Michael Friedman
*Philosophical Naturalism*

Stanley Cavell
*Something Out of the Ordinary*

Henry E. Allison
*We Can Act Only Under the Idea of Freedom*
I want to discuss a tendency of thought which has been extremely widespread within Anglo-American philosophy during the last twenty years or so—but which now, if I am not mistaken, has reached the end of its useful life. This tendency of thought, which I will call “philosophical naturalism,” is characterized by two main ideas. The first is the rejection of any special status for types of knowledge traditionally thought to be a priori—knowledge in logic and mathematics, for example—in that all knowledge whatsoever is now conceived as having fundamentally the same status as that found in the empirical natural sciences. Thus Michael Devitt, in a recent book devoted to what he calls a “naturalistic” program in semantics and philosophy of language, defines naturalism as the view that “there is only one way of knowing, the empirical way that is the basis of science,” so that “from a naturalistic perspective, we should deny that there is any a priori knowledge.”¹ Lying behind this view, as Devitt makes abundantly clear, is an holistic picture of the relationship between knowledge and experience now associated with the names of Duhem and Quine. The totality of human knowledge is pictured as a vast web of interconnected beliefs on which experience or sensory input impinges only along the periphery. When faced with a “recalcitrant experience” standing in conflict with our overall system of beliefs we then have a choice of where to make revisions. These can be made relatively close to the periphery of the system (in which case we make a change in a relatively low-level part of natural science), but they can also—when the conflict is particularly acute, for example—affect the most abstract and general parts of science, including even the truths of logic and mathematics, lying at the center of our system of beliefs. To be sure, such high-level beliefs at the center of our system are relatively entrenched, in that we are relatively reluctant to revise them or to give them up. Nevertheless, and this is the crucial point, no belief whatsoever is forever “immune to revision.”²

The second main idea of what I am calling philosophical naturalism is the view that philosophy, as a discipline, is also best understood as simply one more part—perhaps a peculiarly abstract and general part—of empirical natural science. Thus David Papineau, in a recent book entitled *Philosophical Naturalism*, characterizes such naturalism as the view that we should “set philosophy within science,” so that philosophical investigation as such “is best conducted within the framework of our empirical knowledge of the world.”³ And Quine’s program of “epistemology naturalized,” whereby “epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science,”⁴ provides the best known example of how to realize this general idea. Moreover, there is a close connection, as Quine himself explains, between this idea of setting philosophy within natural science, on the one hand, and the rejection of a priori knowledge on the basis of epistemological holism, on the other. If all human knowledge is basically of the same type, and no knowledge,
in particular, is forever immune to revision, then there is no higher or firmer type
of knowledge than that found in empirical natural science itself. There is no
Archimedean point from which philosophy could hope to justify natural science
from some better grounded and more certain perspective. The traditional dream
of providing a philosophical justification for scientific knowledge must therefore
be given up, and thus there is no longer any reason for attributing a special status
and role to philosophy.

A version of this last argument is the centerpiece of Quine’s “Epistemology
Naturalized.” On the basis of the failure of Carnap’s program for logically
reconstructing science out of sensory experiences in Der logische Aufbau der
Welt, Quine rejects the entire Carnapian enterprise of logical analysis or rational
reconstruction as such:

But why all this creative reconstruction, all this make–believe? The stimulation of his sensory receptors is all the evidence
anyone has had to go on, ultimately, in arriving at his picture of
the world. Why not just see how this construction really
proceeds? Why not settle for psychology? Such a surrender of
the epistemological burden to psychology is a move that was
disallowed in earlier times as circular reasoning. If the
epistemologist’s goal is validation of the grounds of empirical
science, he defeats his purpose by using psychology or other
empirical science in the validation. However, such scruples
against circularity have little point once we have stopped
dreaming of deducing science from observations.5

Given the failure of the Aufbau’s program for logically translating all concepts of
empirical science into purely sensory terms, Quine continues: “[I]t would seem
more sensible to settle for psychology. Better to discover how science is in fact
developed and learned than to fabricate a fictitious structure to a similar effect.6

