In ‘Two Dogmas of Empiricism’, Quine attacks the analytic/synthetic distinction and defends a doctrine that I call epistemological holism. Now, almost fifty years after the article’s appearance, what are we to make of these ideas? I suggest that the philosophical naturalism that Quine did so much to promote should lead us to reject Quine’s brief against the analytic/synthetic distinction; I also argue that Quine misunderstood Carnap’s views on analyticity. As for epistemological holism, I claim that this thesis does not follow from the logical point that Duhem and Quine made about the role of auxiliary assumptions in hypothesis testing, and that the thesis should be rejected.

Quine’s publication in 1951 of ‘Two Dogmas of Empiricism’ was a watershed event in 20th century philosophy. In that essay, Quine sought to demolish the concepts of analyticity and a priori; he also sketched a positive proposal of his own—epistemological holism. There can be little doubt that philosophy changed as a result of Quine’s work. The question I want to address here is whether it should have. My goal is not to argue for a return to the halcyon days of the logical empiricists. Rather, I want to take stock. Now, almost fifty years after the publication of ‘Two Dogmas,’ what view should we take of analyticity, the a priori, and epistemological holism, and of what Quine said about these topics?

Analyticity is an issue in the philosophy of language, a priori an issue in epistemology. As a first approximation, the two questions are as follows: Are there sentences that are true in virtue of the meanings of the terms they contain? And are there propositions that we can know to be true independently of sense experience? A central critical objective of ‘Two Dogmas’ was to dismantle a view that sought to connect these two questions. The
positivists' *linguistic theory of the a priori* claimed that the special epistemological status of *a priori* propositions derives from the fact that they are expressed by analytic sentences and these sentences are analytic because of the meanings assigned to their constituent terms by the adoption of linguistic conventions. Notice that even a devastating criticism of the concept of analyticity would leave open the possibility that some other defence of the concept of *a priori* could be given. It is here that Quine's positive proposal is meant to do its work—epistemological holism excludes the possibility of *a priori* knowledge.

I

Quine on Analyticity. It would be a mistake to think that Quine's point was simply that nothing is *a priori* or analytic. He didn't regard these concepts as clear, but empty, like the concept of a round square. Rather, the point was supposed to be that these concepts are 'unclear'. And this didn't mean that the concepts are vague—that there are situations in which there is no fact of the matter as to whether the concepts apply. Being analytic wasn't supposed to be like being bald; it is perfectly clear what baldness is, even though there are grey areas in which there is no saying whether the concept applies.²

Quine's main complaint in 'Two Dogmas' against the concept of analyticity is that it can be defined only in terms of other equally suspect concepts, such as meaning, necessity, synonymy, proposition, and definition. For example, one might say that the sentence 'bachelors are unmarried' is analytic because the truth of the sentence follows from the definitions of the terms that occur in it. However, if there are doubts about whether analyticity is a clear concept, there also will be doubts about what it takes for a sentence to express a definition.

If this and similar proposals are unsatisfactory, what would count as a 'clear' definition of analyticity? Quine sometimes says

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2. The main concept in Quine's epistemological holism is *centrality*—beliefs differ in their degree of centrality; there is no dividing line between beliefs that are *a priori* and analytic on the one hand and those that are *a posteriori* and synthetic on the other. If this were the whole message of 'Two Dogmas', one might reasonably summarize Quine's point by saying that he regarded the analytic/synthetic distinction as a vague one. However, Quine's criticism of analyticity goes beyond this, as we will see.
that an acceptable characterization of analyticity would have to be specified in terms of observable behaviour (Quine 1953b, 1960, 1970); at other times, he says that extensional semantic concepts, such as reference and truth, are legitimate, and that analyticity would also be legitimate if it could be defined in terms of them (Quine 1953a). But analyticity cannot be defined in terms of behaviour or in terms of concepts drawn just from the theory of reference, so we must reject analyticity as 'unclear'.

It is a testimonial to how influential Quine has been with respect to some of his other philosophical ideas that this criticism of analyticity now seems so thin. Quine taught us that the main question we should ask about a theory is whether it is indispensable as a device for explaining and predicting phenomena. The question, then, should not be whether analyticity can be defined, but whether the concept is useful in science. The concept of 'electron' cannot be defined in terms of observables, but that says nothing about what we should think of the electron theory.

3. Although Quine rejects the notion of analyticity, he occasionally endorses what he regards as acceptably sanitized readings of the concept. For example, Quine (1960) says that a sentence is 'stimulus analytic' if every speaker of the language is disposed to assent to it. And Quine (1974, p. 79) proposes that we say that '... a sentence is analytic if everybody learns that it is true by learning its words.' The sentence 'dogs exist' is probably stimulus analytic. Is it analytic according to the second proposal? Suppose that people learn that dogs exist in the process of learning to use the term. Do they thereby learn that 'dogs exist' is true by learning its words? Well, they learn that the sentence is true when they learn its words. But what about 'by'? The more-or-less traditional concept of analyticity says, roughly, that a sentence is analytic when one can know that it is true just by knowing what its terms mean. Learning that dogs exist may be a standard concomitant of learning to use the constituent terms, but the existence of dogs does not follow from statements that describe what the term means (pace the ontological argument's treatment of a term that has the opposite spelling).

Quine clearly does not intend his proposed definition of analyticity to coincide with the traditional concept, so it would be no objection to his proposal if it judged 'dogs exist' to be analytic. However, the proposal can be interpreted to rule this out: if learning the terms = learning what the terms mean, then speakers do not learn that 'dogs exist' is true just by virtue of learning the terms. Is it possible that Quine's proposal is an endorsement of the traditional concept rather than a replacement of it?

4. Grice and Strawson (1956, p. 203) point out that 'reductive analysis' is too much to hope for, and go on to suggest that concepts can be explained in 'less formal ways.' They then say that the analytic sentences are the ones whose denials 'have no sense (p. 205.)' I, of course, agree with their complaint. However, I suspect that their suggestion will not accord with the notion of meaning required in linguistics and cognitive science. The denial of an analytic truth does have a meaning; this is part of the reason we are puzzled when people deny analytic truths. There is an important theoretical difference between 'bachelors are sometimes married' and strings of words that are not well-formed.
Quine observes in ‘Two Dogmas (p. 44), ‘objects at the atomic level are posited to make the laws of macroscopic objects... simpler and more manageable; and we need not expect or demand full definition of atomic and subatomic entities in terms of macroscopic ones...’ It also is worth recalling that Quine felt obliged to affirm that sets exist, since set theory is needed for mathematics and for mathematized natural science. The philosophical attractiveness of nominalism had to give way to the demands of science. Why didn't Quine take the same attitude towards analyticity?5

Quine's criticism of modal concepts has the same character. He claims that the concept of necessity is not 'clear', where the standard of clarity is in fact reductionistic. Necessity has to be definable in terms that are extensional, just as analyticity has to be definable in terms that are behavioural. This cannot be done, so the concepts should be rejected. Whether science would lose anything of value in rejecting these concepts is a question that never got broached. Quine assumed that an extensional language would be adequate for codifying the results and inferences found in mathematized science.

Why did Quine bypass science and consider the concepts of analyticity and necessity only in terms of how they answer to

5. Occasionally, Quine has drawn back from his demand that analyticity be defined either behaviourally or extensionally. For example, in Word and Object, Quine (1960, p. 206) says ‘...we find it argued that the standard of clarity that I demand for synonymy and analyticity is unreasonably high; yet I ask no more, after all, than a rough characterization in terms of dispositions to verbal behaviour.' If 'rough characterization' means only that one be able to describe behaviours that would be evidence that a sentence is analytic in a speaker's idiolect, the requirement is easy enough to satisfy. As Carnap (1953) pointed out, if you ask a speaker whether it is conceivable to them that a sentence (e.g., 'either pigs exist or pigs do not exist') could turn out to be false, and they reply 'no, the sentence is true by definition,' that is evidence that the sentence is analytically true. The inference is not infallible, of course, but empirical evidence is rarely like that in any case.

Quine's reply to Carnap's (1953) is his doctrine of the indeterminacy of translation. In Chapter 2 of Word and Object, Quine (1960) claims that the totality of a speaker's verbal behaviour (including not just what he actually says, but what he would say in various circumstances) can be consistent with two mutually incompatible accounts of what the speaker's terms mean. Quine claims, not just that these accounts will be behaviourally equivalent, but that there is no fact of the matter as to which is true—his claim is not just that behaviour underdetermines theory, but that semantic theories are prey to an indeterminacy. I reply, as others have done before, that underdetermination is a property of all theorizing, and that Quine has not shown that underdetermination should be coupled with a claim of indeterminacy in the case of theories about meaning.
some antecedently assumed standard of philosophical clarity? With respect to analyticity, part of the reason may have been that cognitive science was not sufficiently developed when Quine wrote for him to have felt that the practices of science could trump the arguments of philosophers. He probably assumed that the concept of meaning was to a large extent a concept that philosophers owned.

