

## FIVE

### Quine's Reasons for Rejecting Carnap's Analytic-Synthetic Distinction

#### 62. A Strategy for Clarifying Quine's Dispute with Carnap

In Quine's presentation of his objections to Carnap's analytic-synthetic distinction, he takes care to avoid giving the impression that he and Carnap are talking past each other. But Quine rejects the core of Carnap's philosophy, and it often seems that Quine just refuses to understand Carnap's point of view.<sup>1</sup> In this chapter I show that the dispute between Quine and Carnap about analyticity is rooted in their rival conceptions of the starting point and task of philosophy, and that Quine does not undermine Carnap's analytic-synthetic distinction on Carnap's own terms.

Quine's and Carnap's contrasting conceptions of philosophy shape their views of the task of logic. For Carnap the central task of logic is to codify intersubjective rules for evaluating assertions, whereas for Quine the task of logic is to provide explanatory generalizations that clarify the conceptual scheme of natural science. In Carnap's view, rules for intersubjective evaluations of our assertions are specified within the science of pure logic, and hence built into language itself. But in Quine's view, Carnap's proposals for specifying rules for evaluating our assertions have no explanatory role, and hence make no contribution to logical theory. Moreover, according to Quine, the indeterminacy thesis shows that there are no intersubjective semantical rules in Carnap's sense. So Quine concludes that Carnap's analytic-synthetic distinction is scientifically meaningless.

My reconstruction of Quine's objections is important to my argument for several reasons. First, Quine's criticisms are standardly taken to undermine Carnap's analytic-synthetic distinction on its own terms. This shows that philosophers are now almost blind to Carnap's conception of the starting point and task of philosophy. To reopen our eyes to Carnap's perspective on philosophy, I explain why it is not vulnerable to Quine's criticisms. Second, many philosophers think that we can accept Quine's central arguments against Carnap's analytic-synthetic distinction without embracing Quine's indeterminacy thesis.<sup>2</sup> I argue that this is a mistake. Once these standard misunderstandings of Quine and Carnap have been exposed, I go on in Chapter Six to reconstruct Putnam's reasons for rejecting Carnap's analytic-synthetic distinction. A thorough understanding of the contrasts between Carnap's, Quine's, and Putnam's views of the starting point and task of philosophy helps to loosen the hold of scientific naturalism and metaphysical realism.

I begin (§63) with a puzzling exchange between Carnap and Quine about what constitutes a language, and then sketch their contrasting conceptions of language, logic, and interpretation (§§64–65). This sketch sets a framework for my clarifications of Quine's central objections to Carnap's attempts to characterize the analytic-synthetic distinction in purely logical terms (§§66–70). I then argue that Quine's objections to Carnap's characterizations of the analytic-synthetic distinction are not complete unless they are supplemented with Quine's argument for the indeterminacy of translation (§§71–73). I conclude by contrasting Carnap's methodological conception of science with Quine's scientific naturalism (§74).

#### 63. A Puzzling Exchange between Carnap and Quine

As Carnap was preparing his reply to Quine's paper "Carnap and Logical Truth," Carnap asked Quine for a clarification of Quine's use of the term "language": "The question is, which of your discussions are meant to refer to (a) natural languages, and which to (b) codified languages (i.e., language-systems based on explicitly formulated rules) . . . The distinction is of great importance for my discussion, because from my point of view the problems of analyticity in the two cases are quite different."<sup>3</sup> Quine replied as follows:

You ask whether I mean "(a) natural languages" or "(b) codified languages . . . based on explicitly formulated rules." Now here I suppose you mean codified languages to carry explicit "semantical rules" with them—i.e., outright specification of the so-called analytic sentences. If so, then (b) is not what I am talking about . . . But I do not mean to limit myself to (a) either. It is indifferent to my purpose whether the notation be traditional or artificial, so long as the artificiality is not made to exceed the scope of "language" ordinarily so-called, and beg the analyticity question itself.<sup>4</sup>

Quine does not want Carnap to beg the analyticity question by stipulating that some languages have explicitly codified rules. Yet from Carnap's point of view, Quine begs the analyticity question by refusing to take for granted that some languages have explicitly codified rules. Each seems unable to avoid begging the question against the other.

#### 64. Carnap and Quine on Language and Logic

The dispute between Carnap and Quine about analyticity can't be separated from their fundamentally different conceptions of language, and their corresponding views of the starting point and task of logic.