In the context of Carnap’s actual motivations for his own program of logical
reconstruction, however, this particular Quinean stratagem is extraordinarily
misleading. For, in the first place, neither in the Aufbau nor in his later works did
Carnap set himself the goal of grounding, justifying, or “validating” science from
some supposedly higher and more certain philosophical vantage point. Indeed,
Carnap himself was always perfectly happy to depend on the best results of
current empirical research (he explicitly depends on the results of Gestalt
psychology in the Aufbau, for example), so that the foundationally motivated
strictures against circularity Quine rejects here were never part of Carnap’s own
motivations. And, in the second place, even after Carnap himself rejects the
Aufbau’s program of logically reconstructing science from a purely sensory basis,
he nevertheless continues to emphasize, in even stronger and more insistent
terms, that philosophy as he conceives it is an a priori or formal discipline whose
special province is logical analysis rather than empirical investigation.7 Even if
the particular logical reconstruction of science envisioned in the Aufbau cannot
in fact be carried out, we can still devote ourselves to articulating the logical
structure or logical framework within which empirical natural science proceeds. In this way, what Carnap is now calling *Wissenschaftslogik* is itself a purely logical or analytic discipline, wherein the correspondingly analytic formal scaffolding of synthetic or empirical natural science is to be clearly and precisely delineated. That the particular delineation attempted in the *Aufbau* cannot, for technical reasons, be carried out in no way undermines the general possibility of *Wissenschaftslogik* as such.

What does seriously challenge Carnap's characterization of philosophy as *Wissenschaftslogik* is Quine's attack on the first of the notorious "two dogmas of empiricism"—the doctrine, that is, that there is a clear and sharp distinction between formal, logical, or analytic truth, on the one side, and factual, empirical, or synthetic truth, on the other. The second "dogma of empiricism" is of course the doctrine of what Quine calls "radical reductionism"—the doctrine that each individual statement of natural science has its own particular range of confirmational sensory experiences via an *Aufbau*-style logical translation. This doctrine is of course threatened by Duhemian epistemological holism, but it is not, *pace* Quine, identical to the analytic/synthetic distinction.\(^8\) Indeed, in the period when Carnap puts the most weight on the analytic/synthetic distinction and the accompanying idea of philosophy as *Wissenschaftslogik*, Carnap himself explicitly adopts epistemological holism (which he associates with the names of Duhem and Poincaré). Accordingly, Carnap himself explicitly maintains that *any* statement of science—even the statements of logic and mathematics—can be revised in response to problematic empirical evidence, and thus Carnap himself explicitly maintains that no statement of science is forever immune to revision.\(^9\) It is just that for Carnap, in contrast to Quine, there remains, nonetheless, a sharp distinction between revisions of language or linguistic framework, in which analytic statements depending solely on the meanings of the relevant terms are revised, and factual revisions within a given language or framework, in which synthetic statements expressing contentful assertions about the empirical world are revised.

Now Quine's attack on the notion of analytic truth—on the notion of truth in virtue of meaning—does (despite its confusion with his attack on the doctrine of radical reductionism) pose a serious challenge to Carnap's formulation of the distinction between revisions of linguistic framework, on the one side, and factual revisions of empirical statements formulated within a given framework, on the other.\(^10\) Quine's attack on the notion of analytic truth thus challenges both Carnap's explanation of the special a priori status of logic and mathematics (as truths flowing simply from the adoption of a given linguistic framework) and Carnap's explanation of the special, non-empirical status of philosophy (as a branch of applied logic, as *Wissenschaftslogik*). So it is this attack—*not* the idea of epistemological holism and the doctrine that no statement of science is immune to revision—that provides the strongest support for contemporary philosophical naturalism. Indeed, as we have just seen, epistemological holism and the rejection of all absolute unrevisability is perfectly compatible, in Carnap's own hands, with both a sharp distinction between a priori and empirical
knowledge in general and a sharp distinction between philosophy and empirical
natural science in particular.

Quine’s attack on the notion of truth in virtue of meaning culminates, as is well
known, in his doctrine of the indeterminacy of translation, wherein the very notion
of determinate meaning, as it traditionally functions in philosophy, is rejected as
scientifically illegitimate. From this point of view, all that remains of the traditional
notion of analytic truth is the frankly ersatz notion of a “stimulus-analytic”
sentence—which receives community wide assent no matter what the given
sensory stimulation. And, from this point of view, it follows that ‘There have been
black dogs’ is just as stimulus-analytic as ‘2+2=4’. According to the doctrine of
the indeterminacy of translation, then, all that is left of the traditional notion of a
priori truth is the notion of relative (community wide) entrenchment.11 It is in this
sense that Quine’s attack on the notion of truth in virtue of meaning culminates
in philosophical naturalism.