As for the concept of necessity, Quine believed that modal discourse is not part of the substance of science proper, but consists in a philosophical overlay that can be peeled away without damaging the properly scientific core. Scientists talk of ‘laws’ and make counterfactual inferences, but the real content of science, for Quine, is resolutely confined to the actual. This point of view has not stood the test of time. It now seems clear that the concept of probability, as it is used in scientific theories, is a modal concept.6 Talk of the half-life of a radioactive material, for example, cannot be glossed in terms of the actual frequency of an event-type in a larger reference class. The point can be seen by asking what it means to say that a coin has a probability of 1/2 of landing heads when tossed. This doesn’t mean that the coin will land heads half the time; a fair coin can be tossed an odd number of times and then destroyed. It once was thought that the statement means that if the coin were tossed again and again indefinitely, that the relative frequency of heads would necessarily approach a limit of 1/2. This would entail that modality is part of the content of scientific theories that use probabilities, but in fact the concept of objective probability is even less reducible to frequencies than this interpretation suggests. The most that can be said about the relationship of probabilities to hypothetical relative frequencies is given by the Law of Large Numbers (Skyrms 1980)—if a coin’s probability of landing heads is 1/2, then the probability approaches unity that the frequency of heads will be within epsilon of 1/2, as the run of tosses is

6. I set to one side the way in which probability helps characterize the evidential credentials of scientific theories. Here I am interested in probabilistic concepts (e.g., fitness, half-life) that are used in theories. Also, in saying that probability is a modal concept, I am talking about the interpreted concept, not the uninterpreted formalism. The Kolmogorov axioms of probability can be interpreted in terms of actual relative frequency, and there is nothing modal about this interpretation. However, this interpretation does not do justice to the way that probability is used in scientific theories.
made longer. A fair coin doesn’t have to land heads 50% of the time, even in the limit. Probability doesn’t reduce to hypothetical relative frequency; rather probability claims confer probabilities on hypothetical relative frequencies. Probability is irreducible.7

In any event, Quine’s circularity argument against analyticity and necessity is a prime example of an appeal to ‘first philosophy’, a practice that Quine later criticized in his essay ‘Epistemology Naturalized’ (Quine 1969). First philosophy seeks a justification for the propositions we believe that is more secure than the evidence provided by natural science. Philosophers who think that Quine was right to reject first philosophy should be skeptical about the arguments in ‘Two Dogmas of Empiricism’ against analyticity and modality.8

II

Carnap on Analyticity. If Quine did not treat analyticity as a theoretical term in empirical science, neither did the the logical empiricist who was the main target of Quine’s criticisms. Carnap thought that it is a matter of convention what sentences one regards as analytic. Claims about what is analytic are proposals; the question of truth and falsity does not arise (Creath 1991,

7. Stein (1992, pp. 288–289) makes a complementary point—‘...the concept of the space of all states of a physical system is a central one in much of classical physics, and in all of quantum physics; and “all states” means, of course, all possible states.’

8. Although Quine often contrasted his naturalism with Carnap’s foundationalism, in fact Quine’s metaphilosophical position is quite Carnapian:

He who wishes to investigate the questions of the logic of science must, therefore, renounce the proud claims of a philosophy that sits enthroned above the special sciences and must realize that he is working in exactly the same field as the scientific specialist, only with a somewhat different emphasis...

This may sound like the Quine of ‘Epistemology Naturalized’ (Quine 1969), but it isn’t. It’s the Carnap of Logical Syntax of Language (Carnap 1937, p. 332).

Naturalism is sometimes understood as the view that a conflict between philosophical argument and scientific practice must always be resolved by rejecting the former. I do not understand it that way. If philosophy is in fact continuous with science, then we should be skeptical of any such formula. If philosophical critique is part of the practice of science, then the right resolution of such conflicts will sometimes be to change scientific practice.

Fogelin (1997, pp. 554–555, pp. 561–563) argues that Quine’s naturalism is at odds with his arguments on behalf of the theses of indeterminacy of translation and of reference. For Fogelin, naturalism is characterized by the Peircean commitment to eschew Cartesian skeptical challenges. I see a different relationship between naturalism and skeptical problems, but that is a subject for another essay.
p. 365; Stein 1992, pp. 279–282; Ricketts 1994). This is the point of Carnap’s (1937) Principle of Tolerance—‘in logic, there are no morals’.

Carnap’s views about meaning, and about analyticity in particular, need to be understood within the context of the three-part distinction that he and other empiricists drew among descriptive definitions, explications, and outright stipulations. A purely descriptive definition aims to record what terms actually mean in the language spoken within a community. An explication takes a pre-existing concept and clarifies it; a stipulation starts from scratch, conferring on a symbol a meaning that may be entirely unrelated to whatever meaning it may have had before. It is obvious why there is no philosophical interest in merely stipulating what a term means. But why did Carnap reject the project of recording the meanings of terms as they actually are used? Carnap’s reason was that he thought that the terms in a natural language are too vague and confused. From Frege, he inherited the idea that the psychological states of language users vary from person to person and, being subjective, are ultimately uncommunicable; and under the influence of Tarski’s theory of truth, he came to believe that natural languages are contradictory. Meaning in natural languages is not amenable to systematic and rigorous scientific study. Although the concept of analyticity has a piecemeal and fragmentary application to natural languages and their speakers (Carnap 1952b, 1953), its rigorous and systematic deployment can take place only within the confines of an artificial language.

Since explications are proposals, they are to be judged by their usefulness, not by whether they are true. Still, one of the considerations that enters into the evaluation of an explication is its fidelity to pre-existing usage (Carnap 1950; Creath 1991, pp. 368–369). For example, the separation of definitions from empirical claims in an axiomatization of Newtonian mechanics would be intended to clarify the question of what is a priori and what is empirical in that theory—i.e., the theory that scientists had developed before the philosophers came along. In such an axiomatization, the claim that ‘F = ma’ is a definition of force would be intended to characterize a theory that was formulated and put to work by scientists; there would be no philosophical point in simply stipulating that ‘F = ma’ is analytic, or in stipulating that it
is not. Philosophers can stipulate all they like; the character of Newtonian mechanics is not affected, and it is the latter that philosophers want to understand.

Explication, then, can be thought of as a ‘mixture’ of description and stipulation, or as a ‘compromise’ between them. To offer an explication is to make a normative remark. One is suggesting that terms be used in a way that retains some allegiance to pre-existing usage but also departs from that usage. To give an explication of what a term means is to say something that is neither true nor false. The same point holds with respect to claims that a certain sentence is analytic. For Carnap, to say that ‘F = ma’ is analytic, or that it is not, is to make a proposal.

Thus, Quine and Carnap agree that claims about meaning and analyticity do not describe facts (Creath 1991, Stein 1992, Ricketts 1994), but they agree on this point for very different reasons. Quine takes the subject matter of such claims to be natural languages; there is no fact of the matter about what terms mean or about which sentences are analytic because the concepts of ‘meaning’ and ‘analyticity’ are not sufficiently clear. Carnap takes the appropriate subject matter of such claims to be artificial languages that are constructed with the goal of explicating usage in this or that natural language. The concepts of ‘meaning’ and ‘analyticity’ are scientifically respectable when deployed within an artificial language, but they are used to formulate proposals, not descriptions. They fail to describe facts for the simple reason that they are not intended to do so.

What Carnap and Quine disagree about is whether the analytic/synthetic distinction, and the concept of meaning generally, is useful. Carnap (1963, p. 922) says that the concept of analyticity is ‘indispensable for methodological and philosophical discussion’; the analytic/synthetic distinction is important because it serves to separate nonempirical from empirical elements in a system of beliefs. This epistemological distinction is normative; it concerns how we should think about the justifications that different beliefs possess. Quine thinks the distinction is useless, for two reasons. First, he thinks it isn’t needed in the empirical study of natural languages. Second, it isn’t needed in epistemology either; Quine’s epistemological holism, which we will discuss later, entails that there is no distinction between a priori and a posteriori beliefs.
III

Analyticity in Natural Languages. Carnap rejected the idea that the analytic/synthetic distinction marks a factual distinction among sentences in a natural language. Did he do so for good reasons? The fact that natural languages are vague and unclear does not show that the analytic/synthetic distinction fails to apply. For example, perhaps ‘rich’ is a vague term; perhaps there is no precise number of dollars that marks the difference between someone who is rich and someone who is not. Even so, the sentence ‘rich people have more money than poor people’ may still be analytic. It also is possible that the concept of being rich is useful in the science of economics. Similar points apply to the claim that natural languages are contradictory. What is true is that natural languages permit the construction of paradoxical sentences such as ‘this sentence is false’. However, that doesn’t show that the language is contradictory; after all, the language is not an assertion, but a device for constructing assertions. In any event, whatever imperfections there may be in natural languages, the task of semantics as an empirical discipline is to describe their semantic properties, warts and all. Philosophers may be more interested in reforming usage, but that does not show that the task of description is ill-conceived. Of course, it does not follow that the concepts of meaning and analyticity as currently understood are sufficiently clear. They are terms of art—in philosophy and in linguistics—and they may need to be elaborated in various ways, perhaps by the construction of formal systems. However, the point of doing so may be that one wishes to describe the semantic features of natural languages.9

Suppose a group of speakers uses a language that is as clear as Carnap might wish. Perhaps they have adopted the proposals set forth in one of Carnap’s artificial languages, or perhaps their discourse is already so clear that an explication of what they are saying is really nothing more than a description. In either case, is and ought coincide. In this instance, claims about meaning and analyticity are not merely proposals; they are descriptions. But of

9. The same point holds if we interpret analyticity as Carnap did—not as a term intended to describe natural languages but as an epistemological category. Surely there are sentences for which it is a factual question whether they are a priori or a posteriori.
what? What properties of utterances is one describing when one talks about what terms mean or about which sentences are analytic? This question needs to be answered even if one holds, as Carnap did, that the analytic/synthetic distinction makes clear sense only when the language under discussion is artificial.

The standard answer to this question provided by logical empiricism is that analytic sentences in some way embody linguistic conventions. They are true, not because of the way the world is, but purely because of how speakers assign meanings to terms. Quine attacks this conventionalist theory of analyticity, so the theory is worth investigating, even if Carnap would have been reluctant to embrace it as an account of meaning in natural language.

IV

Analyticity and Conventionalism. The logical empiricists were attracted to a conventionalist account of analyticity in part because of their conviction that there are important conventional elements in Einstein’s special theory of relativity. When a light signal leaves a source, bounces off a mirror, and then returns to the source, what fraction of its travel time does it spend going out and what fraction does it spend coming back? The positivists thought that Einstein’s insight was to recognize that any answer to this question is a matter of convention (Reichenbach 1949, Carnap 1966). Einstein chose 1/2, but that was a stipulation, not an empirical discovery.

The positivists represented this claim by saying that Einstein had made a linguistic decision; he had adopted a ‘linguistic framework’ in which

\[(S)\] A light ray that goes from a source, to a reflector, and back to the source, spends 1/2 its travel time going out from source to reflector and 1/2 coming back from reflector to source.

is an analytic truth. Other choices of ‘language’ are possible, and within those alternative languages, sentence (S) would be analytically false. This was not a happy formulation. A real analytic truth is true, and what it says typically does not depend for its
truth on our choice of language. Given standard English usage, the sentence ‘bachelors are unmarried’ is true; if English had involved different conventions about what words mean, the sentence might have meant something different, and so it might have been false. However, the proposition expressed by the English sentence—that bachelors are unmarried—does not depend for its truth on how English works. There are bachelors in France who would very much resent the English chauvinism involved in saying that the way English works affects whether (French) bachelors are unmarried. ‘Quelle impertinence!’ one can hear them exclaim. The fact that bachelors are unmarried is no more dependent on English than it is on French. Genuine (object-language) analytic truths express truths, and the propositions those sentences express are language-independent.

