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ARE DINOSAURS EXTINCT?

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Abstract. It is widely believed that empiricism, though once dominant, is now extinct. This turns out to be mistaken because of incorrect assumption about the initial dominance of logical empiricism and about the content and variety of logical empiricist views. In fact, prominent contemporary philosophers (Quine and Kuhn) who are thought to have demolished logical empiricism are shown to exhibit central views of the logical empiricists rather than having overthrown them.

According to a new and widely accepted theory, dinosaurs still exist (Bakker, 1986; for a more cautious view see Horner and Lessem, 1993). This is not because they have been discovered on some hitherto unexplored island, and it is not because some mad scientist, or even Steven Spielberg, has recreated them. Rather dinosaurs continue to exist everywhere, all around us – as birds. Admittedly it is a bit difficult to reconcile the huge lumbering lizards of youthful imagination with the hummingbirds that zoom from flower to flower outside my window. But who are philosophers to argue with facts? Or with well attested theories either? Well, we could ask how the case was made, that is, what in the way of evidence or argument convinced much of the scientific community that dinosaurs are birds. The answer is quite simple, even if the details are not. Our earlier judgments about dinosaurs,

it now seems, were just wrong. Dinosaurs now appear to have been warm blooded (Bakker, 1986, throughout), light boned (Bakker, 352-3, 363-5), agile (Bakker, 214-25), and some of them surprisingly intelligent (Bakker, 371). They weren't lumbering lizards at all. Furthermore we were certainly myopic in concentrating on only a few of myriad kinds of dinosaurs; we were too little impressed with the enormous variety within the realms of dinosaurs and of birds. Okay, it is easy to be wrong when we don't know much and especially when that little turns out to be mistaken.

As everyone has guessed by now, this paper has almost nothing to do with lizards or birds. Rather the "dinosaurs" of interest to me here are philosophic ones: the logical empiricists. Almost nobody doubts that *they*, the logical empiricists, are extinct. In this essay I want to challenge that widely shared assumption. But before I do, we need to reflect a bit more about those assumptions of extinction. Nearly every book and a great many articles in the philosophy of science all through the last third of a century begin with a brief recital on the theme: Why logical empiricism is dead (and deserves to be). But if the movement is extinct, why belabor the point for so long? Perhaps all of these authors just want to begin their expositions on a point of agreement with their audience. Or perhaps these authors are all presbyterians and believe in punishment after death.

Exactly when the death is supposed to have occurred is a bit unclear, but there are several prominent candidates. One of the most widely mentioned is 1951, which is when Quine's first full-blown attacks on analyticity were published in *Two Dogmas of Empiricism*. (Among Quine-as-executioner theorists, 1936 (*Truth by Convention*) and 1960 (*Carnap and Logical Truth*) are also mentioned.) Although I am engaged in a long study of analyticity and will have more to say later in this essay, there is reason even at this point to wonder whether Quine's argument was really the prime agent of extinction. This is because those arguments are quite general. If they are successful against analyticity, they are equally successful against meaning, intention, necessity, (and ultimately, I would argue, probability and confirmation, though Quine does not make these last targets explicit) (Quine, 1951, 29-30). It may be that Quine caused the philosophic community to become suddenly skittish about analyticity, but there was at the same time the great modal logic boom of the 1960s and 70s. Since it is the same argument, it cannot both be successful against analyticity and unsuccessful against necessity. So was the philosophic community really persuaded by Quine's argument?

A second candidate for the death date of logical empiricism is 1962 with

the publication of Kuhn's *The Structure of Scientific Revolutions.* This certainly became wildly popular both in and out of philosophy and the subject of much philosophic criticism as well. Furthermore, its contents, if accepted, were widely perceived as devastating to logical empiricism. On the issue of whether this perception is correct, however, I will simply delay discussion until later in this essay.

A third and intriguing hypothesis about what killed logical empiricism centers on a conference held at the University of Illinois in 1969, the proceedings of which along with a very substantial introduction were published in 1974 as *The Structure of Scientific Theories*, edited by Frederick Suppe. What is intriguing about the hypothesis is that it focuses, not on some new argument, but on a transformation in the way that a later generation conceived of logical empiricism. The conference (and book) treated logical empiricism, not as a tradition or movement within which development and change would be natural and welcome, but rather as a fixed set of doctrines (called the "Received View") to be accepted or rejected. But none of us believes *exactly* what our forbearers did, or even what we ourselves did only a short time ago. Whatever is set in stone or petrified is dead, and only in the "Carnival of the Animals" do the fossils themselves dance. Even within a single species there is change. But when we speak of a group of species like birds (or logical empiricists) it is even more obviously too much to ask that the early ones be exactly like the later examples. The real question is whether there are enough resemblances (in admittedly vague senses of 'enough' and 'resemblances') and the proper sort of historical connections to warrant grouping dinosaurs with birds or the logical empiricists with philosophers active today.