For Carnap a language should be viewed primarily as "an instrument of communication."<sup>5</sup> His starting point is that if speakers are to agree or disagree they must share rules for evaluating their assertions. So a language is a "system of signs and of rules for their use,"<sup>6</sup> not a theory (or "a system of assertions about objects"). The rules of a language fix the intersubjective standards relative to which assertions of sentences of the language can be evaluated as true or false. It is the task of logic to analyze and facilitate communication about objective matters by specifying the intersubjective content, or truth conditions, of sentences.

In contrast, for Quine a language should be viewed primarily as a *theory*. Our starting point in applying logic is our use of sentences of our own idiolect or theory, not our agreement or disagreement with others about how to use our words. The explanatory task of logic is to simplify and clarify the logical structure and conceptual commitments of our own theory. In Quine's view, we continue the

work of natural science when we paraphrase our sentences into a canonical idiom that facilitates deductions and clarifies our conceptual commitments: "The same motives that impel scientists to seek ever simpler and clearer theories adequate to the subject matter of their special sciences are motives for simplification and clarification of the broader framework shared by all the sciences. . . . The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from a quest of ultimate categories, a limiting of the most general traits of reality."<sup>7</sup> Thus Quine's conception of the task of logic is a further expression of his scientific naturalism: "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described."<sup>8</sup>

Just as it is not the task of natural sciences like physics or biology to further communication, so for Quine is it not the task of logic to codify intersubjective standards for evaluating our assertions. Any clarification of the intersubjective "contents" of our sentences must be given in terms of speech dispositions that link sentences and sensory evidence, and logic is silent about these dispositions. In Quine's view, "communication" between two individuals must be understood in terms of the "meshing" of their speech dispositions.<sup>9</sup> As we will see in detail, Quine's indeterminacy thesis directly opposes Carnap's "motivating insight" that if investigators are to agree or disagree they must share rules for evaluating their assertions.

#### 65. Carnap and Quine on the Interpretation of Artificial Notations

Carnap's and Quine's rival conceptions of language and of the starting point and task of logic are reflected in their different views of how the artificial notations of symbolic logic are interpreted.

Carnap conceives of a language as a system of *shared* rules for using symbols. Thus he thinks that to interpret a language is to agree on how to use its expressions—to specify the rules that are to be shared by those who use the language. As we have seen, for Carnap the task of logic is to codify intersubjective rules for using symbols of artificial languages. To interpret symbols of an artificial notation is to agree with others about how they are to be used. In general, for Carnap, to interpret an expression is say how that expression contributes to the truth conditions of sentences in which it occurs.

To interpret the symbols of an artificial notation, we must agree on what their contribution to the truth conditions of sentences is to be. For example, Carnap writes: "Specification of truth-conditions for a connective consists in an *agreement* which fixes the conditions under which a compound sentence (formed by means of the connective and the sentences that enter as components) is to be considered true in terms of the truth or falsity of its components."<sup>10</sup> To come to an explicit agreement with another speaker about how we shall use our words, we must of course share a metalanguage within which to express our agreement. As we saw in Chapter Four, Carnap is tolerant and pragmatic about what counts as a suitable metalanguage for formulating our agreements about how we shall use our words.

For Quine the task of logic is not to codify intersubjective rules, but to simplify and clarify logical theory. *To interpret symbols of an artificial notation is just to use them to further one's own evolving theoretical purposes.* An individual interprets a sentence *S'* of an artificial notation when he uses it *in place of* an unformalized sentence *S* of his idiolect. The relationship of *S'* to *S* is just that "the particular business that the speaker was on that occasion trying to get on with, with help of *S* among other things, can be managed well enough to suit him by using *S'* instead of *S*."<sup>11</sup> Quine carefully avoids any essential reference to agreements with other speakers in his account of the interpretation of an artificial notation. He emphasizes that "the speaker is the one to judge whether the substitution of *S'* for *S* in the present context will further his present or evolving program of activity to his satisfaction."<sup>12</sup> An individual's use of artificial notations is just an extension of his everyday intrasubjective departures from ordinary idioms, departures that help him to clarify and simplify his evolving "theory." For Quine our use of the artificial notations of modern logic is motivated by our quest for simple and clear theories of nature, and not primarily by a desire to facilitate communication.