Applied to sentence (S), this pattern of reasoning entails that if (S) is true in Einstein’s language, then what the sentence expresses is true, and moreover, it is true independently of how Einstein chose to set up his language. But surely this is not what the positivists wanted to say—their epistemological point is better put by saying that there is no fact of the matter about whether (S) is true. Adopting (S) may be a matter of convention, but (S) is not made true by anything, not even by adopting a convention. Given what the sentence about travel time actually means in English, it would be a mistake to say that it is an analytic truth or an analytic falsehood. It is neither, and it isn’t an empirical truth or an empirical falsehood, either. Conventionalists should say two things: First, (S) is neither true nor false and the same holds for all its sister propositions that assign fractions different from 1/2. Second,

10. I say ‘typically’ because the proposition expressed by a meta-linguistic statement does depend for its truth on facts about a language. Consider, for example, the claim that the term ‘mother’ means female parent in English. Whether this is so obviously depends on facts about English. However, object-language statements do not have this property. Whether mothers are female parents does not depend for its truth on facts about English.

11. This point was made forcefully by Frege (1884) and has been reiterated by many subsequent writers (e.g., Ewing 1940, Kneale 1947, Pap 1958, Harman 1967, Lewis 1969, Boghossian 1996). It does not depend on any particular ‘theory of propositions’—i.e., on any particular view about how propositions are individuated. What is required is some distinction between a sentence and ‘what the sentence says’ (Boghossian 1996, p. 380).

12. Here the conventionalist disagrees with the skeptical realist, who claims that (S) has a truth value that it is impossible to ascertain. The conventionalist and the skeptical realist make the same epistemological point—that (S) cannot be known to be true—but they disagree about the reason why.
an arbitrary choice must be made of one such statement for purposes of theory construction. Including (S) in one’s theory is dispensable, but including some such principle is indispensable.\(^\text{13}\)

It is important to recognize the epistemic status that the conventional elements in one’s system of beliefs possess. If (S) lacks a truth value, then no observation can empirically disconfirm it; it is ‘immune from empirical revision’. However, this is not because (S) is certain to be true (Ricketts 1994). (S) is \textit{a priori} in the negative sense that its justification does not depend on experience, but what, if anything, can be said positively about the justification there is for believing it? Here it is useful to recall the distinction between pragmatic and epistemic reasons for believing a proposition, a distinction that is familiar in connection with Pascal’s wager. Epistemic reasons have to do with the proposition believed; pragmatic reasons have to do with the act of believing. Pascal says that even if there is no epistemic reason to believe in God, there is a pragmatic one. Even if there is no evidence for the proposition that God exists, there still is a good reason for agents to get themselves to perform the act of believing. Conventionalists assert that there is no epistemic reason to believe (S), though there may be a pragmatic reason to adopt that convention.

I hope these comments make it clear why conventionalism as a thesis about the content of various scientific theories provides a bad model for thinking about analyticity. Analyticity has to do with language, but conventionalism in science has nothing much to do with that. Furthermore, the example about bachelors shows that it is a mistake to think of linguistic practice as making analytic (object-language) sentences true. The linguistic conventions—the arbitrary pairing of words with meanings that is operative in a language—establish what proposition a given sentence in a language expresses. This is true for \textit{all} sentences in

\(^{13}\text{Thus, it should be clear how conventionalism as a thesis about the content of a specific scientific theory differs from ‘trivial semantic conventionalism’—the claim that the pairing of words in a language with their meanings or referents is arbitrary. It is interesting that Carnap (1963, p. 916) makes this very point. In discussing the logical truth ‘all black dogs are dogs,’ he says that ‘once the meanings of the individual words in a sentence of this form are given (which may be regarded as a matter of convention), then it is no longer a matter of convention or of arbitrary choice whether or not to regard the sentence as true; the truth of such a sentence is determined by the logical relations holding between the given meanings.’\textquoteleft\textquoteleft}
a language, not just the analytic ones. Whether bachelors are unmarried depends on linguistic conventions no more than whether there are nine planets.

The argument just described against the thesis of truth-by-convention makes use of a counterfactual; we are asked to consider, for example, whether bachelors would still be unmarried if English had developed differently. Those, like Quine, who think that counterfactuals are 'unclear' may be unimpressed with this argument. But once again, the rejection of first philosophy that Quine did so much to promote leads to a different assessment. It is true that philosophers have not been able to provide a fully adequate account of the rules that govern counterfactual reasoning. It also is true that extant accounts (e.g., those of Stalnaker 1968 and Lewis 1973) have had to make use of irreducibly modal concepts, and this will arguably be a feature of future theories. If so, philosophical theories of counterfactual reasoning will not measure up to Quine’s standards of clarity. However, none of these points provides a compelling reason to ‘reject’ counterfactual reasoning. What needs to be remembered is that counterfactual reasoning is central to scientific inference. Eating lollipops is correlated with being young, but eating lollies doesn’t make one youthful. Children would still be young if they stopped consuming lollipops. This is an entirely mundane type of remark, one that is often made in scientific contexts (see, for example, Fisher’s smoking hypothesis). The use of counterfactuals in the argument against the thesis of truth-by-convention should be no more suspect.14 Quine’s discussion of modality embodies an attempt to change science; the point, however, is to understand it.

V

The Argument of ‘Truth by Convention’. Not only does the argument just presented against the idea of truth-by-convention involve counterfactual reasoning; in addition, it relies on a

14. It doesn’t help the conventionalist to assert that the relation between our adopting conventions and the truth of various propositions is ‘constitutive’ rather than causal. The counterfactual test is still appropriate—if C makes P true (where ‘makes’ is understood constitutively), then it should turn out that P would not be true if C had not occurred.
distinction—the distinction between a sentence and what the sentence expresses (a 'proposition'). This is a distinction that Quine might have qualms about drawing, since talk of propositions, for him, is not legitimate (but see footnote 11). Accordingly, it is no surprise that Quine advanced a quite different argument against conventionalism in his earlier essay ‘Truth by Convention’. Quine (1936) considers whether logical truths like ‘if pigs fly, then pigs fly’ could be true by convention. He observes that there are infinitely many such sentences, which means that we would not be able to stipulate, one by one, that each is true. Rather, we might try to cover them all by making stipulations about general logical schemata; for example, one could propose the convention that all statements of the form ‘if P, then P’ are true. Quine then points out that applying this stipulation about the schema to each of its many instances requires the use of logic. We would have to reason as follows:

The sentence ‘if pigs fly, then pigs fly’ has the form ‘if P, then P’.
All sentences of the form ‘if P, then P’ are true by stipulation.

'If pigs fly, then pigs fly' is true by stipulation.

As this derivation shows, logical truths aren’t true just in virtue of the stipulations we make about various logical schemata. Rather, the first premiss reports a fact about logical form that is independent of the stipulation described in the second premiss; in addition, the argument uses a rule of inference that connects premisses to conclusion, and this rule is not made valid by the reported stipulation. In this argument, nonstipulated facts about logic are used to connect the general stipulation about the schema to a specific stipulation concerning the sentence at issue. Conventionalism thereby fails as a thesis about all logical truths.

This is an important argument. However, we need to be clear on what it does and does not entail. It does not show that no sentence is true by convention. It is quite possible, for all this argument says, that there are finitely many statements that are true by convention. The problem with conventionalism that Quine identifies arises only when conventionalists are ambitious—when they claim that the number of sentences that are true by convention is infinite. Indeed, Quine says in both ‘Truth by Convention’ and in ‘Two Dogmas’ that he has no
problem with the idea that explicit stipulation can render a sentence true.\(^{15}\)

The argument discussed before that turns on the distinction between a sentence and the proposition it expresses suggests that Quine is too generous to the idea of explicit stipulation. To see why, let's consider a simple example of a stipulative definition. Suppose I announce that I'll use the term 'ap' to mean apple. If I subsequently say 'an ap is an apple', my statement will be true. I'll also be saying something true if I say 'some aps grow in Wisconsin'. How do these two utterances differ? In both cases, we figure out whether a sentence is true by deciding what proposition the sentence expresses and then determining whether that proposition is true. If we know *a priori* that an apple is an apple and we know that 'an ap is an apple' expresses this proposition, then we can say without further ado that the sentence is true. However, if we see that the sentence 'some aps grow in Wisconsin' expresses the proposition that some apples grow in Wisconsin, then we can't say, based just on that and the *a priori* knowledge at our disposal, whether the sentence is true, since the proposition it expresses is *a posteriori*. In short, even modest conventionalists, who propose only a finite number of linguistic stipulations, have a problem. They can't say that a sentence is made true solely by the act of adopting a linguistic convention.

\(^{15}\) Although Quine does not object to the idea that some sentences are made true by explicit linguistic convention, he doubts that it makes sense to talk of linguistic conventions that are merely implicit in language use; he is suspicious of talk of conventions that arise without an act of 'convening.' When I declare what my term 'ap' will mean, I do so within a pre-existing language. However, the meanings of terms in the first language a community comes to use cannot arise by people stipulating what their terms will mean.

David Lewis (1969) answered Quine's question by describing how a community could evolve conventional practices without explicitly consenting to an agreement stated in a pre-existing language. Lewis' idea was to use game theory; terms can acquire meanings in the same way that the drivers in a society can come to drive on the same side of the road. It is arbitrary whether we drive on the left or the right, but it is not arbitrary that we all do the same thing. A community of agents who are guided by the wish to cooperate can evolve a communication system in which signs have significance. The question may be raised as to whether the same account applies when individuals are selfish rather than cooperative (Sober 1994b), but the general point remains—there is nothing unintelligible about the idea of conventions coming into existence without explicit stipulations.

Still, as Lewis notes, this defense of the concept of convention does nothing to save the idea that analytic truths are made true by the conventions one adopts. It remains true that bachelors would still be unmarried if English assigned different meanings to the terms 'bachelor' and 'unmarried,' for example.
Boghossian (1996) provides a nice formulation of the present point. In general, the truth conditions for a sentence in a natural language will have the following form:

\[ s \text{ is true in language } L \text{ iff } s \text{ expresses proposition } p \text{ and } p \text{ is true.} \]

Facts about the meanings of terms in a sentence determine which proposition the sentence expresses. However, that isn’t enough to settle whether the sentence is true; there is, in addition, the extra-linguistic question of whether the proposition is true.