Certainly there was a change in how logical empiricism was perceived. Perhaps all of these three "causes" combined to bring about the change of perception. But a perception of extinction is not the same thing as extinction itself. So perhaps we ought to review the case again to see whether an extinction really did occur.

Before we can determine whether there has been a dramatic decline in logical empiricist numbers or influence, we would have to have at least some ideas of their status in their heyday. One writer on the subject has said: "For over thirty years logical positivism (or logical empiricism as it later came to be called) exerted near total dominance over the philosophy of science." (Suppe, 1977, 617) It is unclear just which three decades are intended, but perhaps it would be more accurate to say that it exerted dominance over what *we remember* of philosophy of science in those years. Among philoso-

phers it was a tiny and reviled minority on both sides of the Atlantic. And unless one simply defines philosophy of science as the logical empiricists and their descendants, it is not immediately clear that they were more dominant in the U.S. than elsewhere. Just where is this domination supposedly taking place? At Harvard, where C.I. Lewis hated the logical empiricists and Quine had supposedly destroyed them? At Chicago, which was firmly in the hands of Aristotelians that made Carnap's life miserable? (Carnap was unhappy there for eighteen years, but there was nowhere else to go.) The reason that we think them dominant is that theirs are the papers we still read, if only to disagree with. So much of the other work we have just forgotten. If one were to go through the country, department by department, editorial board by editorial board, and year by year one would find that they exercised far less power, by any measure, than they are supposed, in myth, to have done. I do not say that they were without influence, but if anyone wants to say that the logical empiricists have been wholly overthrown, that they are vastly diminished in numbers or power, then we need a more realistic estimate of what strength they once had.

We also need to know more about what logical empiricism *is*, that is, what its most central features are. Only then can we judge whether anything on the current scene is close enough to count as the same sort of thing. This problem is compounded by the fact that logical empiricism was a whole chorus of different voices not always singing in harmony. And like any long-lived group its members changed their minds in greater or lesser degrees over time. We thus have the same problem with logical empiricists that any biologist has with a species: there may be no essential qualities that all members of the group share. I propose to deal with this problem by discussing in this essay primarily only one representative figure, Carnap, and concentrate only on aspects of his views that changed relatively little in the last 35 years of his life. Carnap is undoubtedly a central figure, and very often when people speak of logical empiricism they mean Carnap.

Carnap started out his philosophic life as a neo-Kantian who was much impressed with science, especially relativity theory, and utterly bored by metaphysical disputation. This early neo-Kantianism has been better described by others (see esp. Friedman, 1987 and Richardson, 1992 and forthcoming). Here I want to highlight only the Kantian idea that all concepts, even those involved in observational judgments, are the result not merely of the world impinging on us in sensation but also of the active powers of the mind which shape or give form to our observational judgments and indeed to any intelligible claim that we might make. Carnap would hardly have

put things in just this way, but the view is entirely consistent with Carnap's thoroughgoing (if somewhat non-traditional) empiricism and even with his vigorously anti-metaphysical stance. The mix was probably unstable, however, at least until a major development of the 1930s.

What emerged then was a sophisticated conventionalism which might be summarized by saying that logic, epistemology, and for that matter all of those structural features which according to Kant were the result of imposition by the mind were better understood as conventions of language to be adjudicated on practical grounds (For a fuller exposition see Creath, 1992). Instead of one possible structure for the world of experience, we get many. Though Carnap is no Kantian in any strict sense at this point, the Kantian flavor still remains. This pragmatic conventionalism is so central to all the rest of Carnap's philosophy that it will be worthwhile to explore it a bit further. There are actually several different ways of approaching it; here I shall concentrate on the idea of justification.

Let's begin by assuming that some beliefs can be justified directly through observation. This much is non-controversial, even if which beliefs these are, how they are justified, and how much justification is thereby provided is not. Perhaps some other beliefs, not themselves directly observational, could be justified on this observational basis without the benefit of any intervening principles of inference. Given that such inferences have historically been controversial, one might reasonably wonder how to defend these inferential principles. Then there is logic, mathematics, and set theory whose objects are not only not observable; they are not even in the causal order with us. Furthermore, such epistemological claims as 'Observation is to be trusted.' cannot be justified by observation alone, on pain of circularity. And it is no improvement to suppose that observations A can justify observations B, and then B can justify A. Finally, there is a wide variety of claims, for example, *nothing is both red and green all over at the same time*, such that they might be justified to some degree by observation, but also such that we think we know them far better than we ever could on the basis of observation alone.

