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Introduction

A New Kind of Philosophy

Some people think that philosophy never makes progress. In fact, professional philosophers might think that more frequently – and feel it more acutely – than anyone else. At the beginning of the twentieth century, some philosophers were so deeply troubled that they decided to cast all previous philosophy on the scrap heap and to rebuild from scratch. "Why shouldn't philosophy be like science?" they asked. "Why can't it also make genuine progress?"

Now, you might guess that these philosophers would have located philosophy's problems in its lack of empirical data and experiments. One advantage of the empirical sciences is that bad ideas (such as "leeches cure disease") can be falsified through experiments. However, this wasn't the diagnosis of the first philosophers of science; they didn't see empirical testability as the sine qua non of a progressive science. Their guiding light was not the empirical sciences, but mathematics, and mathematical physics.

The nineteenth century had been a time of enormous progress in mathematics, not only in answering old questions and extending applications, but but also in clarifying and strengthening the foundations of the discipline. For example, George Boole had clarified the structure of logical relations between propositions, and Georg Cantor had given a precise account of the concept of "infinity," thereby setting the stage for the development of the new mathematical theory of sets. The logician Gottlob Frege had proposed a new kind of symbolic logic that gave a precise account of all the valid argument forms in mathematics. And the great German mathematician David Hilbert, building on a rich tradition of analytic geometry, proposed an overarching axiomatic method in which all mathematical terminology is "de-interpreted" so that the correctness of proofs is judged on the basis of purely formal criteria.

For a younger generation of thinkers, there was a stark contrast between the ever more murky terminology of speculative philosophy and the rising standards of clarity and rigor in mathematics. "What is the magic that these mathematicians have found?" asked some philosophically inclined scientists at the beginning of the twentieth century. "How is it that mathematicians have a firm grip on concepts such as 'infinity' and 'continuous function,' while speculative philosophers continue talking in circles?" It was time, according to this new generation, to rethink the methods of philosophy as an academic discipline.

The first person to propose that philosophy be recreated in the image of nineteenth-century mathematics was Bertrand Russell. And Russell was not at all modest in what he thought this new philosophical method could accomplish. Indeed, Russell cast himself as a direct competitor with the great speculative philosophers, most notably with Hegel. That is, Russell thought that, with the aid of the new symbolic logic, he could describe the fundamental structure of reality more clearly and accurately than Hegel himself did. Indeed, Russell's "logical atomism" was intended as a replacement for Hegel's monistic idealism.

Russell's grand metaphysical ambitions were cast upon the rocks by his student Ludwig Wittgenstein. In essence, Wittgenstein's *Tractatus Logico Philosophicus* was intended to serve as a *reductio ad absurdum* of the idea that the language of mathematical logic is suited to mirror the structure of reality in itself. To the extent that Russell himself accepted Wittgenstein's rebuke, this first engagement of philosophy and mathematical logic came to an unhappy end. In order for philosophy to become wedded to mathematical logic, it took a distinct second movement, this time involving a renunciation of the ambitions of traditional speculative metaphysics. This second movement proposed not only a new method of philosophical inquiry but also a fundamental reconstrual of its aims.

As mentioned before, the nineteenth century was a golden age for mathematics in the broad sense, and that included mathematical physics. Throughout the century, Newtonian physics has been successfully extended to describe systems that had not originally been thought to lie within its scope. For example, prior to the late nineteenth century, changes in temperature had been described by the science of thermodynamics, which describes heat as a sort of continuous substance that flows from one body to another. But then it was shown that the predictions of thermodynamics could be reproduced by assuming that these bodies are made of numerous tiny particles obeying the laws of Newtonian mechanics. This reduction of thermodynamics to statistical mechanics led to much philosophical debate over the existence of unobservable entities, e.g., tiny particles (atoms) whose movement is supposed to explain macroscopic phenomena such as heat. Leading scientists such as Boltzmann, Mach, Planck, and Poincaré sometimes took opposing stances on these questions, and it led to more general reflection on the nature and scope of scientific knowledge.

These scientists couldn't have predicted what would happen to physics at the beginning of the twentieth century. The years 1905–1915 saw no fewer than three major upheavals in physics. These upheavals began with Einstein's publication of his special theory of relativity, and continued with Bohr's quantum model of the hydrogen atom, and then Einstein's general theory of relativity. If anything became obvious through these revolutions, it was that we didn't understand the nature of science as well as we thought we did. We had believed we understood how science worked, but people like Einstein and Bohr were changing the rules of the game. It was high time to reflect on the nature of the scientific enterprise as a whole.