It is easy to forget this extra-linguistic contribution when the proposition in question is \textit{a priori}. In the apple example, the fact that an apple is an apple is part of what makes the sentence ‘an ap is an apple’ true. This extra-linguistic contribution to truth value is present in any object-language sentence. We can illustrate this two-part contribution via the following derivation:

‘An ap is an apple’ is true in \( L \) iff an apple is an apple.

An apple is an apple.

............... 

‘An ap is an apple’ is true in \( L \).

The first premiss describes what the quoted sentence means by stating its truth conditions. The conclusion does not follow from this first premiss taken by itself; the second premiss is needed as well, and this premiss states a fact that is language-independent.\(^{16}\)

\(^{16}\) Although it sounds quite intuitive to say, for example, that bachelors would still be unmarried if we adopted different linguistic conventions concerning the use of the term ‘bachelor,’ it is not immediately obvious what is wrong with the following argument:

Because of our linguistic conventions, the English sentence ‘a bachelor is an unmarried man’ is true.

............... 

Because of our linguistic conventions, a bachelor is an unmarried man.

The premiss seems true and the conclusion seems to follow from the premiss by a sort of disquotation principle. However, I claim that the conclusion is false. Where does the argument go wrong? I understand the premiss as follows. It says that

(\*) Because of our linguistic conventions, the English sentence ‘a bachelor is an unmarried man’ expresses some true proposition \( p \).

Notice that the fact brought into being, according to this premiss, involves a relation between the sentence and the class of true propositions. As such, it is similar to the following claim:

Because of something Joe did, his herringbone suit now fits him.

This sentence could be true because Joe went on a diet, or because Joe had his suit altered (or both). The relation got changed by changing one of the \textit{relata}, but the sentence does not say which \textit{relatum} got changed.

The same point holds of (\*). It is true; our adoption of certain linguistic conventions changed something. But what? There are two possibilities. Our behaviour could
In ‘Two Dogmas’, Quine (1953c, pp. 36–37) says that it is tempting to think that ‘the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements’. It should be clear that I do not share Quine’s skepticism about the possibility of effecting the separation he describes. But, in addition, I disagree with the idea that analytic truths are ‘nonfactual’—that there is nothing besides the ‘linguistic component’ that makes them true. Fortunately, the legitimacy of analyticity as a category does not depend on this conventionalist position.

I have suggested that Quine was right in ‘Truth by Convention’ to deny that explicitly adopted conventions can account for the truth of all the logical truths, since there are too many. I then suggested that Quine is too generous; on closer analysis, it is implausible to claim that even one logical truth is true just by virtue of the conventions we adopt. However, even if conventionalism is mistaken as an account of the ‘ground’ of logical truth, it still may be true that there is an irreducibly arbitrary element in our choice of logical principles. The epistemic side of conventionalism remains untouched. In addition, the semantic thesis that logical principles do not have truth values is not undermined by Quine’s argument in ‘Truth by Convention’; just as human conventions cannot make the law of excluded middle true, so the human incapacity to propose an infinite number of conventions cannot show that the law has a determinate and convention-independent truth value. The conventionalist claim that certain statements lack truth values but should be accepted on pragmatic grounds does not require that any sentence is literally made true just by language users adopting linguistic conventions.

have changed which proposition the sentence expresses, or it could have changed the truth value of a proposition. Clearly, the former is correct. This is why the premiss in the quoted argument is true, but the conclusion does not follow.

17. Even though my claim that a definition never suffices to establish the truth of any object-language sentence conflicts with what Quine says in the passage just cited from ‘Two Dogmas of Empiricism,’ it accords with an idea he developed earlier in ‘Truth by Convention’. There Quine (1936, p. 71) says that ‘a definition is incapable of grounding the most trivial statement’. This is because he thinks that a definition is ‘a convention of notational abbreviation;’ ‘a definition is not a premise to theory, but a licence for rewriting theory’ (p. 71). Quine is saying that a definition is a metalinguistic statement that says that two object-language expressions are synonymous, or at least intersubstitutable salve veritate.
VI

Back to Frege. Quine’s target in ‘Two Dogmas’ is Carnap and the logical empiricists, not Frege. However, in the light of the errors we have identified in the idea that analytic truths are true by convention, it makes sense to go back to Frege, whose conception of analyticity was not conventionalist. Frege (1884) said that analytic truths are deductive consequences of definitions. However, as noted above in connection with apples, the truth value of an object-language sentence is never settled solely by describing what the sentence means. The meaning of an object-language sentence is given by stating a biconditional that describes its truth conditions; from this biconditional, taken by itself, the truth of the left-hand side does not follow. However, Frege’s idea easily accommodates this observation. The truth of an analytic (object-language) sentence follows from logical truths when (meta-linguistically formulated) definitions of the terms in the sentence are supplied.

Definitions, as I am using the term, are meta-linguistic statements that describe the semantic properties of expressions or sentences in an object language. This may sound overly restrictive, since it entails, for example, that the object-language sentence ‘a mother is a female parent’ is not a definition. However, notice that the quoted sentence says nothing about English or any other natural language; what the sentence says would still be true even if English never existed. Definitions must be meta-linguistic statements if they are, literally, to describe the semantic properties of terms in a particular language. Without construing definitions in this way, analyticity fails to have anything to do with language.

This Fregean conception of analyticity should be understood in such a way that it is consistent with the apparent fact that very few terms in a natural language can be defined noncircularly in that language. The term ‘triangle’ is an exception; even the term ‘bachelor’ turns out, on reflection, to be such that one can state necessary, but not necessary and sufficient, conditions for the term to apply—perhaps bachelors must be unmarried adult male human beings, but the converse seems not to be true.18

18. It has often been noted that it sounds odd to say that the Pope is a bachelor; it also sounds odd to say of a man who lives out of wedlock with a woman for many years and has children with her, that he is a bachelor (Harman 1996).
However, the case for analyticity doesn’t depend on the existence of definitions in this sense. Carnap (1952a) used the term ‘meaning postulate’ to describe statements that provide analytically necessary or analytically sufficient conditions for the application of a term. Analyticity does not require that the terms in a natural language be definable noncircularly.

Even in the absence of full-scale definitions, analytic truths can be made to fall into the pattern described before in connection with ‘ap’:

‘Bachelors are unmarried’ is true in English iff unmarried bachelors are unmarried.
Unmarried bachelors are unmarried.
....................

‘Bachelors are unmarried’ is true in English.

In such two-premiss arguments, the first premiss describes what speakers of a natural language know if they know what the quoted sentence means. What of the second premiss in these displayed arguments? They express ‘logical truths’, but what are they? Quine restricts logic to first-order logic with identity. I see no reason to be so parsimonious. For example, the following sentence seems to me to be just as analytic as ‘bachelors are unmarried’, but second-order logic is needed to explain why:

If Jack falls down and Jill falls down, then there is a characteristic that Jack and Jill both have.

In any event, I don’t think it is necessary to settle here what logic is with any precision. For now, we can envision a plurality of notions of analyticity, each relativized to its own conception of what makes a truth a logical truth. Perhaps this tolerant pluralism can be whittled down by well-motivated arguments concerning the nature of logic.

VII

Analyticity as Part of a Theory of Linguistic Competence. A cognitivist model of language use seeks to predict and explain

19. It is for this reason that ‘“bachelors are unmarried” is true in English iff grass is green’ is a true biconditional, but not one that can serve to show that ‘bachelors are unmarried’ is analytic.
linguistic behaviour by attributing certain sorts of knowledge to people. It will say that people who are able to speak English have this ability by virtue of their possessing an abstract representation of their language. This knowledge allows them to say when a string of words constitutes a well-formed English sentence and when it does not. In addition, speakers know various facts about what words mean and they know rules that allow them to assign meanings to sentences, based on the meanings assigned to their parts.20

A linguist will want to describe how the knowledge that English speakers possess allows them to understand what English sentences mean. It is not the mission of linguistics to say which of those English sentences is true. Linguists will want to provide an abstract and general theory that assigns truth conditions to ‘there are nine planets’ and ‘there are ten planets’. It is for astronomy, not linguistics, to say which of these sentences is true. The same point holds for the sentences ‘bachelors are unmarried’ and ‘bachelors are married’. The linguist wants a general theory of the semantics of the language that describes what each sentence means; which of them is true is not a problem for linguistics. The legitimacy of the concept of analyticity is thus not completely settled by seeing how theories in linguistics and cognitive science develop. Those empirical sciences will tell us what we should think of the concept of meaning (Putnam 1962), but meaning is only one of the two ingredients that go into the concept of analyticity. The other ingredient is logic.

Still, it is not unreasonable to think that if speakers of a language know what sentences in their language mean, and also know some logic, then they will know (or be able to know), on that basis, that certain sentences of their language are true. Of

20. If theories of meaning are to be thought of in this way—as empirical theories that seek to explain and predict the behaviour of language users—then a standard criticism of verificationist theories of meaning needs to be reconsidered. Verifi
cationism has often been criticized for being meaningless by its own standard—it isn’t a correct definition of what the term ‘meaning’ means in ordinary usage, nor is it empirically testable. Carnap (1936–37) replied that verificationism is a proposal, not a statement that has a truth value; however, critics then were able to say that it is a proposal that they felt no particular obligation to accept. On the view I am suggesting, theories of meaning are no more ‘proposals’ than are theories of planetary motion. Claims about meaning are introduced to explain and predict the behaviour of language users, just as concepts like mass and acceleration are introduced to explain and predict the behaviour of planets.
course, speakers of a language will not have at their disposal the full panoply of what logicians mean by 'logic'. But suppose that some elements of logic are psychologically real—speakers of a language have at least some understanding of the entailment relation. If so, their grasp of their language, along with whatever logical acumen they possess, will allow them to recognize that some of the sentences in their language are true. Knowledge of language allows speakers who know some logic to know that certain object-language sentences are true, where those sentences are not about language at all.

This conception of analyticity leaves open the possibility that some languages may fail to contain analytic sentences. As an admittedly artificial example, consider a 'language' in which the only well-formed sentences are ones that report an object's temperature; each wff has the form 'object x has a temperature of n degrees Centigrade'. A theory of meaning can be provided for this language, one that describes what it takes to be a competent user of this representational system. However, no sentence in this language is analytic. This is enough to show that analytic truths are not an inevitable byproduct of the meaningfulness of sentences. Rather, they are to be found in languages that possess a certain type of expressive power—these languages contain sentences that competent speakers know have logical truths as their truth conditions.