So from where else other than observation can we derive justification? The classical answer, from the Greeks through Descartes and even Frege and Russell, is intuition. Various terms were used for it, but the idea remained pretty much the same. Intuition is a supposed non-empirical source of news about independent matters of fact. Unfortunately intuitions disagree, and even if they do not there is no accounting for any agreement, at least not the causal sort of explanation that there can be for ordinary observation. Eventually we have to face the fact that an appeal to intuition is no justification

at all, but rather an admission that we have none.

So, if not intuition, what? In the early 1930s Carnap gave a new answer. It wasn't, of course entirely new. In the late nineteenth century Poincaré and Hilbert had each suggested something analogous for the special case of geometry. But Carnap was perhaps the first to make it fully general and rigorous. In any case, the "new" answer was quite simple. We could, he said, treat the basic axioms of say logic, or arithmetic, or epistemology (if we had one), not as the mysterious deliverance of a supposed faculty of intuition, but rather as *definitions* of the terms that they contain. To be sure, they would not be the sort of explicit definition that one might find in a dictionary: these new (implicit) definitions would not allow us to replace the defined term with some other expression. But that is okay. We don't expect the most basic vocabulary to be replaceable with something even more basic.

There are two important consequences of this axioms-as-definition idea: (1) Every judgment is relative to a set of definitions (to be called a language or a conceptual framework) – you cannot say anything without speaking some language or other, and the apparent disagreements between sets of axioms are only apparent rather than real: they are just defining terms in different ways. (2) The choice of a language is *conventional.* This implies that there are alternative sets of definitions that one could have chosen and that the choice among these alternatives is epistemically arbitrary. Of course, this is just as it should be. There can be no reason for thinking that one language is more likely to be true than another because a language is not the sort of thing that is either true or false. Once we have chosen a language we can say true or false things in it, but the language itself is neither true nor false. There is also another important reason why there can be no epistemic justification for choosing among these conceptual frameworks. Remember that among the beliefs that could not be justified by observation alone was epistemology itself. Indeed, the whole enterprise of justification presupposes commitments about what counts as a reason, how strong those reasons are, and what besides reasons (such as observation or definition) might justify a belief. In short, it presupposes an epistemology and by that I mean to include a philosophy of science. If the definitions are or include an epistemology, then to try to choose among definitions on epistemic grounds begs the very question at issue.

This is not to say, however, that all sets of definitions, languages, or conceptual frameworks are equally good. Some will be handier than others. As an extreme example a set of definitions might be inconsistent. If so, it will have every sentence (and its negation) as a consequence, and there would

be no way to distinguish what a theory says or predicts; it would say or predict everything. A theory in an inconsistent language would, in practical terms, be utterly useless. Conversely, a system without inductive rules would disallow prediction (and much else) and again be virtually useless. In between, a framework of definitions might be strong or weak in various ways both in what it will count as an observation and in what it will count as an acceptable argument. Thus, two theories from different frameworks cannot in general be evaluated by the same epistemic standard. Given the contingencies of this world and the contingencies of human interests and desires, these frameworks will be useful in varying degrees, and we may choose among them on such *pragmatic* grounds.

It is important to remember that it is not necessary to establish before a language can be used that it is especially useful. Just use it, and improve it if you can. After all, no question of truth arises in this choice of language. But once a language is adopted then the very conventions that constitute that language will guarantee that some sentences (say those of the language's logic) are justified. It is important to note also that while the whole epistemic structure is conventional (in the sense specified) this leaves plenty of room for objectivity. First, if a specific judgment is made, then the terms or concepts employed in that judgment are thereby specified and the language or epistemic structure which defines those terms or concepts is likewise specified. Thus, for any clear judgment there is a uniquely correct standard of evaluation. Second, while we are largely free to choose what language and concepts we will use in describing the world, even this choice is constrained, as noted earlier, by important pragmatic considerations.

There is an a priori in this system, but in calling it conventional Carnap emphasizes that it is a relativized and revisable a priori. In saying that changes are to be made on pragmatic grounds, Carnap is making the a priori sensitive to contingent matters of fact and hence to the empirical evidence we have. Carnap's approach to philosophy is, thus, highly naturalistic especially in comparison to most philosophers of his day. Unlike some of our contemporaries, however, he did recognize that one could carry naturalism too far.