The new theories in physics also raised further questions, specifically about the role of mathematics in physical science. All three of the new theories – special and general relativity, along with quantum theory – used highly abstract mathematical notions, the likes of which physicists had not used before. Even special relativity, the most intuitive of the three theories, uses four-dimensional geometry and a notion of "distance" that takes both positive and negative values. Things only got worse when, in the 1920s, Heisenberg proposed that the new quantum theory make use of non-commutative algebras that had no intuitive connection whatsoever to things happening in the physical world.

The scientists of the early twentieth century were decidedly philosophical in outlook. Indeed, reading the reflections of the young Einstein or Bohr, one realizes that the distinction between "scientist" and "philosopher" had not yet been drawn as sharply as it is today. Nonetheless, despite their philosophical proclivities, Einstein, Bohr, and the other scientific greats were not philosophical system builders, if only because they were too busy publicizing their theories and then working for world peace. Thus, the job of "making sense of how science works" was left to some people who we now consider to be philosophers of science.

If we were to call anybody the first "philosopher of science" in the modern sense of the term, then it should probably be **Moritz Schlick** (1882–1936). Schlick earned his PhD in physics at Berlin under the supervision of Max Planck and thereafter began studying philosophy. During the 1910s, Schlick became one of the first philosophical interpreters of Einstein's new theories, and in doing so, he developed a distinctive view in opposition to Marburg neo-Kantianism. In 1922, Schlick was appointed chair of *Naturphilosophie* in Vienna, a post that had earlier been held by Boltzmann and then by Mach.

When Schlick formulated his epistemological theories, he did so in a conscious attempt to accommodate the newest discoveries in mathematics and physics. With particular reference to mathematical knowledge, Schlick followed nineteenth-century mathematicians – most notably Pasch and Hilbert – in saying that mathematical claims are true by definition and that the words that occur in the axioms are thereby implicitly defined. In short, those words have no meaning beyond that which accrues to them by their role in the axioms.

While Schlick was planting the roots of philosophy of science in Vienna, the young **Hans Reichenbach** (1891–1953) had found a way to combine the study of philosophy, physics, and mathematics by moving around between Berlin, Göttingen, and Munich – where he studied philosophy with Cassirer, physics with Einstein, Planck, and Sommerfeld; and mathematics with Hilbert and Noether. He struggled at first to find a suitable academic post, but eventually Reichenbach was appointed at Berlin in 1926. It was in Berlin that Reichenbach took on a student named Carl Hempel (1905–1997), who would later bring this new philosophical approach to the elite universities in the United States. Hempel's students include several of the major players in twentieth-century philosophy of science, such as Adolf Grünbaum, John Earman, and Larry Sklar. Reichenbach himself eventually relocated to UCLA, where he had two additional students of no little renown: Wesley Salmon and Hilary Putnam.

However, back in the 1920s, shortly before he took the post at Berlin, Reichenbach had another auspicious meeting at a philosophy conference in Erlangen. Here he met a young man named Rudolf Carnap who, like Reichenbach, found himself poised at the intersection of philosophy, physics, and mathematics. Reichenbach introduced Carnap to his friend Schlick, the latter of whom took an eager interest in Carnap's ambition to develop a "scientific philosophy." A couple of short years later, Carnap was appointed assistant professor of philosophy in Vienna – and so began the marriage between mathematical logic and philosophy of science.

Carnap

Having been a student of Frege's in Jena, Rudolf Carnap (1891–1970) was an early adopter of the new logical methods. He set to work immediately trying to employ these methods in the service of a new style of philosophical inquiry. His first major work – *Der Logische Aufbau der Welt* (1928) – attempted the ultra-ambitious project of constructing all scientific concepts out of primitive (fundamental) concepts. What is especially notable for our purposes was the notion of *construction* that Carnap employed, for it was a nearby relative to the notion of *logical construction* that Russell had employed, and which descends from the mathematician's idea that one kind of mathematical object (e.g., real numbers) can be constructed from another kind of mathematical object (e.g., natural numbers). What's also interesting is that Carnap takes over the idea of *explication*, which arose in mathematical contexts – e.g., when one says that a function f is "continuous" just in case for each $\epsilon > 0$, there is a $\delta > 0$ such that ...