But what is translation underdetermined by? In virtue of what (more precisely,
in virtue of the lack of what) is the traditional notion of meaning scientifically
illegitimate? In response to a sharp challenge on exactly this point from Noam
Chomsky, Quine explains that translation, and thus the traditional notion of
meaning, is underdetermined by the totality of truths of natural science:

Thus, adopt for now my fully realistic attitude toward electrons
and muons and curved space–time, thus falling in with the
current theory of the world . . . Consider, from this realistic point
of view, the totality of truths of nature, known and unknown,
observable and unobservable, past and future. The point about
indeterminacy of translation is that it withstands all this truth, the
whole truth about nature.12

Quine’s attack on the notion of truth in virtue of meaning, his correlative rejection
of the Carnapian distinction between a priori truth and empirical truth, and his
consequent articulation of philosophical naturalism, thus rests, in the end, on a
starkly physicalistic conception of modern natural science as the standard and
measure of all truth as such. And it is just this conception, taken in a looser and
more general sense, which then undergirds our current philosophical climate in
which philosophical naturalism appears to be all but intuitively self–evident. From
the point of view of modern natural science there can appear to be no room, as
it were, for either a special status for logic and mathematics or a special status for
philosophy. The only kind of truth it now appears possible to envision is just that
of empirical natural science itself, and any other putative type of truth now
appears to be shrouded in mystery.13

Let us consider this philosophical gloss on the preeminent status of modern
natural science more closely. And let us begin by considering those truths of
modern natural science given paradigmatic status by Quine—truths about
"electrons and muons and curved space–time." How, in particular, did the notion of curved space–time, which, as is well known, is central to Einstein’s general theory of relativity, actually arise and develop?

The general relativistic conception of curved space–time is the product of two fundamental developments in late nineteenth century mathematics: Felix Klein’s group–theoretical incorporation of the classical non–Euclidean geometries of constant curvature (including the Euclidean case of constant zero curvature) into the more general framework of projective geometry, and Bernhard Riemann’s even more revolutionary articulation of a general theory of manifolds of arbitrary dimension and curvature—including the hitherto uncontemplated case of spaces of \textit{variable} curvature. It was the first set of developments that led Hermann Minkowski to interpret Einstein’s 1905 special theory of relativity as describing a four–dimensional geometry—in Minkowski’s language, an “absolute world”—in which the Lorentz transformations linking inertial reference frames in Einstein’s theory are conceived as constituting a Kleinian group of a geometry of zero curvature closely analogous to Euclidean geometry. In this way, Einstein’s radical thesis of the relativity of simultaneity, which rejects Newtonian absolute time existing independently of space and motion, is interpreted as the assertion of a fundamentally new type of physical reality: “Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.” Yet such a Minkowskian four–dimensional framework, as Einstein soon came to realize, is inadequate for a theory of gravitation. The relativistic theory of gravitation Einstein finally brought to completion in 1916 adds Riemann’s ideas on arbitrary manifolds of variable curvature to the initial framework of what we now call Minkowski space–time. Gravitation is interpreted as a perturbation of the underlying Minkowski geometry by the distribution of matter and energy within space–time so that, in particular, the trajectory of a body in a gravitational field is now conceived as a maximally straight curve or geodesic—in Minkowski’s language, a maximally straight “world–line”—in a four–dimensional geometry of variable curvature.

It is in this way that the notion of curved space–time first entered modern physics. And, as is well known, this notion became generally accepted within modern physics on the basis of a small number of experimental tests—the most famous of which was the confirmation of Einstein’s predictions for the deflection of light in a gravitational field by observations made during a total eclipse of the sun by the British Solar Expeditions led by Arthur Eddington in 1919. It is in this way that the general theory of relativity, including the fundamental notion of curved space–time, first faced, in Quine’s words, the “tribunal of experience.” But the crucial question, from our point of view, concerns the status of the mathematical machinery of general relativity in such experimental tests. Eddington’s results on the deflection of light certainly confirm, or were taken to confirm, Einstein’s particular field equations governing the relationship between mass–energy density and space–time curvature. (More precisely, they confirm the so–called Schwarzschild geometry in the neighborhood of the sun, which is one particular solution to Einstein’s equations.) But do they also confirm the Kleinian theory of transformation groups and the Riemannian theory of
n-dimensional manifolds constituting the mathematical background to general relativity? Even if we are willing to speak in terms of differing degrees of "entrenchment" here, does it really make sense to envision a process of empirical testing in which even this mathematical background somehow equally faces the "tribunal of experience"?