## 66. Quine on Truth by Convention Again

We are now in a position to see that Quine's objection to the thesis that logic is true by convention reflects Quine's fundamental rejection of Carnap's view of language, logic, and interpretation.

As we saw in §52, Quine asserts that the thesis that logic is true by convention has "explanatory force" only if logic is *made true* by the explicit adoption of conventions for the use of logical words. He observes that we must presuppose rules of inference in our metalanguage if we are to derive consequences from any explicitly adopted axioms for generating the logical truths. He concludes that logic is not made true by the explicit adoption of conventions for the use of logical words, and so the thesis that logic is true by convention is empty.

In §53 we saw that from Carnap's point of view this objection is irrelevant. According to Carnap, the central task of logic is to codify intersubjective rules for using the symbols of artificial languages. Since our specifications of the rules of an object language *L* always presuppose a shared metalanguage, we can't explain why the *L*-true sentences of *L* are true. But in Carnap's view we can nevertheless show that in any given context of inquiry in which investigators share a metalanguage, they can specify rules relative to which the *L*-true sentences of an object language *L* are "empty of empirical content." In this deflationary way Carnap solves the positivists' problem of showing that our knowledge of logical and mathematical truths is not dependent on evidence of any kind.

Why did Quine fail (or refuse) to see this?<sup>13</sup> The answer lies in Quine's view of the starting point and task of logic. We have seen that for Quine the central task of logic is to provide explanatory generalizations that clarify the conceptual scheme of natural science. Logical theories are to be evaluated in the same way as theories in other sciences like physics and biology.<sup>14</sup> But in the sciences we aim at truth; there is no scientific theory whose primary purpose is to facilitate communication. So when Quine tries to understand the thesis that logic is true by convention, he views it as an explanatory hypothesis, which we must evaluate on scientific grounds. From Quine's point of view, Carnap's proposals for clarifying logical truth amount to the recommendation that we derive the logical truths from explicitly adopted axioms and inference rules. But Carnap's proposed axioms and inference rules merely reorganize our knowledge of the logical truths; they do not yield new truths or explain old ones. So by Quine's standards, Carnap's linguistic doctrine of logical truth makes no contribution to logical theory. To understand this fundamental rejection of Carnap's view of logical

truth, we must carefully reconstruct Quine's critique of Carnap's proposals for explicating the analytic-synthetic distinction.

### 67. Preliminaries for Understanding Quine's Critique of Analyticity

Quine's objections to analyticity are of a piece with his objections to the thesis that logic is true by convention.<sup>15</sup> In this section I sketch a framework for my reconstructions (in §§68-70) of Quine's objections to analyticity. My aim is to show that Quine's objections to analyticity are rooted in his view that the task of logic is to facilitate deduction and clarify our conceptual commitments.

Let us begin by considering Quine's distinction between logical truth and analyticity. An example of what Quine calls a "logical truth" is: "No unmarried man is married." Quine characterizes logical truth as follows: "If we suppose a prior inventory of *logical* particles, comprising 'no', 'un-', 'not', 'if', 'then', 'and', etc., then in general a logical truth is a statement which is true and remains true under all reinterpretations of its components other than the logical particles."<sup>16</sup> The "second class" of "analytic truths" is exemplified by: "No bachelor is unmarried." This class includes any statement that "can be turned into a logical truth by putting synonyms for synonyms."<sup>17</sup> Quine draws an important distinction between his characterizations of logical truth and analyticity: "We still lack a proper characterization of this second class of analytic statements, and therewith of analyticity generally, inasmuch as we have had in the above description to lean on a notion of 'synonymy' which is no less in need of clarification than analyticity itself."<sup>18</sup> At this point in "Two Dogmas of Empiricism" Quine leaves the notion of logical truth behind, and begins his critique of analyticity.

But to see the roots of Quine's critique of analyticity, we must first see why he thinks logical truth is a *clear* and *explanatory* notion, whose proper characterization eschews all links to Carnap's project of codifying intersubjective rules for evaluating our assertions.

To define logical truth for sentences we can use, we need only paraphrase sentences of our ordinary language into a suitable artificial notation, specify the "logical particles," and define in syntactical terms what counts as an admissible reinterpretation of the nonlogical components of sentences in the notation.<sup>19</sup> We can then

define a logical truth as a true sentence (of the notation) that remains true under all admissible reinterpretations of its nonlogical components. This definition is as clear as the notion of truth. When truth is defined disquotationally, any application of truth to a sentence is as clear as the sentence itself. So whether or not we care about codifying intersubjective rules for evaluating our assertions, we can each use Quine's method intrasubjectively to define a notion of logical truth that is as clear to us as the sentences of our own idiolects.