When assessing philosophical developments such as these, which are so closely tied to developments in the exact sciences, we should keep in mind that ideas that are now clear to us might have been quite opaque to our philosophical forebears. For example, these days we know quite clearly what it means to say that a theory T is complete. But to someone like Carnap in the 1920s, the notion of completeness was vague and hazy, and he struggled to integrate it into his philosophical thoughts. We should keep this point in mind as we look toward the next stage of Carnap's development, where he attempted a purely "syntactic" analysis of the concepts of science.

In the late 1920s, the student Kurt Gödel (1906–1978) joined in the discussions of the Vienna circle, and Carnap later credited Gödel's influence for turning his interest to questions about the language of science. Gödel gave the first proof of the completeness of the predicate calculus in his doctoral dissertation (1929), and two years later, he obtained his famous incompleteness theorem, which shows that there is some truth of arithmetic that cannot be derived from the first-order Peano axioms.

In proving incompleteness, Gödel's technique was "metamathematical" – i.e., he employed a theory M *about* the first-order theory T of arithmetic. Moreover, this metatheory M employed purely syntactic concepts – e.g., the length of a string of symbols, or the number of left parentheses in a string, or being the last formula in a valid proof that begins from the axioms of arithmetic. This sort of approach proved to be fascinating for Carnap, in particular, because it transformed questions that seemed hopelessly vague and "philosophical" into questions that were tractable – and indeed tractable by means of the very methods that scientists themselves employed. In short, Gödel's approach indicated the possibility of an exact science of the exact sciences.

And yet, Gödel's inquiry was restricted to one little corner of the exact sciences: arithmetic. Carnap's ambitions went far beyond elementary mathematics; he aspired to apply these new methods to the entire range of scientific theories, and especially the new theories of physics. Nonetheless, Carnap quickly realized that he faced additional problems beyond those faced by the metamathematician, for scientific theories – unlike their mathematical cousins – purport to say something *contingently true* – i.e., something that could have been otherwise. Hence, the logical approach to philosophy of science isn't finished when one has analyzed a theory T qua mathematical object; one must also say something about how T latches on to empirical reality.

Carnap's first attempts in this direction were a bit clumsy, as he himself recognized. In the 1920s and 1930s, philosophers of science were just learning the basics of formal logic. It would take another forty years until "model theory" was a well-established discipline, and the development of mathematical logic continues today (as we hope to make clear in this book). However, when mathematical logic was still in its infancy, philosophers often tried the "most obvious" solution to their problems – not realizing that it couldn't stand up to scrutiny. Consider, for example, Carnap's attempt to specify the empirical content of a theory T. Carnap proposes that the vocabulary Σ in which a theory T is formulated must include an empirical subvocabulary $O \subseteq \Sigma$, in which case the empirical content of T can be identified with the set $T|_O$ of consequences of T restricted to the vocabulary O. Similarly, in attempting to cash out the notion of "reduction" of one theory to another, Carnap initially said that the concepts of the reduced theory needed to be explicitly defined in terms of the concepts of the reducing theory – not realizing that he was thereby committing to a far more narrow notion of reduction than was being used in the sciences.

In Carnap's various works, however, we do find the beginnings of an approach that is still relevant today. Carnap takes a "language" and a "theory" to be objects of his inquiries, and he notes explicitly that there are choices to be made along the way. So, for example, the classical mathematician chooses a certain language and then adopts certain transformation rules. In contrast, the intuitionistic mathematician chooses a different language and adopts different transformation rules. Thus, Carnap allows himself to ascend semantically – to look at scientific theories from the outside, as it were. From this vantage point, he is no longer asking the "internal questions" that the theorist herself is asking. He is not asking, for example, whether there is a greatest prime number. Instead, the philosopher of science is raising "external questions" – i.e., questions about the theory T, and especially those questions that have precise syntactic formulations. For example, Carnap proposes that the notion of a sentence's being "analytic relative to T" is an external notion that we metatheorists use to describe the structure of T.

The twentieth-century concern with analytic truth didn't arise in the seminar rooms of philosophy departments – or at least not in philosophy departments like the ones of today. In fact, this concern began rather with nineteenth-century geometers, faced with two parallel developments: (1) the discovery of non-Euclidean geometries, and (2) the need to raise the level of rigor in mathematical arguments. Together, these two developments led mathematical language to be disconnected from the physical world. In other words, one key outcome of the development of modern mathematics was the *de-interpretation* of mathematical terms such as "number" or "line." These terms were replaced by symbols that bore no intuitive connection to external reality.