I submit that this way of looking at the matter does not make sense—and not simply because no sane physicist or mathematician would describe the situation in this way. The fundamental problem is that general relativity is not happily viewed as something like a large conjunction, such that one conjunct is given by Einstein's field equations, another conjunct is given by the Kleinian theory of transformation groups, and a third conjunct is given by the Riemannian theory of manifolds—where we then view Eddington's experimental results, say, as potentially spreading empirical confirmation over the entire conjunction. Rather, the mathematical background of Einstein's theory functions as a necessary presupposition of that theory, as a means of representation or a language, as it were, without which the theory could not even be formulated or envisioned as a possibility in the first place.

To see this, let us first consider the situation in the seventeenth and eighteenth centuries, during the heyday of the Newtonian theory of gravitation. In this context, the modern concept of space–time simply does not exist. Space is represented by a three–dimensional geometry (Euclidean geometry is of course the only possibility), time is an entirely separate independent variable used to parametrize three–dimensional spatial trajectories, and gravitation is represented by a three–dimensional force acting immediately across arbitrary three–dimensional spatial distances. In this context, the general theory of relativity could not even be formulated, let alone be subject to empirical test. It is not that Newton's theory is adopted in preference to Einstein's on the basis of the then available evidence; the latter theory simply does not yet belong among the conceivable alternatives. Conversely, let us now consider the situation from the point of view of the space–time physics of the twentieth century. In this context, we see that we can now formulate all the theories of interest to us here—Newtonian physics, special relativity, general relativity—within the same four–dimensional language. Newtonian physics, too, can now be represented as a space–time theory, postulating a different structure for space–time—one containing counterparts of absolute time and absolute space—from that postulated by either special relativity or general relativity. Indeed, as the mathematician Elie Cartan showed in the 1920s, we can even formulate Newtonian gravitation theory using variably curved space–time, just as in general relativity. From this point of view it is then crystal clear that the mathematical machinery within which the concept of curved space–time is formulated is part of the means of representation or language of general relativity and not part of its empirical content. For all the theories in question here—which differ widely, of course, in empirical content—are now formulated within the very same mathematical language.

Kant understood a priori knowledge as supplying the presuppositions or conditions of possibility of empirical knowledge—as that which makes it possible
to formulate and justify objective empirical claims about sensibly given nature in the first place. And Kant modelled his particular theory of these a priori conditions of the possibility of objective experience on the Newtonian mathematical physics of his day—on Newtonian space, Newtonian time, and the Newtonian conception of matter, force, and interaction encapsulated in the laws of motion and exemplified in universal gravitation. At one place, Kant even compares this a priori framework to a language—as that which makes it possible for us “to spell out appearances, in order to be able to read them as experience.”16 We learned in the late nineteenth and early twentieth centuries, however, that the particular a priori framework envisioned by Kant is not the only possible such framework. And we learned this, of course, on the basis of precisely the sequence of revolutionary developments in both mathematics and mathematical physics briefly sketched above. We thereby learned, without a doubt, that such conditions of possibility or necessary presuppositions of empirical natural science should not be viewed as rigidly fixed for all time, as forever immune to revision. It does not follow, however, that such mathematical frameworks no longer have the characteristic “constitutive” function Kant first articulated—the function of making the rigorous formulation and confirmation of properly empirical theories in natural science first possible. On the contrary, as we have just seen, this is emphatically still the case in the general theory of relativity, where it is simply not possible either to formulate or empirically to test Einstein’s field equations without the revolutionary new mathematical framework due ultimately to Riemann and Klein.

Now such a generalization and relativization of the Kantian a priori, whereby it loses its rigidly fixed character but retains its essential “constitutive” function with respect to empirical knowledge, was in fact common coin within late nineteenth and early twentieth century scientific philosophy—most importantly, for our purposes, among the philosophers now known as logical positivists. Thus Reichenbach, for example, distinguished two meanings of the Kantian a priori—necessary and un revisable, fixed for all time, on the one hand, “constitutive of the concept of the object of [scientific] knowledge,” on the other.17 He argued, in this context, that the great lesson of the theory of relativity is that the former meaning must be dropped while the latter must be retained. Relativity theory, that is, involves a priori constitutive principles as necessary presuppositions just as much as does Newtonian physics; it is just that mathematical physics has changed its constitutive principles in the transition from the latter theory to the former one. And it was Carnap who brought this new, relativized and dynamical conception of the a priori to its most precise expression via his formally characterized distinction, briefly noted above, between revision of language or linguistic framework, on the one side, and revision of empirical statements formulated within a given linguistic framework, on the other.