For Quine the "explanatory value" of a proposed characterization of a logical notion should be judged by its contribution to logical theory, whose task is to facilitate deduction and clarify our conceptual commitments. Quine's characterization of logical truth simplifies the theory and practice of deduction, and it clarifies our total theory of nature by enabling us to frame general truths that hold for all subjects. Thus whether or not we care about codifying intersubjective rules for evaluating our assertions, we are each in a position intrasubjectively to see that a definition of logical truth makes an important contribution to our own evolving theories of nature.

The burden of §§2-4 of "Two Dogmas of Empiricism" is to show that in contrast with logical truth, the notion of analyticity cannot be clarified or explained in purely logical terms. This is a direct challenge to Carnap's view that the notion of analyticity is available within pure logic,<sup>20</sup> independent of any special empirical investigations. Quine examines two strategies for clarifying or explaining analyticity in purely logical terms for sentences of languages we already use. The first strategy, discussed in §§2-3, is to give a purely logical clarification of the notion of *cognitive synonymy*. If we had such a clarification, we could clarify the "second class" of analytic truths as those sentences that become logical truths on substitution of synonyms for synonyms. The second strategy, discussed in §4, is to use the notion of a semantical rule to explicate analyticity. Carnap uses the second strategy, as Quine well knows. Quine's discussion of the first strategy is designed to prepare us for his rejection of the second strategy, and therewith of Carnap's explications of analyticity in terms of semantical rules.

In the next three sections I reconstruct Quine's critique of both of

these strategies for characterizing analyticity in purely logical terms. I start (in §68) with Quine's criticisms of the first strategy, next turn (in §69) to Quine's rejection of the second strategy, focusing on Carnap's use of the notion of a semantical rule. I then continue my discussion of the second strategy (in §70) by examining Quine's objection to Carnap's idea that we can use "postulates" to determine primitive truths of a language system.

### 68. Definition and Interchangeability

Pursuing the first strategy, Quine considers attempts to clarify cognitive synonymy by the use of formal *definitions* (§2), and in terms of *interchangeability salve veritate* (§3). He argues that both attempts fail to clarify or explain cognitive synonymy in purely logical terms. Quine's arguments in §§2-3 leave open the possibility that we can clarify cognitive synonymy in psychological terms, perhaps using Quine's favored concept of a speech disposition. But even if we could clarify cognitive synonymy in terms of speech dispositions, this would not show that the notion of analyticity is part of logical theory, and so it would not vindicate Carnap's assumption that he can clarify the notion of analyticity without presupposing any concepts of an empirical science. With this in mind, let us now turn to Quine's arguments.

Consider the naive proposal that the cognitive synonymy of 'bachelor' and 'unmarried man' is based in a *definition*. Quine points out that although our dictionaries define 'bachelor' as 'unmarried man' the definition is actually a report on usage, and so it is not available independent of empirical observations: "The lexicographer is an empirical scientist, whose business is the recording of antecedent facts; and if he glosses 'bachelor' as 'unmarried man' it is because of his belief that there is a relation of synonymy between those forms, implicit in general or preferred usage prior to his own work."<sup>21</sup> Thus the definitions that appear in dictionaries do not show that the notion of cognitive synonymy can be explained solely in logical terms, without appeal to any special science.

A more sophisticated proposal is that within pure logic we can give *explications*—precise definitions of terms whose prior use was vague—and thereby avoid the charge that our definitions are mere reports of preexisting usage. But this proposal fares no better:

even explication, though not merely reporting a preexisting synonymy between definiendum and definiens, does rest nevertheless on *other* preexisting synonymies. The matter may be viewed as follows. Any word worth explicating has some contexts which, as wholes, are clear and precise enough to be useful; and the purpose of explication is to preserve the usage of these favored contexts while sharpening the usage of other contexts. In order that a given definition be suitable for purposes of explication, therefore, what is required is not that the definiendum in its antecedent usage be synonymous with the definiens, but just that each of these favored contexts of the definiendum, taken as a whole in its antecedent usage, be synonymous with the corresponding context of the definiens.<sup>22</sup>

Against this one might reply that all we require of an explication is that it further our present theoretical purposes, not that it preserve a preexisting synonymy relation. The trouble with this reply is that the only reason to adopt an explication is that it captures what is cognitively important to us about a preexisting usage.