VIII

Knowledge of Language and Knowledge of Extra-Linguistic Propositions. The concept of analyticity builds a bridge between the truth of a sentence in a natural language and a proposition that is not linguistic in its subject matter. For example, if 'mothers are female parents' is an analytic truth in English, then speakers will know the following biconditional (or something like it):

The English sentence 'mothers are female parents' is true iff mothers are mothers.

In principle, knowing this biconditional might allow speakers who already know the left-hand side of the biconditional to come to know the right, or it might permit individuals who already know the right-hand side to come to know the left. Do we know
that the English sentence is true because we know that mothers are mothers, or do we know that mothers are mothers because we happen to know that the quoted English sentence is true? The linguistic theory of the a priori claims that knowledge of language is the source of the a priori knowledge we possess. However, it is entirely possible that the situation is precisely the reverse—that our knowledge of language is predicated on our already having certain items of a priori knowledge at our disposal.

Even if knowledge flows from logic to language in the kind of case just described, there may be other cases in which the direction is reversed. Consider, for example, Cantor’s proof that there are nondenumerably many numbers between 0 and 1. A naive person who is told that there are more numbers between 0 and 1 than there are whole numbers will react with bafflement. However, if you write down a series of decimals, and then show how Cantor’s diagonalization procedure can be used to construct a decimal not on the list, the claim becomes clear. Cantor’s proof helps people see that the proposition about cardinality is true by exploiting certain obvious properties of decimal notation. Try to convince someone of Cantor’s claim by using Roman numerals and you’ll get nowhere. Cantor’s proof allows one to learn something about numbers by examining properties of a notational system.21

Although knowing Cantor’s theorem is facilitated by knowing facts about a language, it is clear that one can’t learn that the theorem is true just by examining facts that are about language and nothing else. After all, Cantor’s theorem is about numbers, and whatever numbers are, they are not language-bound entities. One must know that items of notation refer to numbers for the proof to tell you anything about numbers.

Competent users of a representational system (whether it is a natural language or decimal notation) know that certain biconditionals are true. These biconditionals connect the truth of a representation with a proposition that is extra-linguistic in its subject matter. Knowing such biconditionals can permit knowledge to proceed from left to right, or from right to left. The

21. Clearly, examining a notational system isn’t necessary for knowing that the theorem is true; after all, if you are told by a reliable person that Cantor’s theorem is true, this allows you to know the proposition or to rationally believe it. And proofs of the theorem that do not refer to notational systems can be given as well.
linguistic theory of the *a priori* envisioned a one-way street running from analyticity to *a priori*, but not in the opposite direction. A better picture is a two-way street, with perhaps more traffic flow in one direction than in the other.

Earlier in this paper, I rejected the idea that analytic truths are literally ‘made true’ by the conventional assignment of meanings to words. I also have rejected the quite different, epistemic, idea that analytic truths can be known to be true just by knowing what they mean. A more satisfactory understanding of analyticity is Frege’s—that the truth of an analytic sentence follows from logical truths plus definitions. One virtue of this proposal is its austerity. The proposal does not have packed into it an assumed link between analyticity and *a priori*, nor does it presuppose any special status for logic. These require separate arguments.

IX

*Is Analyticity ‘Epistemologically Significant’?* If meaning is a useful theoretical concept in cognitive science, then there can be no objection to the notion of analyticity, or so a naturalist should be willing to grant. However, philosophers sympathetic with Quine’s critique of analyticity sometimes say that Quine’s real contribution was to deprive analyticity of its ‘epistemological significance’. Of course, even if analyticity were a legitimate scientific notion, it wouldn’t follow that it has any epistemological or philosophical importance. How is this further issue to be addressed?

I take it that the question of ‘epistemological significance’ concerns the issue of *a priori* knowledge. If a sentence is analytic, then what the sentence says will be *a priori*, if logic is *a priori*. As described in our various examples about bachelors, mothers, and apples, the biconditional that specifies the meaning of an

22. Boghossian (1996) also separates these metaphysical and epistemic conceptions of analyticity, but he endorses the latter.

23. If a sentence is analytic, then the fact that the *sentence* is true will not be decidable *a priori* if it is an empirical question what the sentence means in the particular (socially shared) natural language in question. Perhaps sentences that are analytic in a speaker’s idiolect can be known by that speaker to be true *a priori*, but that is another matter. In any event, for socially shared languages, the connection between analyticity and *a priori* is effected via the *proposition* that the sentence expresses.
object-language analytic truth will connect that sentence to a logically true proposition. Thus, the philosophical interest of ‘analysis’, if it has any, consists in the fact that it provides access to the *a priori*. This is what analytic philosophers care about that separates them from lexicographers. If there is no such thing as *a priori* knowledge, then analyticity loses its philosophical interest, however legitimate the concept of meaning might be in cognitive science. It is to the concept of the *a priori* that we now must turn.

X

*What Is Epistemological Holism?* Quine’s critique of analyticity aimed to undermine the linguistic theory of the *a priori*, but this critique, even if successful, would leave open the possibility that there are propositions that can be known or justified independently of experience. Quine’s epistemological holism is intended to shut down that possibility. Not only can’t the concept of analyticity be used to show that there is *a priori* knowledge; nothing else can either, because there is no such thing.

The distinction between *a priori* and *a posteriori* is intended to separate two types of knowledge or justification. We can make rational judgments about the truth values of *a posteriori* propositions only by appealing to the testimony of the senses; however, for *a priori* propositions, the rational justification for assigning truth values does not depend on sense experience. What, then, does it depend on? The usual answer is that ‘rational reflection’ of some kind on the content of the proposition suffices to justify an assignment of truth value. This reflection might involve noticing that a sentence that expresses the proposition is analytic, as the logical empiricists thought; the Kantian alternative is that it might involve noticing that the proposition is synthetic, but is a presupposition of the possibility of empirical knowledge.

The contrast between *a priori* and *a posteriori* is often thought to coincide with a contrast between beliefs that are certain and beliefs that are not. However, nothing I have said about *a priority* implies that this is so. Suppose I know that triangles have three sides by examining the content of that proposition—by analyzing its constituent concepts. I therefore bypass the use of sense experience with its attendant sources of error. However, it is
entirely possible that the introspective examination of one's concepts is prey to unreliabilities of its own. Various philosophers have claimed that the exercise of 'reason' yields infallible results, but I do not. The *a priori/a posteriori* distinction simply distinguishes two sources of knowledge or justified belief.

If the *a priori/a posteriori* distinction makes sense, then we should be able to divide the different propositions we believe into two classes, each having its own distinctive sort of support. Quine's holism denies that this is possible. For Quine, the totality of what we believe is supported by the totality of the sense experiences we have. We adjust our total theory of the world by bringing it into accord with the sense experiences we have and also by making it conform to requirements such as logical consistency, simplicity, elegance, generality, and tractability. Logical reflection and sense experience jointly shape this total theory; it is a mistake to think that some of our beliefs are justified just by rational reflection while others depend for their justification on sense experience.

An analogy may help convey the character of what Quine is claiming. Suppose a husband and wife deposit their salaries into a joint checking account from which they pay their bills. Does the wife's salary cover the car insurance, while the husband's takes care of the groceries? In some marriages this division may occur, but in the one I am imagining, it does not. The expenses as a whole are covered by the two paychecks together. When their income shrinks or grows, and when prices rise and fall, they revise their list of what they buy. But individual purchases never trace back to one paycheck rather than the other.

Quine's holism separates his position from that of Mill, according to whom propositions that we might think are *a priori* are in fact empirical. Mill (1859) thought that our belief in simple arithmetic propositions like $2 + 3 = 5$ is based on the experiences we have when we count. Arithmetic is *a posteriori*. However, for Mill, the justification that we have in believing that $2 + 3 = 5$ can be separated from the justification we have for believing that grass is green; both are experiential, but different experiences justify different beliefs. This isn't holism, but a kind of epistemological atomism. Mill's position makes it clear how epistemological holism goes beyond the simple denial that there are *a priori* truths.
XI

Centrality and Minimum Mutilation. If our beliefs ‘face the tribunal of sense experience not individually but as a corporate body’, what determines how we should revise our beliefs when we encounter ‘recalcitrant experience’? For example, what do we do if we expect it to be snowing when we look out the window, but don’t have the experience of whiteness that we expected to have? We need to revise our beliefs, but which ones should we change? Why do we revise the belief that it is snowing, rather than the belief that our eyes are functioning properly, or the belief that snow is white, or still others? Quine says that our beliefs differ from each other in terms of their ‘centrality’, and that we follow a policy of ‘minimum mutilation’—we revise less central beliefs rather than more central ones. Our logical and mathematical beliefs are the most central beliefs we have, which is why we rarely if ever are tempted to revise them in the light of experience. However, this is not because they enjoy some special sort of nonempirical justification. Logic and mathematics differ in degree, not in kind, from the beliefs we have that clearly seem to be empirical; our belief in the validity of modus tollens is just more central than our belief that it is snowing.