As we also observed above, Quinean considerations about revisability and epistemological holism do not, by themselves, touch this new conception of the a priori in the slightest. Indeed, revolutionary scientific changes, wherein the very background framework or language within which empirical scientific theories are formulated itself undergoes radical transformation, provide this conception with its primary motivation and its strongest corroboration. In the case of the radical
conceptual transformation culminating in the theory of relativity, for example, we see that both mathematics and mathematical physics have undergone profound revolutionary changes. Nevertheless, although these two sequences of developments—mathematical and physical—indeed come together in a striking and dramatic fashion in the physical theory of general relativity, they still remain separate and distinct sequences evolving according to their own characteristic dynamics. The mathematical developments are driven largely by considerations of conceptual generalization and unification internal to mathematics, together with fruitful new results obtainable within mathematics by purely mathematical methods—methods which of course involve no appeal whatsoever to experimental or observational testing—whereas the developments in physics, by contrast, are self-consciously driven by precise experimental results. And in all this the mathematical developments constitute the necessary presupposition or condition of possibility of the physical developments, in that the formulation and precise experimental confirmation of the latter would not even be possible in the first place without the former. It is no wonder, then, that we find in Thomas Kuhn's theory of the nature and character of scientific revolutions—in the central Kuhnian distinction between change of paradigm, on the one side, and normal science, on the other—an informal counterpart of Carnap's formalized distinction between change of language or linguistic framework and rule-governed operations carried out within such a framework. Although Carnap's particular formalization has not in fact survived, the historical and philosophical relevance of this distinction for properly understanding the nature and evolution of modern natural science has in no way been thereby diminished.

We can deepen and generalize our appreciation of the characteristically constitutive role of mathematics within modern natural science, finally, by glancing back briefly at the scientific revolution of the sixteenth and seventeenth centuries which initiated it. For it was at this point, in the work of such thinkers as Kepler, Galileo, Descartes, Huygens, and Leibniz, that the very idea of a thoroughgoing mathematical description of sensible nature first gained wide currency. It was at this point, that is, that the previously dominant Aristotelian ideal of a largely qualitative and teleological description of our experience of the natural world was overturned in favor of the new ideal—constitutive of all modern physics—of a mathematically exact description based on geometry and laws of motion. This radical conceptual revolution profoundly transformed our idea of what it means for scientific statements to face the tribunal of experience—for this idea was now interpreted as requiring the deduction of precise mathematical results which could then be subject to correspondingly exact procedures of testing and measurement. And, as I hope to have made amply clear, in this meaning it simply makes no sense at all to assert that the mathematical background itself also faces the tribunal experience. Blindness to this simple yet fundamental point—and thus blindness to the characteristic status and function of what we might call the constitutive a priori—on the basis of a philosophical conception that prides itself on taking modern natural science as the paradigm of knowledge in general, is perhaps the most peculiar, and, I am tempted to say, most perverse, legacy of contemporary philosophical naturalism.
We have seen that the idea of a special a priori role for the mathematical
disciplines in our natural scientific knowledge is alive and well in post–Newtonian
mathematical physics and post–Kantian scientific philosophy. This idea has
nothing to do with a jejune obsession with epistemic certainty, unshakable
foundations, or absolute unrevisability. On the contrary, it is motivated throughout
by an appreciation of the manifold possibilities for development, growth, and
radical transformation in both pure mathematics and mathematical natural
science—and by an appreciation, above all, of the striking and unexpected ways
in which these two types of developments can influence and even merge with one
another in the course of revolutionary conceptual changes such as those
exemplified in the theory of relativity. It remains important, nonetheless, to
recognize that mathematical conceptual revolutions and physical conceptual
revolutions are not the same—and, in particular, that, in precisely such cases as
the theory of relativity, mathematics, however revolutionary in content, continues
to function as a means of representation or condition of possibility for the physical
principles which are thereby subject to exact empirical tests. We have also seen
that all of these ideas are given precise logical expression in the philosophy of
formal languages or linguistic frameworks developed by Carnap, a philosophy
which, as we noted at the very beginning, is in no way motivated by traditional
concerns for certainty, justification, or philosophical “validation.” And, whereas
Carnap’s repeated attempts to fashion an explicit logical characterization or
explication of the distinction between a priori and empirical truth have indeed
fallen prey to Quine’s penetrating attack on the analytic/synthetic distinction, it
does not follow that we should simply close our eyes to the historical realities of
scientific practice on behalf of a blandly undifferentiated philosophical holism.