Out of desperation one might insist that no understanding of what is cognitively important to us about a preexisting usage is needed for explication. But then one's purposes should be just as well served by mere definitional abbreviations of terms already in use. This is the third proposal Quine considers, and he finds it clear enough: "Here the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens. Here we have a really transparent case of synonymy created by definition; would that all species of synonymy were as intelligible."<sup>23</sup> Quine's point is that this is the only kind of definition that does not presuppose that we already understand cognitive synonymy. With this cryptic observation, he concludes that we cannot clarify or explain cognitive synonymy by giving definitions.

To test Quine's conclusion, suppose someone stipulates that by "cognitive synonymy" he just means the kinds of synonymies based in trivial definitional abbreviations for terms already in use. Can he then make sense of analyticity, as logical truth plus definitional abbreviations? In "Two Dogmas of Empiricism" Quine does not consider this question, but it is important to see that in his view

the answer is "No." Even an *attenuated* notion of analyticity resists clarification in terms of logical truth plus definitional abbreviations. The reason is that a mere stipulation of a definitional abbreviation by itself does nothing to clarify the difference between a revision in our beliefs and a change in our language. To explicate this difference, as we have seen, Carnap relies on the notion of an explicit semantical rule. In Quine's view the acceptance of definitional abbreviations does not amount to the specification of semantical rules for the expressions thus defined, and so it cannot by itself bridge the gap between Quine's schematic characterization of logical truth and the positivists' idea of analyticity.<sup>24</sup>

In §3 of "Two Dogmas of Empiricism" Quine examines the proposal that two expressions of a language L are cognitively synonymous if in all their logically significant occurrences in sentences of L they are interchangeable *salve veritate*. Quine observes that to make this proposal precise one has to specify the language for which it is given. If the language contains a sentential operator that has the same meaning as the English word 'necessarily', then interchangeability *salve veritate* is sufficient for cognitive synonymy. The problem is that the word 'necessarily' is as much in need of clarification in logical terms as 'analytic'. So we make no real progress by assuming that we understand necessity and by defining cognitive synonymy in terms of interchangeability in all sentences *salve veritate*. If on the other hand we begin with an extensional language, then interchangeability *salve veritate* is not sufficient for cognitive synonymy. A proper characterization of cognitive synonymy should enable us to distinguish between synthetic truths and truths based in meaning alone, but so far we have seen no way to draw this distinction without presupposing that we understand necessity or cognitive synonymy itself.

In Quine's view, if we are to accept a notion as part of our logical theory, we must be able to show that it facilitates the theory and practice of deduction or clarifies our conceptual commitments. We saw earlier that Quine's characterization of logical truth passes this test, and we have recently seen why neither definition nor the criterion of interchangeability *salve veritate* can similarly vindicate analyticity. Worse than that, Quine's discussion of definition shows that analyticity is closely linked to our ordinary notion of cognitive synonymy, whose proper clarification awaits empirical investiga-

tions of preexisting usage. So by the end of Quine's discussions of the first two proposals we are still without any proper characterization of analyticity, and we have reason to suspect that there are serious obstacles to finding one.

### 69. Semantical Rules

In §4 of "Two Dogmas of Empiricism" Quine finally turns to an explicit discussion of Carnap's proposal that we clarify analyticity in terms of semantical rules. Quine's aim is to show that Carnap's talk of semantical rules and language systems makes no meaningful contribution to logical theory—that such talk is explanatorily empty. What drives Quine's criticisms is his view that the task of logical theory is to facilitate deduction and clarify our conceptual scheme.

Quine's criticisms of analyticity are a natural extension of his rejection of Carnap's view that logical truths are "true in virtue of rules." We saw (§65) that for Quine there is no explanatory value to reorganizing our body of logical truths by showing that they can be derived from a set of explicitly adopted axioms. Quine sees no explanatory value to Carnap's assumption that for each precisely characterized language there is a unique set of rules for using its expressions, and so Quine sees no point in saying that logical truths are "true in virtue of rules." His central criticism of the notion of analyticity is that there is no way to specify the analytic truths of a language without using the "scientifically meaningless" notion of a semantical rule. Quine concludes that unlike logical truth, analyticity makes no explanatory contribution to logical theory.

