for step solution of problems". Just as organic "evolution is not directed towards a predetermined goal", Carnap's principle of tolerance ensures that there is no one ideal philosophical model of scientific theory, no one "ideal system" toward which philosophical analyses of science will converge. Rather, various philosophical goals will engender "species" of philosophical "instruments" each intended to clarify and reconstruct scientific reasoning

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and the resulting total language system fulfills certain given desiderata" (1963a, 68). Einstein's criticisms of quantum mechanics turn on just such a desideratum for physics: Wave mechanics should convey a "complete" description of microphysical systems. See Einstein, Podolsky and Rosen 1935; and Bohr 1935.

from a particular vantage and for a particular set of purposes. Just as organic species may become more fit in their respective niches, these different instruments may become more "efficient" or "fruitful" in fulfilling their purposes (1963a, 66). On the other hand, these instruments may prove useless and become extinct—a point made in Carnap (1956a), written perhaps in those "recent years" when he had "come to a similar idea" about the evolution of philosophical tools:

> Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. (P. 221)

Presuming that this is the substance of his illumination, Carnap says that he was led to it by Kuhn's "formulations and clarifications by examples and *also* by [his] analogy with Darwin's theory" (emphasis added). That is, not only did Kuhn's Darwinian analogy for science evoke Carnap's "similar idea" about progress in his philosophical work, but so did Kuhn's larger historical portrait of science, which is what Carnap probably means by "formulations and clarifications by examples". This branch of the analogy—between progress in logical empiricism (or at least in inductive logic) and scientific history as a succession of paradigms of frameworks—is precisely the analogy this essay has established: Just as Kuhn holds different paradigms to be "incommensurable" since they each contain their own standards by which paradigms themselves should be evaluated, different philosophical languages must be evaluated not according to any single canon of adequacy but rather against the various purposes for which they are introduced.

6. Conclusions and Qualifications. My central point in this essay is that if this principle of language planning in philosophy is extended to cover choices between radically different theories in science—and I have shown that Carnap makes just such an extension—the picture of revolutionary science that results is very much like Kuhn's. To borrow Kuhn's terminology "normal science" in this Carnapian picture is scientific activity within a theoretical framework, that is, within a scientific language. Here, the language remains stable and scientific activity consists of assigning truth values to theoretical statements. On the other hand, "revolutionary science" for Carnap would occur when the very suitability and value of that language is called into question. At this point, the terms of debate shift: The very structure and form that scientific language should take is now up for grabs, and arguments about what that form and structure should be may not be easily settled. Since the value of linguistic frameworks resides in their utility with respect to practical purposes, there

will be a sense in which revolutionary scientific debate will turn on values and principles, on beliefs about what scientific languages should do and how they should do it.

Kuhn makes just this point while defending his own historiography against the tired complaint that it bestows "irrationality" on science: "Debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof" (1970, 199) for debate about the merits of incommensurable theories "is about premises" (ibid.)—the premises that would contain canonical and universally accepted standards for their evaluation. Until these are available and accepted, this debate will similarly turn on values and practical desiderata.

As evidenced by these letters, Carnap comes away from Kuhn's book without exhibiting any reservations about the implications for scientific rationality it is reputed to contain. I will not dwell on this issue further than to note this and to suggest that Carnap saw something many critics of *The Structure of Scientific Revolutions* seem to have missed: Scientific activity does depend in certain ways on practical decisions which, by virtue of what they are decisions about, namely, first principles, cannot be justified by appeal to first principles. It appears that in Carnap's judgement, no implication establishing irrationality in science follows from this fact.

Finally, the aim of this essay is mostly expository. The present lack of editorial comments about claims made by Kuhn and by Carnap should not be taken to imply that their parallel views on scientific revolutions are without problems, nor that Kuhn's "revolutionary" views of science have not themselves been directly challenged. Nor have I offered any substantial account of why logical empiricism did fall into disfavor. If the Kuhnian "watershed" constitutes a chapter in the history of philosophy of science, I have only suggested that this chapter should recount not an argument between Kuhn and Carnap but rather the introduction of a historical picture of science—one which highlights "practical" aspects of scientific reasoning that had already received parallel and similar treatment by the archon of logical empiricism.

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