If, as Carnap says, there are choices to be made on the usual philosophic topics, then it would be well to see what choices Carnap recommends. Concerning observation, Carnap departs from traditional empiricism, exemplified by Descartes, Berkeley, and Russell, and urges that we take direct observation to be about physical objects rather than about sensory experiences. Moreover, Carnap recognizes that these observations are far from certain

and that as theories change the very meaning of observational reports can change as well. Without a neutral observation language, translation from one framework to another is bound to be inexact (Carnap, 1936, 126). As far as theories are concerned, Carnap was a scientific realist (Creath, 1985), though in today's terms his realism would be called an internal rather than a metaphysical one. He was a methodological behaviorist, not a philosophic one, and that combined with his scientific realism would yield a version of functionalism in the philosophy of mind. All of these Carnapian views are still controversial, but it is difficult to escape the idea that Carnap would have felt very much at home in today's philosophic climate.

Let us turn then to compare this very brief sketch of Carnap's views with sketches (also brief) of the views of the two writers who are supposed to have demolished him, Quine and Kuhn. In neither case do I wish to deny that there are differences from Carnap, but I want to reflect first on the similarities. As far as Quine is concerned, he certainly shares Carnap's physicalism and falliblism with respect to direct observation. Quine is famous for holding that logic and mathematics are revisable in the light of empirical evidence. This is usually presented as a criticism of Carnap, but consider this remark of Carnap's:

> No rule of the physical 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 but also to the L-rules, including those of mathematics. In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. (Carnap, 1934, 318)

Plainly this is a view that Quine and Carnap share. Quine is also famous for an indeterminacy of translation thesis first published in 1958 (Quine, 1958, 1-5). Again this is something that they share, for Carnap had in 1947 published a version of the indeterminacy of translation thesis strikingly like Quine's (Carnap, 1947, 100-06; Creath, 1994; Berge, 1991, 29-38). Quine has championed a number of indeterminacies, but Carnap would not object, for indeterminacy is just what conventionalism becomes when translated into Quine's idiom. Quine is also a famous holist with respect to theory testing, but consider this passage from Carnap:

> 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 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). (Carnap, 1934, 318)

Now holism of a broad and general sort is compatible with an uneven distribution of cognitive praise and blame. An account of such an uneven distribution would be one form of a probability theory, and Carnap spent much effort over the last three decades of his life to develop such a probability theory. Since that time probability theory has become a major philosophic industry, but it must be admitted that here is one major difference between Carnap and Quine. Rather than making a similar attempt, Quine's remarks about probability are dismissive (Quine, 1951, 41-2). It remains to be seen whether this is a result of lack of interest on Quine's part or whether there is something within Quine's general views which would make an account of degrees of confirmation especially difficult or even impossible. Finally, there is one point of obvious disagreement between Quine and Carnap, one that is obviously important as well. Carnap's distinction between the analytic and the synthetic is central to his view, and Quine rejects it. There are two things to note here. First, this difference of opinion did not prevent them from agreeing on a wide variety of other topics as we have already seen. Second, despite my qualms about speaking of the similarity of theories (Creath, 1989, forthcoming), if such a notion makes sense, then Quine and Carnap are more similar to each other than either is to the other major movements of the day: Wittgensteinianism, ordinary language philosophy, Cartesianism, or Aristotelianism, not to mention existentialism, phenomenology, and other forms of continental philosophy. Indeed, Quine is closer to Carnap than even some of Carnap's closest friends in the Vienna Circle. Neurath did not accept the analytic-synthetic distinction either, but then he did not accept the notions of truth or reference or much of anything else. This is not the place to discuss Neurath, but the judgment of the 1930s that he was closer in view to Carnap than was anyone else in the Circle is not unreasonable. And Quine's view, if anything, falls somewhere between Carnap's and Neurath's. And that is not far enough away to put Quine wholly outside the logical empiricist group.

But what about Kuhn? Here too we find a surprising amount of similarity with Carnap. Both offer us a two-tier system of scientific commitments.

The more general tier is called a paradigm by one and a language or conceptual framework by the other. By whichever name, it determines what counts as an observation or as justifying argument. For both it determines holistically the meaning of all terms whether observational or theoretical. This entails that there are no neutral observations as between competing paradigms/conceptual frameworks, but rather each will see the world differently and will see it as conforming to and thus confirming its own approach. Thus, Carnap and Kuhn share theses of meaning change and incommensurability. Moreover, the appraisal of commitments at the general level is pragmatic or instrumental, and no sense can be made of its getting closer to the truth. At the less general level, theories can be appraised more or less straightforwardly because questions of evidence, argument, and meaning have already been settled. Naturally, commitments at both levels are open to revision, though what those revisions come to are very different in the two cases. At the narrower level it is just a change of doctrine, but at the paradigm/conceptual framework level changes involve changes of meaning that can profoundly alter the character of what we thought we already knew (Carnap, 1963, 921). In recent years Kuhn has come to describe his own view as "Kantianism with moveable categories" (Kuhn, 1990). As we have seen, this phrase is also an apt description of Carnap's view.