What does ‘centrality’ mean? Quine’s examples of logic and mathematics suggest that centrality has to do with the importance a belief has in organizing experience, which in some measure is influenced by the belief’s generality. We don’t abandon logical principles of inference when we expect to see snow but don’t; the reason is that logic applies to lots of other situations besides this one, and moreover plays an important role in organizing how we think about those situations. There are several problems with this suggestion. First, even though modus tollens is central in the sense just described, it isn’t true that every theorem in pure mathematics is like this. A result in some recondite area of algebraic topology, for example, might play little or no general role in organizing how we think about the world. Why, then, are such theorems accorded so great a degree of immunity from revision in the light of recalcitrant sense experience? The same point applies to trivial analytic truths. Surely the belief that bachelors are unmarried isn’t central to our understanding of the world; it
is utterly unlike *modus tollens* in this respect (Putnam 1962, p. 39; Sidelle 1989). A second problem for Quine's proposal derives from the fact that general beliefs that organize our interpretation of wide swaths of experience are sometimes revised in the light of observations; this is what happens when scientists reject a general theory because they think that the evidence obliges them to do so. Why doesn’t the principle of minimum mutilation dictate that we should always assume that the data are misleading when the evidence seems to disconfirm a theory that is central to our system of beliefs? The fact of the matter is that scientists often reject theories in the light of data, but they sometimes retain a theory in the face of contrary evidence, conjecturing that the data must be misleading in some way they can’t quite figure out (this is roughly what Kuhn 1970 meant by an ‘anomaly’). The rule of minimum mutilation isn’t sufficiently elaborated for it to help illuminate this important phenomenon. Lastly, suppose the concept of centrality were sufficiently clear that we could see that logic and mathematics are the most central of our beliefs. The question then arises as to how the claim that logic and mathematics are maximally central differs from the claim that mathematics and logic are *a priori*. If rational revisability in the light of sense experience is governed by the principle of minimum mutilation, in what sense are our most central beliefs empirically revisable?24

**XII**

_The Holistic Concept of Confirmation._ Quine’s holism has been extremely influential in philosophy generally, but perhaps most influential in philosophy of mathematics. Many philosophers hold that mathematical statements are empirically well supported because they play an indispensable role in natural science (Putnam 1971; Kitcher 1983; Maddy 1990; Hellman 1992,

24. To these criticisms, it might be replied that Quine never pretended that the principle of minimum mutilation is a complete epistemology. My response is that if the principle is just one of the considerations that governs belief revision, then it is left open whether Quine is right when he says that all beliefs are empirically revisable.
forthcoming; Resnik 1992, 1995). Calculus, for example, might appear to be an *a priori* subject, but the fact that it is used in predictively successful physical theories means that calculus enjoys the empirical confirmation that those theories have received. That calculus is used in relativity theory means that the empirical evidence that supports relativity theory also accrues to calculus; but the fact that calculus is used in Newtonian theory does not mean that the empirical disconfirmation of Newtonian theory also accrues to calculus. Ronald Reagan used to be called 'the Teflon President'—accusations of malfeasance never seemed to stick to him, whereas he was often given credit for the good things that happened while he was in office. Quinean philosophy of mathematics holds that mathematics has the same Teflon quality—it is empirically confirmed by the good theories in which it is used, but it is not empirically disconfirmed by the bad theories in which it also is used (Sober 1993, 1999).

The confirmation relation that holism invokes is *bizarre*. Consider a perfectly mundane example. I draw a card at random from a standard deck of cards without looking at it. The probability that it is the seven of hearts is 1/52. You then inform me that the card is red. This information confirms the hypothesis that the card is the seven of hearts, not in the sense of insuring that the hypothesis is true, but in the sense of making the hypothesis more plausible than it was before; the probability that I have the seven of hearts has just increased to 1/26. However, this information does not confirm the hypothesis that the card I hold is a seven; the probability that I have a seven remains what it is.

25. Maddy (1992) criticizes the indispensability argument; Field (1980) accepts the Quinean assumption that mathematics would be justified if it were indispensable, but argues against the antecedent. Peressini (1997) identifies issues that need to be addressed in the indispensability argument concerning the relationship between pure mathematics and mathematized natural science that are separable from the question of confirmational holism.

It is worth noting that Quine has recently changed his mind about the holistic status of mathematics. In *From Stimulus to Science*, Quine writes:

The accepted wisdom is that mathematics lacks empirical content. This is not contradicted by the participation of mathematics in implying [observational] categoricals for... such participation does not confer empirical content. The content belongs to the implying set, and is unshared by its members. I do, then, accept the accepted wisdom. No mathematical sentence has empirical content, nor does any set of them (Quine 1995, p. 49; quoted by Gibson 1998, pp. 677–678).

In his reply to Gibson, Quine (1998, p. 685) confirms that his present view is that 'mathematics lacks empirical content.'
was, namely $1/13$. The following pattern of inference (which Hempel 1965, p. 31 called ‘the special consequence condition’ of confirmation) is fallacious:

\[
\begin{align*}
O \text{ confirms } H & \quad O = \text{the card is red} \\
H \text{ entails } S & \quad H = \text{the card is the seven of hearts} \\
\cdots & \quad S = \text{the card is a seven} \\
O \text{ confirms } S & 
\end{align*}
\]

Yet, holism maintains that when an observation confirms relativity theory, this observation also confirms the pure mathematics that is used in relativity theory.

The example just described has a natural Bayesian representation. Bayesians say that $O$ confirms $H$ precisely when $\Pr(H|O) > \Pr(H)$ and that $O$ disconfirms $H$ precisely when $\Pr(H|O) < \Pr(H)$; confirmation means probability raising and disconfirmation means probability lowering. In the card example, $O$ raises the probability of $H$ but it does not raise the probability of $S$, even though $H$ entails $S$. Although it is convenient to use a Bayesian model of confirmation to analyze this example, the force of the example does not depend on Bayesianism’s being adequate in general. It is enough that Bayesianism makes sense in the case at hand.

Another property of simple examples of confirmation is that there is a kind of symmetry between confirmation and disconfirmation. If observing that the card is red confirms the hypothesis that the card is the seven of hearts, then, if the card had not been red, that observation would have disconfirmed the hypothesis. This idea also can be given a Bayesian representation. It follows from Bayes’s theorem that

\[
\Pr(H|O) > \Pr(H) \text{ if and only if } \Pr(H|\neg O) < \Pr(H). 
\]

$O$ confirms $H$ if and only if $\neg O$ would disconfirm $H$. Yet, holism maintains that the observational outcomes that are said to confirm propositions of pure mathematics, had they been different, would not have disconfirmed those propositions (Musgrave 1986; Sober 1993, 1999).²⁶

²⁶. Hellman (1999) attempts to rescue Quinean holism from these problems by formulating it as the following claim, which he calls ‘moderate holism’. When $M&L&A$ entail $O$ and $O$ is observed to be true, then $O$ confirms the conjunction $M&L&A$. Here $M$ is a set of pure mathematical axioms, $L$ are lawlike statements in a scientific theory, and $A$ are auxiliary statements. Hellman notes that this condition ‘typically holds’ in Bayesian confirmation theory;
Quine's holism has a consequence that is even more radical. If I believe relativity theory, and this theory is confirmed by some observation that I make, then *everything* I believe is also confirmed. To say otherwise is to say that the observation impinges only on part of what I believe; my total system of beliefs then would not have confronted the tribunal of experience as a corporate body. So if I believe X and Y, anything that confirms X also confirms Y, even when X and Y are thoroughly unrelated. In fact, if I believe X and Y, and you believe X and not-Y, then anything that confirms X confirms Y *for me*, whereas anything that confirms X confirms not-Y *for you*. This is implausible, and not just on the Bayesian model.

**XIII**

*Duhem's Thesis.* Although the original 1951 version of 'Two Dogmas' that was published in *Philosophical Review* did not mention Duhem's thesis, the thesis is referred to in a footnote in

in fact, it holds if Pr(O) and Pr(M&L&A) are each less than 1. But this raises a problem. Even if M were completely irrelevant to L and to O, the conjunction would still have its probability go up in the circumstances described. Hellman sees that if M is to be confirmed, it must be shown that this conjunct has its probability raised by O; it isn't enough that M is part of a conjunction whose probability increases. 'How then,' he asks, 'can evidential support for a body of theory including mathematics result in a boost to the mathematical component?'

His answer is this: 'it depends on the prior credibility of the mathematical assumption(s). If M is already certain, of course it does not need further "confirmation". But if it is less than certain... it can benefit from empirical support of bodies of theory...' Hellman adds that one also must consider the fact that the mathematical component M helps other theories besides L with other auxiliary assumptions besides A to make accurate predictions, and that this further confirms M. As a third point, Hellman considers a disjunction of theories (∨T,) in which each disjunct entails M; Hellman points out that Pr(M|O) ≥ Pr(∨T|O).

None of these three points answers the question that Hellman poses. Even if M is less than certain, it still may fail to be confirmed by O. And the fact that M helps other pairs of theories plus auxiliary assumptions to make accurate predictions merely raises for them the same question that arises with respect to L and A. Having lots of the same thing provides lots of confirmation only if each test provides some confirmation, and this has not been shown. Finally, Hellman's point about the disjunction ∨T does not show that Pr(M|O) > Pr(M).

Hellman also says that mathematical propositions are confirmed by their role in empirically successful theories, but are not disconfirmed by their role in empirically unsuccessful theories. However, as noted above, Bayes's theorem entails that there is a symmetry between confirmation and disconfirmation. In addition, Hellman says that his position is consistent with there being propositions of pure mathematics that are immune from empirical revision. As he realizes, this is a substantial retreat from Quine's position. That some propositions of pure mathematics (like the ones that Plateau investigated) can be tested empirically is not in dispute.
the version of the paper later published in *From a Logical Point of View* (Quine 1953c, p. 41). Quine (1991, p. 269) says that he ‘didn’t know about Duhem’ when he wrote ‘Two Dogmas’, but inserted the footnote at the prompting of Hempel and Philipp Frank. In that footnote, he equates his epistemological holism with Duhem’s thesis.

Duhem (1914) made a claim about hypotheses in physics—taken on their own, they do not have observational consequences. If physical hypotheses are to make testable predictions, they must be conjoined with ‘auxiliary assumptions’. Even though H by itself does not entail any observation statements, the conjunction (H&A) may do so. Quine generalized Duhem’s point—what is true of physical hypotheses is true of all our beliefs; no hypothesis is ‘testable in isolation’. I do not doubt the logical point with which Duhem and Quine begin. However, epistemological holism does not follow.

The logical point has implications about what happens when a prediction fails to come true. If (H&A) entails O, and not-O turns out to be true, then there is a choice; one might abandon H, or abandon A, or abandon one or more logical principles. Quine thinks that the fact that we face a choice here shows that all our beliefs are potentially at risk when we make observations. We could abandon our logical and mathematical beliefs, rather than our physical theories, when predictions fail to come true. Only pragmatic considerations reflecting our allegiance to the policy of minimum mutilation lead us to do otherwise.