The essential dependence of Carnap's characterization of analyticity on his notion of a semantical rule is illustrated by Carnap's explication of analyticity in terms of state descriptions. We saw (§§55–56) that for Carnap a sentence is *L-true* if it is true in all state descriptions, where a *state description* for the language  $L_1$  is a class that contains, for every atomic sentence (consisting of a predicate with a name in each of its argument places) of  $L_1$ , either that sentence or its negation, but not both. When state descriptions of a language L are characterized in this unrestricted syntactic way, Carnap's notion of *L-truth* is extensionally equivalent to what Quine calls logical truth.<sup>25</sup> Yet as we have seen, Carnap explicates analy-

specify the set *K* are distinguishable only by the fact of appearing on a page under the heading rules that specify the set *K*; this heading is itself then just an indication that we have made an arbitrary selection of true sentences." Carnap's appeal to semantical rules does not show that sentences labeled "analytic" have any explanatory value. From an explanatory point of view, the heading 'Semantical Rules' is arbitrary, hence scientifically meaningless.

Carnap agrees that within pure semantics the question "What are the semantical rules of *L*?" makes no sense for languages whose semantical rules are not precisely specified. For Carnap, within pure logic the analytic-synthetic distinction can only be drawn for a language system with precisely formulated rules.<sup>27</sup> So from Carnap's point of view, Quine's objections to the application of the notion of semantical rules to languages whose semantical rules are not precisely specified are irrelevant to the question of whether the analytic-synthetic distinction can be precisely drawn within pure logic.

Quine was well aware that Carnap would make this reply. The central point of §4 is to try to show that Carnap's idea of a "language-system" is of no use in clarifying analyticity. In the third paragraph of §4 Quine writes: "It is often hinted that the difficulty in separating analytic statements from synthetic ones in ordinary language is due to the vagueness of ordinary language and that the distinction is clear when we have a precise artificial language with explicit 'semantical rules.' This, however, as I shall now attempt to show, is a confusion."<sup>28</sup> By the end of Quine's discussion of artificial languages without precisely formulated semantical rules, Quine takes himself to have shown that the very idea of a "semantical rule" is scientifically meaningless, even for what Carnap calls "language-systems." Quine argues that Carnap's stipulations that some rules are to count as the "semantical rules" of a language system *L* does no more than specify a particular set of true sentences of *L*. Mere stipulation that some subset of the true sentences of a language *L* are called "analytic" does not show that there is any explanatory value in distinguishing between the "analytic" sentences and other true sentences of *L*. So the claim that semantical rules are built into Carnap's idea of a language system does not tell us why the notion of analyticity is of any scientific interest.<sup>29</sup> Quine concludes that Carnap's specifications of language systems add no content to the idea of semantical rules, or of analyticity.

ticity in terms of *L-truth* by placing further restrictions on what counts as a state description. Carnap calls these restrictions "meaning postulates." From Quine's point of view, the trouble is that to make sense of "meaning postulates" we must assume with Carnap that for each precisely characterized language there is a unique set of semantical rules for using its expressions.

Quine begins §4 by assuming that we are each able to use sentences in an artificial notation, and he asks how we could clarify or explain Carnap's idea that some of these sentences are analytic. We could arbitrarily stipulate that a certain class of sentences of *L* are to be called "analytic." But this leaves us in the dark about the explanatory value, if any, of such a stipulation—we might as well just say that this is the class of sentences of kind *K*.

We know that "analytic" sentences are supposed to be true, so we can add this information to our understanding of *K*. Suppose then that we give a recursive specification of a set of true sentences of our language, and call them "K-truths." What is the explanatory value for logical theory of defining a set of K-truths? Once we have a Tarski-style truth definition for a language *L*, we can define arbitrary subsets of the true sentences of *L*. For example, we can define the set of true sentences of *L* that begin with the letter 'K'. This set of sentences obviously has no explanatory value for logical theory. Quine's question is how the set of "analytic" sentences is to be distinguished in point of explanatory value from the countless arbitrary sets of true sentences we could define.