Of course, there *are* some differences between Kuhn and Carnap, but on inspection many of them turn out to be differences of emphasis and interest rather than substantive disagreements. Of these the two most prominent might be denominated Kuhn's Wittgensteinianism and his sociological turn. Carnap regularly tries to make the rules of a conceptual framework explicit, but Kuhn insists that science proceeds implicitly, by means of examples and inarticulate analogy. This is a point that Wittgensteinians have made about language generally. There is no real disagreement here. Carnap also insists that ordinary language (and by extension the ordinary practice of science) is a vague affair with only implicit rules. Perhaps Carnap is more optimistic about the possibility of eventually making the implicit explicit, but remember that insofar as Kuhn can make his case that the paradigm governs the work done within it he must be correspondingly explicit. The scientist may not have to say what the rules are and how they apply, but the Kuhnian historian does.

Kuhn's sociological turn, at least from Carnap's point of view, is neither a disagreement nor a difference of emphasis. It is rather a change of subject. Describing accurately how and for what causes the sciences develop is a task that philosophers as philosophers are ill-equipped to perform, though some

people who are philosophers have done the historical and sociological work quite well. In any case the outcome of that inquiry would be enormously interesting and important to Carnap. Given the similarity between Carnap's and Kuhn's accounts of theory, of observation, and of the relation between them, perhaps the issue of whether we are dealing with one subject or with two interrelated ones can recede into secondary importance.

Besides the rarely acknowledged similarity of doctrine between Carnap and Kuhn, there is also a historical connection between the two men that is perhaps unexpected (Reich, 1991). For one thing, Kuhn's *The Structure of Scientific Revolutions* (which is supposed to have demolished logical empiricism) appeared as a volume of the *International Encyclopedia of Unified Science*, which was the house organ of the logical empiricists. Moreover, Carnap was not only the founder and editor of the *Encyclopedia*, he was the editor specifically of Kuhn's volume. And he loved the book. In view of our earlier considerations this is hardly surprising. When he wrote to Kuhn in officially accepting it, Carnap specifically drew the parallels between Kuhn's view and his own, emphasized that he considered his own philosophic work as a tool, and hoped that Kuhn's work would give this conception wider currency. Well, Kuhn's ideas certainly became famous, even if their connection and similarity to Carnap's did not. Note that my point is not a sterile one about priority; both of these men are original. The point is, rather, to get away from the usual picture of these men as polar opposites; the connections between them, both doctrinal and historical, are real and important.

So far we have noticed that two of the supposed causes of the demise of logical empiricism, Quine and Kuhn, are themselves unexpectedly close to central examples of that movement. It remains only to note that the third "cause" previously mentioned is of a sort different from the first two. This is the change from treating logical empiricism as a movement to treating it as a doctrine. This, however, represents not a change in logical empiricism, but rather in how we viewed it. It is not, by itself, even a change in how we do philosophy. A change in historical perspective could, at least in principle, have profound effects on subsequent work, but the case still needs to be made that it did. If all the birds in the world decided that they were wholly unlike dinosaurs that would not make it so. In order to decide whether these birds were right we would still have to discover what the dinosaurs were really like and then weigh the obvious differences against the perhaps newly uncovered similarities. Certainly the views, described above, which were most central to Carnap's philosophy, have a contemporary feel as well as major defenders today.

Let us return then to the original questions about dinosaurs. What hid the similarity of the ancient dinosaurs to birds, and thus the possibility of persistence for dinosaurs, is that we badly misunderstood what dinosaurs were like. They turn out to have been warm-blooded, light on their feet, and more intelligent than we suspected. The dinosaurs were different from birds, but not in the most important respects. So it is with the so-called dinosaurs of the philosophic world. They, too, have been badly misunderstood (and in the relevant respects warm-blooded, light on their feet, and intelligent). Also like their ancient counterparts, the logical empiricists showed a wide variety of modes of (philosophic) life, so that if some figure now differs in some important respect from some figure then, this is well within the range of variation originally exhibited. Like species, ideas evolve, but how fast and in which ways they have in fact evolved can be hidden from us. Many of the ideas of the logical empiricists are more popular now than ever before. This holds even when we do not recognize the ancestry of our ideas and suppose them utterly new and devastating to what has gone before.

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