It was this de-interpretation of mathematical terms that gave rise to the idea that analytic truth is *truth by postulation*, the very idea that was so troubling to Russell, and then to Quine. But in the middle of the nineteenth century, the move that Russell called "theft" enabled mathematicians to proceed with their investigations in absence of the fear that they lacked insight into the meanings of words such as "line" or "continuous function." In their view, it didn't matter what words you used, so long as you clearly explained the rules that governed their use. Accordingly, for leading mathematicians such as Hilbert, mathematical terms such as "line" mean nothing more nor less than what axioms say of them, and it's simply impossible to write down false mathematical postulates. There is no external standard against which to measure the truth of these postulates.

It's against this backdrop that Carnap developed his notion of analytic truth in a framework; and that Quine later developed his powerful critique of the analytic–synthetic distinction. However, Carnap and Quine were men of their time, and their thoughts operated at the level of abstraction that science had reached in the 1930s. The notion of logical metatheory was still in its infancy, and it had hardly dawned on logicians that "frameworks" or "theories" could themselves be treated as objects of investigation.

Quine

If one was a philosophy student in the late twentieth century, then one learned that Quine "demolished" logical positivism. In fact, the errors of positivism were used as classroom lessons in how not to commit the most obvious philosophical blunders. How silly to state a view that, if true, entails that one cannot justifiably believe it!

During his years as an undergraduate student at Oberlin, **Willard van Orman Quine** (1908–2000) had become entranced with Russell's mathematical logic. After getting his PhD from Harvard in 1932, Quine made a beeline for Vienna just at the time that Carnap was setting his "logic of science" program into motion. Quine quickly became Carnap's strongest critic. As the story is often told, Quine was single-handedly responsible for the demise of Carnap's program, and of logical positivism more generally.

Of course, Quine was massively influential in twentieth-century philosophy – not only for the views he held, but also via the methods he used for arriving at those views. In short, the Quinean methodology looks something like this:

1. One cites some theorem ϕ in logical metatheory.
2. One argues that ϕ has certain philosophical consequences, e.g., makes a certain view untenable.

Several of Quine's arguments follow this pattern, even if he doesn't always explicitly mention the relevant theorem from logical metatheory. One case where he is explicit is in his 1940 paper with Nelson Goodman, where he "proves" that every synthetic truth can be converted to an analytic truth. Whatever one may think of Quine's later arguments against analyticity, there is no doubt, historically speaking, that this metatheoretical result played a role in Quine's arriving at the conclusion that there is no analytic–synthetic distinction. And it would only be reasonable to think that *our* stance on the analytic–synthetic distinction should be responsive to what this mathematical result can be supposed to show.

As the story is typically told, Quine's "Two Dogmas of Empiricism" dealt the death blow to logical positivism. However, Carnap presented Quine with a moving target, as his views continued to develop. In "Empiricism, Semantics, and Ontology" (1950), Carnap further developed the notion of a *framework*, which bears striking resemblances both to the notion of a *scientific theory* and, hence, to the notion of a theory T in first-order logic. Here Carnap distinguishes two types of questions – the questions that are *internal* to the framework and the questions that are *external* to the framework. The internal questions are those that can be posed in the language of the framework and for which the framework can (in theory) provide an answer. In contrast, the external questions are those that we ask *about* a framework.

Carnap's abstract idea can be illustrated by simple examples from first-order logic. If we write down a vocabulary Σ for a first-order language, and a theory T in this vocabulary, then a typical internal question might be something like, "Does anything satisfy the predicate $P(x)$?" In contrast, a typical external question might be, "How many predicate symbols are there in Σ?" Thus, the internal–external distinction corresponds roughly to the older distinction between object language and metalanguage that frames Carnap's discussion in *Logische Syntax der Sprache* (1934).

The philosophical point of the internal–external distinction was supposed to be that one's answers to external questions are not held to the same standards as one's answers to internal questions. A framework includes rules, and an internal question should be answered in accordance with these rules. So, to take one of Carnap's favorite examples, "Are there numbers?" can naturally construed as an external question, since no mathematician is actively investigating that question. This question is *not* up for grabs in mathematical science – instead, it's a presupposition of mathematical science. In contrast, "Is there a greatest prime number?" is internal to mathematical practice; i.e., it is a question to which mathematics aspires to give an answer.

Surely most of us can grasp the intuition that Carnap is trying to develop here. The external questions must be answered in order to set up the game of science; the internal questions are answered in the process of playing the game of science. But Carnap wants to push this idea beyond the intuitive level – he wants to make it a cornerstone of his theory of knowledge. Thus, Carnap says that we may single out a certain special class of predicates – the so-called *Allwörter* – to label a domain of inquiry. For example, the number theorist uses the word "number" to pick out her domain of inquiry – she doesn't investigate whether something falls under the predicate "x is a number." In contrast, a number theorist might investigate whether there are numbers x, y, z such that $x^3 + y^3 = z^3$; and she simply doesn't consider whether some other things, which are not themselves numbers, satisfy this relation.