It is often replied that this flexibility would disappear if we had independent reason to believe the background assumptions A—if the conjunction (H&A) entails a false prediction, and we know that A is true, then the culprit must be H, which we then reject. To this, Quine might reply—why should our prior conviction that A is true continue to hold sway? Why shouldn’t the failure of our current prediction be a reason to re-evaluate A? Here is a way to think about testing that answers this question: Typically, scientists test a hypothesis by testing it against another hypothesis; testing is a comparative process in which one tries to find observations that favour one hypothesis over another (Sober 1994a, 1999). The logical point about the need for background assumptions is entirely correct. However, the typical situation in science is that the competing hypotheses make use of the same
set of background assumptions. The conjunction (H₁ & A) and the conjunction (H₂ & A) both make predictions. However, these conjunctions do not deductively entail what they predict. Rather, since observation is always subject to error, what happens is that these conjunctions confer probabilities on observations. The conjunction (H₁ & A) confers a probability on the observational outcome O, and the same is true of the conjunction (H₂ & A). We now can define the idea that an observational outcome O favours one hypothesis over another:

Given background assumptions A, O favours H₁ over H₂ if and only if \( \text{Pr}(O|H₁, A) > \text{Pr}(O|H₂, A) \).

If O is very probable according to (H₁ & A) and very improbable according to (H₂ & A), then O strongly favours H₁ over H₂. If the probabilities are closer together, the degree of favouring is less decisive. The point to notice is that O's favouring H₁ over H₂ says nothing about whether O favours A over some alternative auxiliary assumption. The experiment just described does not test A at all because it does not test A against an alternative. The usual role that mathematics and logic play in empirical science is to furnish a common set of background assumptions, which all the competing hypotheses exploit. This is why logic and mathematics are not tested in typical scientific experiments (on this point, see Popper, 1963, p. 112).

For logic and mathematics to be tested empirically, one logical or mathematical statement would have to be pitted against an alternative and a framework of shared background assumptions would have to be supplied that permits the two statements to make different predictions about observations. It is instructive to examine a case in the history of mathematics in which this actually happened. Plateau was a 19th century French mathematician

27. Friedman (1997, p. 12) claims that the ‘fundamental problem’ with Quinean holism is that it does not make sense to view a mathematized theory like general relativity as a conjunction in which the pure mathematics is isolated in one of the conjuncts; ‘rather, the mathematical background of Einstein’s theory functions as a necessary presupposition of that theory...’ Holism, I would say, does not require this partition; in effect, Hempel’s special consequence condition can be thought of as expressing the content of holism. Friedman then goes on to note that Newtonian physics, special relativity, and general relativity are all now thought of as different theories that share the same mathematical machinery. It is because that machinery is common across these theories that the empirical evidence that favours one theory over another does not confirm (or disconfirm) the shared mathematical presuppositions.
who wanted to figure out what the surface of least area is that fills various closed curves. A simple example of the type of problem that Plateau had in mind is a curve that has the shape of an ellipse. An elliptical curve can be filled by the ellipse it contains; any other surface that fills the curve will bulge into a third dimension and so must have more surface area. Although the answer to Plateau’s question is obvious for this example, the answers aren’t at all obvious for more complicated three-dimensional curves. What Plateau did was to dip wires bent in different shapes into soapy water and observe the resulting soap bubbles that adhered to the frames when they were removed from the water (Courant and Robbins 1969). Given the physical assumption that soap bubbles take on the surface of least area in this experiment, different mathematical hypotheses (’the surface of least area for curve C is $s_1$’ versus ‘the surface of least area for curve C is $s_2$,’ for example) make different predictions. This example is interesting in the history of mathematics precisely because it is so atypical. No such test of ‘$2 + 3 = 5$’ against alternative arithmetic hypotheses has ever been carried out, nor is it remotely clear what this would be like.

Quine suggests in ‘Two Dogmas’ that the alternative to epistemological holism is the idea that single sentences have their confirmation conditions separately—that one should be able to say, for each hypothesis, which observations would confirm it and which would infirm it. This is the second dogma of empiricism that Quine rejects. However, there is no need to choose between epistemological holism and this positivist idea. Confirmation and disconfirmation are three-place relations, not two-placed—hypothesis H is confirmed or disconfirmed by observation O relative to background assumptions A. This logical point does not force one to say that anything that confirms/disconfirms H also confirms/disconfirms A (see also Creath 1991, pp. 379–382).28

28. Although there are many passages that suggest that Duhem was indeed an epistemological holist, the following passage suggests a rather different interpretation:

The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us. The physicist may declare that this error lies in exactly the hypothesis he wishes to refute, but is he sure it is not in another proposition? If he is, he accepts implicitly the accuracy of all the other propositions he has used, and the validity of his conclusion is as great as the validity of his confidence (Duhem 1914, p. 185).
XIV

Moderate Holism. In various writings since ‘Two Dogmas’, Quine (1960, p. 13; 1975, p. 314; 1986, p. 427; 1991) has mentioned that this essay overstates his epistemological holism (Creath 1991; Gibson 1998). The claim should not be that the totality of one’s system of beliefs faces the tribunal of sense experience as a corporate body, but that largish sets of beliefs do so. The most minimal sort of holism concerns ‘clusters of sentences just inclusive enough... to imply an observable effect of an observable experimental condition (Quine 1991, p. 268)’.29 It is interesting that all forms of holism, including even this most moderate formulation, are subject to the criticisms just adumbrated.

Suppose two biologists disagree about the age of a fossil. Mr. One believes the hypothesis (H₁) that the fossil is at most 50 million years old; Ms. Two believes H₂, which says that the fossil is at least 60 million years old. They agree that potassium-argon dating is a reliable technique in general and that the potassium-argon dating device they have in their lab is in good working order. This auxiliary assumption (A) permits the two hypotheses to issue in different predictions about what the device will say.

The test is performed, and the device says that the fossil is at least 70 million years old. Mr. One has his belief in H₁ disconfirmed and Ms. Two has hers in H₂ confirmed. What happens to their shared assumption about potassium-argon dating in general and the device in particular? According to Quinean holism, the same observation disconfirms A and also confirms A, for A is conjoined with H₁ in the mind of Mr. One while A is conjoined with H₂ in the mind of Ms. Two.

Notice that Duhem here does not prohibit the inference that a physical hypothesis is false or that it is disconfirmed by an observation. He simply points out that this conclusion depends on one’s being willing to say that the auxiliary assumptions are correct. And the point about auxiliary assumptions is not that one can never have evidence that they are true, but that one cannot be absolutely certain that they are. All this is rather unholistic.

29. The shift from radical to moderate holism makes it important to consider the mathematical propositions that are not used in empirical natural science. What is their epistemological status? Quine (1990, p. 95) gives this answer: ‘The main axioms of set theory are generalities operative already in the [empirically] applicable part of the domain. Further sentences... can still be submitted to considerations of simplicity, economy, and naturalness that contribute to the molding of scientific theories generally.’ Quine goes on to remark that these considerations justify the axiom of choice and the continuum hypothesis.
There is no difficulty in imagining that these two scientists might react in opposite ways to the device’s performance. Perhaps Mr. One has his confidence in potassium–argon dating decline, while Ms. Two has hers increase. This is a familiar human reaction—people sometimes increase their confidence in messengers who bring good news, and sometimes they doubt the reliability of messengers (and even want to kill them) when the news is bad. Ego intrudes in science, just as it does in other walks of life. However, confirmation theory has always tried to separate the confirmation relation that obtains among propositions from the psychological quirks that may attach to people. Surely it is perfectly possible that the auxiliary assumptions in this example are independently confirmed, that both parties rationally agree that this is so, and that the status of these shared background assumptions is entirely unaffected by the experimental result. This cannot happen according to Quine’s holism (whether it is extreme or moderate), but surely it can and does (Creath 1991, p. 380).30

If Mr. One’s confidence in A and his confidence in H₁ crumble simultaneously as a result of the test, what can he say was his reason for interpreting the observation as providing a reason for rejecting H₁? It can’t be that he thought and continues to think that potassium–argon dating is a reliable method and that the device in his lab works properly, for this is just what he has abandoned. Rationally rejecting a hypothesis formerly believed typically requires that one not abandon the auxiliary assumptions that are used to test the hypothesis. Holism must be false for this to occur.

Quine would interpret the behaviours of our two scientists by saying that they embrace various pragmatic principles, including the principle of minimum mutilation. I have already expressed my reservations about the ideas of centrality and minimum mutilation. I now want to focus on the word ‘pragmatic’. What does this word mean, and what does it exclude? As I mentioned in

30. The fact that the auxiliary assumptions A are rationally retained by both parties in this example is not, by itself, a counterexample to the most modest sort of holism, which is the one that Quine endorses in the passage I quoted. Minimal holism says that it is H₁ and the conditional (if H₁ then O₁) that stand or fall together and that the same is true of H₂ and the conditional (if H₂ then O₂). The conditionals (if H₁ then O₁) and (if H₂ then O₂) are each weaker than the auxiliary assumption A. However, everything said about A applies also to these two conditionals.
the first part of this paper, I take my cue from Pascal's wager; 'pragmatic' means *prudential*, and it contrasts with *evidential*. With respect to Mr. One and Ms. Two, I claim that it isn't just 'useful' for them to think, before and after the experiment, that potassium–argon dating is reliable and that their laboratory device is in good working order. They have good evidence that this is so before the experiment is performed, and the result of the experiment does not provide any evidence that these auxiliary assumptions are false. The experiment tests one hypothesis about the fossil's age against another; the auxiliary assumptions are *used*, but they are not *tested*.

**XV**

*Nondistributive Holism.* I so far have construed holism, whether it is moderate or radical, as obeying a distributive principle—if a conjunction is confirmed or disconfirmed, so are each of its conjuncts. What is true of the whole must be true of the parts. We have seen that this epistemological principle is unsatisfactory, regardless of how inclusive the conjunctions are supposed to be. However, there is a different way to formulate epistemological holism, one that denies distribution. The idea is that confirmation and disconfirmation accrue *only* to sufficiently inclusive conjunctions; what is true of the whole cannot be affirmed or denied of the parts. This nondistributive version of holism is inconsistent with the Quinean views one finds so often in philosophy of mathematics. One will not be able to say that propositions of pure mathematical are confirmed when they help empirical theories to make accurate predictions. Still, nondistributive holism leads to the conclusion that mathematics and empirical hypotheses are in the same boat, and that might be symmetry enough to vouchsafe the claim that mathematics is just as empirical as natural science. 31

In any event, the story of Mr. One and Ms. Two shows that nondistributive holism is implausible. Before the experiment, Mr.