If post–Kantian scientific philosophy no longer aims at supplying a foundation
or “validation” of scientific practice, however, then what role remains left for it?
Are we not faced, once again, with the idea that philosophy, as a discipline,
should simply be absorbed into empirical natural science—that it should, for
example, become that branch of the empirical study of actual human beings
where, in Quine’s words, “[w]e are after an understanding of science as an
institution or process in the world, and we do not intend that understanding to be
any better than the science which is its object?”21 Here, I believe, we can again
derive an important clue from the Carnapian distinction between change of
linguistic framework and rule–governed operations within a given such
framework—the distinction, in other words, between what Carnap calls external
and internal questions. For Carnap held that it is the characteristic fate of
philosophy to be entangled with external questions—with questions, in particular,
about which linguistic framework should be adopted for the total language of
science. Such questions, Carnap further held, can in no way be settled by
theoretical considerations, by either rules of evidence and confirmation
characteristic of factual or empirical science or rules of deduction and proof
characteristic of formal or mathematical science. External questions considered
in philosophy are therefore purely practical questions, and, as such, they are
answered, not by theoretical assertions, but by practical proposals to adopt one or another form of language.

I would put the guiding thought behind this Carnapian characterization of the peculiar role of philosophy as follows. In empirical natural science, as we have seen, we proceed against a background of concepts and principles—typically, mathematical concepts and principles—which constitute the framework or language of our inquiry. In particular, these concepts and principles make the rigorous formulation and testing of particular empirical hypotheses first possible, and, in this sense, they help to define what success or failure within this inquiry amount to. As such, the background framework in question contributes to the norms and standards of the discipline—norms and standards which, in the normal course of affairs, are generally taken for granted by the practitioners of the discipline. (In Kuhnian language, then, we are here concerned with elements of a paradigm definitive of a particular part or episode of normal science.) In pure mathematics, too, we typically operate against the background of generally agreed upon definitional stipulations and methods of proof—which, in a Carnapian-style rational reconstruction, would appear as primitive vocabulary, primitive axioms, and primitive rules of inference. And, in both cases, it is precisely the presence of such a generally agreed upon and taken for granted background that makes possible an inquiry we can honorifically characterize as “scientific”—that is, as progressive, as problem solving, and as capable of wide if not universal consensus.

It may also happen, however, that we have occasion to step back and reflect upon such a taken for granted background of disciplinary norms and standards. We may have occasion, that is, to call such norms and standards into question and to ask ourselves why precisely these concepts and principles should govern our inquiry. Indeed, during periods of deep revolutionary change it is just such questions that come to the foreground. Older constitutive principles (in Kuhnian terms, older paradigms) are challenged, new constitutive principles (in Kuhnian terms, new paradigms) are suggested. As Carnap would put it, we are now faced with an external question concerning the replacement of one linguistic framework by another. How, then, can we decide such a question? We cannot, by definition, appeal any longer to a generally agreed upon and taken for granted constitutive background, for it is just such a background that has now been called into question. We are thus no longer dealing with purely scientific questions in the above sense—that is, we are no longer operating wholly within what Kuhn calls normal science—and it is precisely here that characteristically philosophical considerations come into play.

Let us illustrate these ideas with a couple of examples. Consider first the revolutionary conceptual changes of the sixteenth and seventeenth centuries which initiated modern natural science as we know it. These events certainly involved a number of instances of striking empirical success in providing exact mathematical representations of nature in the modern style—notably, Kepler's new planetary astronomy (building, to be sure, on a long mathematical tradition) and Galileo's mathematical description of projectile motion (which was, in its own right, almost entirely new). Nevertheless, the ambitions of this new intellectual
movement far exceeded its grasp. For one here aimed at nothing less than a precise mathematical description of all of the phenomena of nature, to be achieved by an atomistic or corpuscular theory of matter that reduced all natural changes to the motions and mutual impacts of the constituent particles. And nothing even approximating such an atomistic reduction was actually achieved until the late nineteenth and early twentieth centuries—when, we might add, it was achieved using entirely new and hitherto entirely unforeseen mathematical and physical concepts. So it was not simply empirical and mathematical success in the modern style that motivated and sustained this intellectual movement. On the contrary, during its first fifty years especially, the intellectual movement which initiated modern natural science supported itself, above all, on the new system of natural philosophy fashioned by Descartes—a philosophy which not only sketched a complete program for a new, geometrical physics, but which also undertook the task of radically revising and reorganizing the wider system of philosophical concepts and principles bequeathed to western thought by Scholasticism (involving such concepts as substance, force, space, time, matter, mind, creation, divinity). Virtually all of the important thinkers of the period—for example, Huygens, Leibniz, and even Newton—began their intellectual careers as disciples of this new philosophical system. And, if they were later radically to revise or even to reject it, it still cast a long and indelible shadow across their own intellectual contributions—to such an extent, in fact, that these contributions are almost impossible to conceive except against this Cartesian background.