Quine (1951a, 1960) takes up the attack against Carnap's internal–external distinction. While Quine's attack has several distinct maneuvers, his invocation of hard logical facts typically goes unquestioned. In particular, Quine appeals to the supposedly hard logical fact that every theory in a language that has several distinct quantifiers (i.e., many-sorted logic) is equivalent to a theory in a language with a single unrestricted quantifier.

> It is evident that the question whether there are numbers will be a category question only with respect to languages which appropriate a separate style of variables for the exclusive purpose of referring to numbers. If our language refers to numbers through variables that also take classes other than numbers as values, then the question whether there are numbers becomes a subclass question . . . Even the question whether there are classes, or whether there are physical objects becomes a subclass question if our language uses a single style of variables to range over both sorts of entities. Whether the statement that there are physical objects and the statement that there are black swans should be put on the same side of the dichotomy, or on opposite sides, comes to depend upon the rather trivial consideration of whether we use one style of variables or two for physical objects and classes. (Quine, 1976, p. 208)

Thus, suggests Quine, there is a metatheoretical result – that a many-sorted theory is equivalent to a single-sorted theory – that destroys Carnap's attempt to distinguish between *Allwörter* and other predicates in our theories.

We won't weigh in on this issue here, in our introduction. It would be premature to do so, because the entire point of this book is to lay out the mathematical facts in a clear fashion so that the reader can judge the philosophical claims for herself.

In "Two Dogmas of Empiricism," Quine argues that it makes no sense to talk about a statement's admitting of confirming or infirming (i.e., disconfirming) instances, at least when taken in isolation. Just a decade later, **Hilary Putnam**, in his paper "What Theories Are Not" (Putnam, 1962) applied Quine's idea to entire scientific theories. Putnam, student of the ur-positivist Reichenbach, now turns the positivists' primary weapon against them, to undercut the very distinctions that were so central to their program. In this case, Putnam argues that the set $T|_O$ of "observation sentences" does not accurately represent a theory T's empirical content. Indeed, he argued that a scientific theory cannot properly be said to have empirical content and, hence, that the warrant for believing it cannot flow from the bottom (the empirical part) to the top (the theoretical part). The move here is paradigmatic Putnam: a little bit of mathematical logic deftly invoked to draw a radical philosophical conclusion. This isn't the last time that we will see Putnam wield mathematical logic in the service of a far-reaching philosophical claim.

The Semantic Turn

In the early 1930s, the Vienna circle made contact with the group of logicians working in Warsaw, and in particular with **Alfred Tarski** (1901–1983). As far as twentieth-century analytic philosophy is concerned, Tarski's greatest influence has been through his bequest of **logical semantics**, along with his explications of the notions of **structure** and **truth in a structure**. Indeed, in the second half of the twentieth century, analytic philosophy has been deeply intertwined with logical semantics, and ideas from model theory have played a central role in debates in metaphysics, epistemology, philosophy of science, and philosophy of mathematics.

The promise of a purely syntactic metatheory for mathematics fell into question already in the 1930s when Kurt Gödel proved the incompleteness of Peano arithmetic. At the time, a new generation of logicians realized that not all interesting questions about theories could be answered merely by looking at theories "in themselves", and without relation to other mathematical objects. Instead, they claimed, the interesting questions about theories include questions about how they might relate to antecedently understood mathematical objects, such as the universe of sets. Thus was born the discipline of logical semantics. The arrival of this new approach to metatheory was heralded by Alfred Tarski's famous definitions of "truth in a structure" and "model of a theory." Thus, after Tarski, to understand a theory T, we have more than the theory qua syntactic object, we also have a veritable universe $\mathrm{Mod}(T)$ of models of T.

Bas van Fraassen was one of the earliest adopters of logical semantics as a tool for philosophy of science, and he effectively marshaled it in developing an alternative to the dominant outlook of scientific realism. Van Fraassen ceded Putnam's argument that the empirical content of a theory cannot be isolated syntactically. And then, in good philosophical fashion, he transformed Putnam's modus ponens into a modus tollens: the problem is not with empirical content, per se, but with the attempt to explicate is syntactically. Indeed, van Fraassen claimed that one needs the tools of logical semantics in order to make sense of the notion of empirical content; and equipped with this new explication of empirical content, empiricism can be defended against scientific realism. Thus, both the joust and the parry were carried on within an explicitly metalogical framework.