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31. It is interesting that hypothetico-deductivism—the view that an observation sentence O confirms (disconfirms) X iff X deductively implies O and O is true (false)—leads to nondistributive holism. If (H&A) entails O, but neither conjunct entails O on its own, then O would confirm (and not-O would disconfirm) the conjunction, but neither conjunct, according to hypothetico-deductivism. Resnik (1997, p. 115) endorses hypothetico-deductivism in his defense of epistemological holism.
One has a certain degree of confidence in the conjunction $H_1 \& A$; afterwards, that degree of confidence has declined. Ms. Two likewise begins with a certain degree of confidence in $H_2 \& A$, which increases as a result of the experiment. There is absolutely no reason to believe, either from what Quine has written or on any other basis, that distribution must be a mistake. As mentioned above, it can be perfectly reasonable for the two participants to think that the auxiliary assumption about potassium–argon dating is not tested in this experiment, and so their confidence in $A$ remains unchanged; what happens is that the plausibility of $H_2$ increases and that of $H_1$ declines.

If the probability of a conjunction goes up or down and the conjuncts are probabilistically independent of each other, then at least one of the conjuncts must have its probability move in the same direction. However, it is false that each conjunct must move in the same direction as the conjunction. This is why Bayesianism is inconsistent with nondistributive and distributive holism alike.\textsuperscript{32} Again, the lesson I draw from this does not depend on one's accepting Bayesianism as a completely adequate general account of confirmation. Bayesianism is uncontroversial in limited domains and holism of both sorts is therefore false.

XVI

A Duhemian Definition of Empirical Unrevisability. It is perfectly consistent with the correct logical point with which Duhem and Quine began that there might be propositions that are empirically un revisable. If we use the idea mentioned earlier that testing is a contrastive activity—that one tests a hypothesis $H_1$ by testing it against a competing hypothesis $H_2$—then we can formulate the following definition:

The choice between competing hypotheses $H_1$ and $H_2$ is empirically undecidable, within the context of background beliefs $B$, iff, for all observations $O$, $\Pr(O|H_1 \& B) = \Pr(O|H_2 \& B)$, or else the probabilities are undefined.\textsuperscript{33}

\textsuperscript{32} This point about Bayesianism is not new; see, for example, Carnap (1950, pp. 392–393).

\textsuperscript{33} The last clause (that the probabilities may be undefined) is included because some questions may be empirically undecidable within the context of a set of background beliefs because those background beliefs don’t allow the competing hypotheses to confer probabilities on observations.
No observation can favour one hypothesis over the other in this circumstance.

My reason for focusing on the difference between empirically decidable and empirically undecidable questions, rather than propositions, can be gleaned by considering the following three statements:

- There is a printed page in front of you.
- There is a salami in front of you.
- There is no printed page in front of you, but an undetectable evil demon is causing you to have just the experiences you would be having if there were a printed page in front of you.

Is the first statement empirical? Well, it is possible to cite experiences that discriminate between it and the second statement. However, there is no experience that discriminates between the first statement and the third. Whether a proposition is empirical depends on what you take its competitors to be (Sober 1994a, 1999).

Notice that the concept of empirical undecidability just defined is not coextensive with the idea that a proposition can be justified by reason alone, independently of experience. For one thing, the choice between a proposition and its negation might be empirically undecidable and not decidable by rational reflection, either. For another, a proposition might be both empirically decidable and a priori, as the example of Plateau's soap bubble experiment illustrates (see also Rey 1998).

The notion of empirical undecidability just defined might be generalized. Perhaps there are questions that are not only empirically undecidable with respect to a single set of background beliefs, but ones that are empirically undecidable relative to a range of possible sets of background beliefs. The strongest such concept would be that of a question that is empirically undecidable relative to all possible sets of background beliefs. This is an empty category,\(^{34}\) and not of much interest. However, a more interesting concept can be obtained as follows. Let's begin with a censored version of our current set of beliefs; let \(B_0\) consist

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34. Simply let \(B\) be the pair of conditionals if \(H_1\) then \(O\) and if \(H_2\) then not-\(O\), where \(O\) is some observation. Trivially, the choice between \(H_1\) and \(H_2\) is empirically decidable, relative to that background assumption.
of all the beliefs we have, minus the ones that depend for their justification on our believing $H_1$ rather than $H_2$ (or vice versa). $B_0$ is the set of background beliefs that people might now use to see whether observations favour one hypothesis over the other. As we make further observations, we rationally modify $B_0$ and so construct a new set of beliefs $B_1$; this, in turn, will be modified by new experience to yield $B_2$, and so on. We now have a sequence of sets of background beliefs $B_0$, $B_1$, $B_2$, etc. Each of these is justified by the observations available at the time, and none of them begs the question of whether $H_1$ or $H_2$ is more plausible. Perhaps there are questions that are not only empirically undecidable now, relative to our present set of beliefs $B_0$, but which will remain so, as our background beliefs are modified in the light of experience.\footnote{The fact that hypotheses are able to predict observations only via the mediation of background beliefs does not show that this is impossible.} XVII

Duhemian Circumspection. Although the logical point that hypotheses make observational predictions only when supplemented by background assumptions cannot be said to vindicate the Quinean contention that every belief is revisable in the light of experience, it does suggest that a certain humility is in order when one is asked whether observations could ever justify rejecting this or that hypothesis. It is difficult for us to imagine how there could be consistent alternatives to "$2 + 3 = 5$" that

\footnote{It also is possible that the choice between $H_1$ and $H_2$ is empirically decidable now, but turns out not to be so at a later date. Shifts in both directions are possible.}

\footnote{Skyrms (1984) suggests that a 'metaphysical' proposition $H$ is one such that, for all observations $O$, $\Pr(H|O) = \Pr(H)$. Since Skyrms is a subjective Bayesian, he is content to have a proposition be metaphysical for one person, but not for another. As explained above, I prefer to define the notion of empirical undecidability as a property of questions, not of propositions. Furthermore, my construal does not require that probabilities be assigned to hypotheses. Grice and Strawson (1956) and Creath (1991) both describe a Duhemian notion of confirmational equivalence and suggest that it might provide a definition of synonymy that breaks out of the vicious circle that Quine criticized. I doubt that this is the concept of meaning that cognitive science requires; it is too verificationist. Confirmationally equivalent sentences can differ in meaning.}
make different probabilistic predictions about observations. However, as the historical example of non-Euclidean geometry suggests, this may simply be due to our lack of imagination, which future generations of investigators perhaps will be able to overcome. Putnam (1983, pp. 90–91) thinks that this is the deep objection that ‘Two Dogmas’ develops against the idea of a priori knowledge. However, this point does not establish that it could make sense to abandon the belief that $2 + 3 = 5$ in response to an experimental outcome. The fact that we can’t be certain that a proposition is immune from empirical revision does not show that the proposition is, in fact, empirically revisable.

Likewise, it may seem obvious that two hypotheses are empirically equivalent, but we should take to heart the possibility that new auxiliary assumptions will be discovered that reveal that the two hypotheses in fact make different predictions. Even if $(H_1 \& B_o)$ and $(H_2 \& B_o)$ make the same predictions, it is conceivable that a new set of auxiliary assumptions $(B_n)$ will be adopted with the result that $(H_1 \& B_n)$ and $(H_2 \& B_n)$ make different predictions. But once again, that this is conceivable does not show $H_1$ and $H_2$ in fact make different predictions. All that follows is that our ability to determine whether the two hypotheses are empirically equivalent may be limited by our lack of omniscience concerning the future of science.

These Duhemian admonitions also apply within the realm of the a priori. As Russell’s paradox made vivid, the fact that a mathematical postulate seems to be self-consistent is no guarantee that it is. Creative work in mathematics can alter the epistemological status of a mathematical proposition, just as creative work in empirical science can change the status of a physical proposition. However, it would be wrong to conclude from this that no proposition is, in fact, a priori. Russell’s paradox and its various remedies were developed without the mediation of empirical evidence. The fact that we can be mistaken in our judgments about what is a priori true doesn’t show that nothing is a priori.

XVIII

Conclusion. Although Quine attacked two ‘dogmas’ that he saw in the work of the logical empiricists, I think Quine’s philosophy

37. Although contradictions entail everything in classical logic, $\Pr(O|H)$ is not defined if $\Pr(H) = 0$. 
embodies two ‘dogmas’ of its own. The first is Quine’s reduc-
tionism with respect to meaning and modality (Grice and Straw-
son 1956; Stein 1992, p. 277). Analyticity is a legitimate concept
only if it can be defined in terms of concepts that Quine regards
as ‘clear’; he imposed the same requirement on the concept of
necessity. The second Quinean dogma is epistemological holism
(Creath 1991, p. 351, p. 384)—that, in the first instance, it is larg-
ish conjunctions of beliefs that are confirmed and disconfirmed;
what holism says happens to the conjuncts in those conjunctions
depends on whether the doctrine is formulated distributively or
nondistributively.

The objections I have presented to these two Quinean
dogmas are different in character. Against the first, I mustered
a characteristically Quinean consideration. The fundamental
question about the concept of meaning is whether it is needed
to explain and predict. If it is, and if meaning turns out not
to be reducible in the way that Quine demands, so be it. I
criticized the second dogma by adducing some elementary
considerations about the confirmation relation. Quine’s dis-
cussion of ‘minimum mutilation’ and of belief systems facing ‘the
tribunal of experience as a corporate body’ needs to be made
more precise. When even the first steps are taken in this
enterprise, serious problems arise.

The linguistic theory of the a priori was very ambitious; it
sought to account for all instances of a priori knowledge by
tracing them back to facts about language. Quine began to
dismantle that imposing edifice in ‘Truth by Convention’; ‘Two
Dogmas of Empiricism’ is widely thought to have completed
the demolition job, but, in my opinion, it dislodged not a single
brick. Not that my criticisms of Quine’s criticisms amount to
a defense of the views that Quine was attacking. Analyticity
should not be understood to mean truth-by-convention. Nor
should we think that isolated statements are confirmed or
disconfirmed by observations in the absence of background
assumptions. If cognitive science produces predictive and
explanatory theories that attribute to speakers a knowledge of
what their terms mean, then the concept of meaning is on safe
footing, and so is the concept of analyticity. And even though
Duhem and Quine are right that auxiliary assumptions are
needed to bring hypotheses into confirmational contact with
observations, there still may be hypotheses that cannot be tested observationally, either now, or as science grows.\textsuperscript{38}

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