For a second example, consider once again the relativistic revolution in physics wrought by Einstein. It is well known that purely empirical considerations played a decidedly secondary role here. Not only did Einstein entirely ignore the celebrated experiment of Michelson and Morley in his 1905 paper on special relativity, but there was on the scene a fully developed competitor theory—the Lorentz–Fitzgerald “aether” theory—which was empirically equivalent to Einstein’s theory. Einstein himself cites a variety of philosophical influences on his thinking—including, especially, the “critical” and “skeptical” philosophies of Hume and Mach. With the benefit of hindsight, however, we can say that the philosophical ideas of the great French mathematician and mathematical physicist Henri Poincaré (who was of course deeply involved with the problems in electrodynamics addressed by special relativity and who Einstein was intensively reading at the time) were of perhaps even more importance. For Poincaré had arrived, on the basis of his own fundamental mathematical work on non–Euclidean geometry, at the idea that geometry is neither (pace Kant) a synthetic a priori product of pure intuition nor (pace Gauss and Helmholtz) a straightforward empirical description of what we can experience in nature. Establishing one or another system of geometry, according to Poincaré, rather requires a free choice, a convention of our own in order to bridge the irreducible gulf between our crude and approximate sensory experience and our precise mathematical descriptions of nature. There is no doubt that Einstein found this idea to be tremendously liberating, and it appears that it was this idea, above all, that stimulated him to view the concept of simultaneity, not as a simple datum of immediate intuition or experience, but rather as something to be fixed.
axiomatically by definition as part of the framework of a new proposed kinematics.\textsuperscript{24} Although Einstein was later, through his work on the general theory, to move decisively beyond Poincaré’s conventionalist philosophy of geometry, it is, once again, almost impossible to conceive Einstein’s initial liberating move without this philosophical background. Philosophy does not function, in such cases of fundamental conceptual transformation, as a firmer or more certain source of knowledge which we can then use to justify or “validate” the scientific changes in question. Nor does it proceed in splendid isolation, independent of the scientific developments themselves. Descartes was motivated in his new system of natural philosophy by earlier scientific discoveries—notably, by Copernican astronomy and by his own discovery of what we now call analytic geometry. Poincaré, as we just observed, was motivated by his own purely mathematical work in non-Euclidean geometry and was himself deeply involved with the newly emerging foundations of electrodynamics. Philosophy rather functions here at one level removed, as it were, from conceptual transformations within the sciences. It operates in an environment where a new constitutive framework (a new scientific paradigm) is not yet in place, and it suggests ideas, concepts, principles, and programs—typically of a less precise but more general character than the scientific constitutive frameworks themselves—which can motivate and support the pursuit of one such constitutive framework rather than another. In this sense, if scientific conceptual revolutions take place at one level removed from what Kuhn calls normal science, philosophy operates rather at two levels removed.

Carnap characterizes the answers we might reasonably attempt to give to philosophical questions as both conventional and purely pragmatic. He thereby emphasizes the element of free decision—that we are here not bound by fixed and antecedently agreed upon rules—as well as the fundamentally practical character of such questions—that, as a consequence, we are governed by standards of utility and expediency rather than truth. To this I would add the proviso that standards of utility and expediency are themselves often at issue in such cases—that the real problem is often to decide what we will now count as fruitful or successful. Our problem is rationally to negotiate new standards or ideals of fruitfulness and success, and not simply to estimate the probabilities of achieving already clear and agreed upon goals on the basis of accepted empirical results. I would also add a fundamentally historical dimension to our understanding of philosophical theorizing. In formulating new philosophical ideals we typically react to, and operate against the background of, previous philosophical ideals—as Descartes operated against the background of Scholastic natural philosophy or Poincaré operated against the background of both Kantianism and empiricism. Philosophy thus not only functions at a different level than the scientific disciplines, but also within its own characteristic intellectual context.