Since the 1970s, philosophical discussions of science have been profoundly influenced by this little debate about the place of syntax and semantics. Prior to the criticisms – by Putnam, van Fraassen, et al. – of the "syntactic view of theories" philosophical discussions of science frequently drew upon new results in mathematical logic. As was pointed out by van Fraassen particularly, these discussions frequently degenerated, as philosophers found themselves hung up on seemingly trivial questions, e.g., whether the observable consequences of a recursively axiomatized theory are also recursively axiomatizable. Part of the shift from syntactic to semantic methods was supposed to be a shift toward a more faithful construal of science in practice. In other words, philosophers were supposed to start asking the questions that arise in the practice of science, rather than the questions that were suggested by an obsessive attachment to mathematical logic.

The move away from logical syntax has had some healthy consequences in terms of philosophers engaging more closely with actual scientific theories. It is probably not a coincidence that since the fall of the syntactic view of theories, philosophers of science have turned their attention to specific theories in physics, biology, chemistry, etc. As was correctly pointed out by van Fraassen, Suppes, and others, scientists themselves don't demand first-order axiomatizations of these theories – and so it would do violence to those theories to try to encode them in first-order logic. Thus, the demise of the syntactic view allowed philosophers to freely draw upon the resources of set-theoretic structures, such as topological spaces, Riemannian manifolds, Hilbert spaces, C^*-algebras, etc.

Nonetheless, the results of the semantic turn have not been uniformly positive. For one, philosophy of science has seen a decline in standards of rigor, with the unfortunate consequence that debating parties more often than not talk past each other. For example, two philosophers of science might take up a debate about whether isomorphic models represent the same or different possibilities. However, these two philosophers of science may not have a common notion of "model" or of "isomorphism." In fact, many philosophers of science couldn't even tell you a precise formal explication of the word "isomorphism" – even though they rely on the notion in many of their arguments. Instead, their arguments rely on some vague sense that isomorphisms preserve structure, and an even more vague sense of what structure is.

In this book, we'll see many cases in point, where a technical term from science (physics, math, or logic) has made its way into philosophical discussion but has then lost touch with its technical moorings. The result is almost always that philosophers add to the stock of confusion rather than reducing it. How unfortunate it is that philosophy of science has fallen into this state, given the role we could play as prophets of clarity and logical rigor. One notable instance where philosophers of science could help increase clarity is the notion of *theoretical equivalence*. Scientists, and especially physicists, frequently employ the notion of two theories being equivalent. Their judgments about equivalence are not merely important for their personal attitudes toward their theories, but also for determining their actions – e.g., will they search for a crucial experiment to determine whether T_1 or T_2 is true? For example, students of classical mechanics are frequently told that the Lagrangian and Hamiltonian frameworks are equivalent, and on that basis, they are discouraged from trying to choose between them.

Now, it's not that philosophers don't talk about such issues. However, in my experience, philosophers tend to bring to bear terminology that is alien to science, and which sheds no further light on the problems. For example, if an analytic philosopher is asked, "when do two sentences ϕ and ψ mean the same thing?" then he is likely to say something like, "if they pick out the same proposition." Here the word "proposition" is alien to the physicist; and what's more, it doesn't help to solve real-life problems of synonymy. Similarly, if an analytic philosopher is asked, "when do two theories T_1 and T_2 say the same thing?" then he might say something like, "if they are true in the same possible worlds." This answer may conjure a picture in the philosopher's head, but it won't conjure any such picture in a physicist's head – and even if it did, it wouldn't help decide controversial cases. We want to know whether Lagrangian mechanics is equivalent to Hamiltonian mechanics, and whether Heisenberg's matrix mechanics is equivalent to Schrödinger's wave mechanics. The problem here is that space of possible worlds (if it exists) cannot be surveyed easily, and the task of comparing the subset of worlds in which T_1 is true with the subset of worlds in which T_2 is true is hardly tractible. Thus, the analytic philosopher's talk about "being true in the same possible worlds" doesn't amount to an *explication* of the concept of equivalence. An explication, in the Carnapian sense, should supply clear guidelines for how to use a concept.

Now, don't get me wrong. I am not calling for a Quinean ban on propositions, possible worlds, or any of the other concepts that analytic philosophers have found so interesting. I only want to point out that these concepts are descendants, or cousins, of similar