Lying at the basis of contemporary philosophical naturalism is the Quinean picture of the totality of human knowledge with which we began. Our knowledge is pictured as a vast web of beliefs, which responds as a total system to the impact of sense experience along the periphery, and within which, accordingly,
the only relevant distinctions we can make involve degrees of centrality and thus of entrenchment. Let me suggest, as an alternative, the picture of a dynamical system of beliefs, concepts, and principles that can be analyzed, for present purposes, into three main components: an evolving system of empirical natural scientific concepts and principles, an evolving system of mathematical concepts and principles which frame those of empirical natural science and make their rigorous formulation and precise experimental testing possible, and an evolving system of philosophical concepts and principles which serve, especially in periods of conceptual revolution, as a source of suggestions and guidance in choosing one scientific framework rather than another. All of these systems are in continual dynamical evolution, and it is indeed the case that no concept or principle is forever immune to revision. Yet we can nonetheless clearly distinguish the radically different functions, levels, and roles of the differing component systems. In particular, although the three component systems are certainly in perpetual interaction, they nonetheless evolve according to their own characteristic dynamics. Only in the case of the empirical natural scientific system, for example, do precise experimental tests function as relevant dynamical factors—only here do our beliefs, in this sense, squarely face the tribunal of experience. Both in mathematics and in philosophy, by contrast, freely creative responses to preceding intellectual developments are the primary engines of change. In all this, however, what is most striking and inspiring are the periods of profound conceptual transformation where each of the three components contributes its own characteristic involvement in those revolutions of thought that figure among the very highest achievements of our intellectual life.

Notes

5. Quine, op. cit., p. 75–76.
6. Quine, op. cit., p. 78.
7. In the most explicit polemic against psychologism Carnap ever wrote, “Von der Erkenntnistheorie zur Wissenschaftslogik,” in Actes du Congrès international du philosophie scientifique, Paris, 1936, Carnap depicts scientific philosophy as going through three main stages of development: the first is the transition from speculative philosophy or metaphysics to epistemology, the second is the rejection of the synthetic a priori and the transition to an empiricist epistemology, the third and final stage is the transition from epistemology to the logic of science [Wissenschaftslogik]. The main problem here is to realize that epistemology as practiced so far—including in Carnap’s own earlier work—is an “unclear mixture of psychological and logical constituents” (p. 36).
8. I thus cannot follow Quine in his assertion, "Two Dogmas," p. 41, that "[t]he
two dogmas are, at root, identical."
1937, §82.
10. Compare Carnap, "Empiricism, Semantics, and Ontology," in his Meaning
12. Quine, "Reply to Chomsky," in Words and Objection: Essays on the Work of
13. A good example of this more general physicalistic tendency is Paul
661–679, which raises problems for the notion of mathematical truth on the basis
of a causal theory of knowledge: see especially pp. 671–72, which motivate this
causal theory of knowledge by reference to twentieth century space–time physics.
14. H. Minkowski, "Space and Time," Address delivered at the 80th Assembly of
German Natural Scientists and Physicians, 1908, trans. W. Perrett and G. B.
15. Such a "conjunctive" view of empirical confirmation and testing—where logic
and mathematics are treated simply as further conjuncts—is explicit in Quine,
16. I. Kant, Prolegomena to Any Future Metaphysics, §30.
17. Reichenbach developed this analysis in his first book, Relativitätstheorie und
Erkenntnis Apriori, published in 1920. For an English version see H.
Reichenbach, The Theory of Relativity and A Priori Knowledge, trans. M.
Reichenbach, Berkeley, 1965. For further discussion see my "Geometry,
Convention, and the Relativized A Priori: Reichenbach, Schlick, and Carnap," in
18. It is well known that Carnap, for his part, was quite enthusiastic about The
Structure of Scientific Revolutions. And, towards the end of his career, Kuhn was
fond of characterizing his viewpoint as "Kantianism with movable categories."
19. A fuller treatment of this point would need to distinguish pure mathematics,
where we develop theories of various mathematical structures (e.g., differentiable
manifolds), from applied mathematics, where we assert that some such
mathematical structure provides a model or representation for a given physical
domain (e.g., that space–time events can be modelled or represented by a
relativistic differentiable manifold). The claim in the text is clear and
incontrovertible, I believe, for pure mathematics, but the applied case obviously
raises numerous additional questions. (I am indebted to Elisabeth Lloyd and
Stephen Leeds for prompting me to make this distinction explicit.)
20. For this terminology, deployed in a more general philosophical setting, see G.
De Pierris, "The Constitutive A Priori," Canadian Journal of Philosophy,

24. The passage from Einstein cited in note 22 above states that the key insight was to recognize the "arbitrariness" of "the axiom of the absolute character of time, viz, of simultaneity"—language which certainly sounds far more like Poincaré than either Hume or Mach.