According to Carnap, the difference between the "analytic" sentences of a language *L* and arbitrary sets of true sentences of *L* is that the former but not the latter are true "in virtue of the semantical rules of *L*." Thus the whole weight of his explication of analyticity rests on the notion of a semantical rule. From Quine's point of view, until we are given some clear explanation of how talk of "semantical rules" contributes to logical theory, we simply do not understand the difference between a set of sentences that are true "in virtue of semantical rules," on the one hand, and any arbitrary set of true sentences, such as those true sentences whose first letter is 'K', on the other. As Quine puts it: "Semantical rules are distinguishable, apparently, only by the fact of appearing on a page under the heading 'Semantical Rules'; and this heading is itself then meaningless."<sup>26</sup> We could paraphrase this as follows: "rules that

The contrast between Carnap's and Quine's views of the task of logic emerges clearly when Quine concedes that a choice of postulates can be of methodological significance, but insists that this does not show that a choice of postulates or semantical rules has any significance for logical theory:

The word 'postulate' is significant only relative to an act of inquiry; we apply the word to a set of statements just in so far as we happen, for the year or the moment, to be thinking of those statements in relation to the statements which can be reached from them by some set of transformations to which we have seen fit to direct our attention. Now the notion of semantical rule is as sensible and meaningful as that of postulate, if conceived in a similarly relative spirit—relative, this time, to one or another particular enterprise of schooling unconvertsant persons in sufficient conditions for truth of statements of some natural or artificial language L. But from this point of view no one signalization of a subclass of the truths of L is intrinsically more a semantical rule than another; and, if 'analytic' means 'true by semantical rules', no one truth of L is analytic to the exclusion of another.<sup>34</sup>

Carnap would agree that independent of our interest in a precise codification of rules for evaluating our statements "no one signalization of a subclass of the truths of L is intrinsically more a semantical rule than another." But in Carnap's view, the task of logic is the codification of the methodology of the sciences. Relative to this task, our specification of semantical rules is as clear as our use of postulates in a given act of inquiry. Thus Carnap should reply that the selection of a particular set of semantical rules makes sense relative to his project of codifying the methodology of the sciences.

In the paragraph just quoted Quine concedes that the notion of a postulate makes sense relative to an act of inquiry. So why does Quine refuse to accept Carnap's use of semantical rules to codify the methodology of science?<sup>35</sup>

Part of the answer is that for Quine the task of logic is not to codify the methodology of science. Logic is itself a natural science, whose business is truth, not method. From Quine's point of view, Carnap has confused epistemology with logic. What Carnap takes to be part of logic makes no sense when viewed as an explanatory

Early in §4 Quine characterizes his skeptical challenge to Carnap's notion of analyticity as follows: "The notion of analyticity about which we are worrying is a purported relation between statements and languages: a statement S is said to be *analytic* for a language L, and the problem is to make sense of this relation generally, that is, for variable 'S' and 'L'."<sup>30</sup> Carnap replies that it is impossible to give a precise explication of analyticity for variable 'S' and 'L', but that in this respect analyticity is no worse off than other concepts used in logic.<sup>31</sup> As we have seen, Quine argues that Carnap's explications do not show that the concept of analyticity has any scientific interest. To explain the interest of the notion of analyticity, we would have to say what different definitions of 'S is analytic in L' have in common, for variable S and L. For Quine this is not a general condition on all definitions used in logic, but a consequence of our failure to understand the notion of a "semantical rule" on which Carnap's explications of analyticity depend.<sup>32</sup> Since according to Quine we do not understand the notion of a semantical rule, all we can glean from Carnap's explications is that a sentence S is analytic in L if and only if S is a member of some subset of true sentences of L, and this is not enough to show that the notion of analyticity has any explanatory value.

#### 70. Postulates, Semantical Rules, and the Task of Logic

We have now seen that given Quine's view of the task of logic, Carnap's explications of analyticity are arbitrary and hence without logical interest. But do Quine's criticisms rest on an arbitrary definition of 'logic'? This question becomes especially pressing when Quine compares postulates with semantical rules.

Relative to a given set of postulates, it is easy to say what a postulate is: it is a member of the set. Relative to a set of semantical rules, it is equally easy to say what a semantical rule is. But given simply a notation, mathematical or otherwise, and indeed as thoroughly understood a notation as you please in point of the translations or truth conditions for its statements, who can say which of its true statements rank as postulates? Obviously the question is meaningless—as meaningless as asking which points in Ohio are starting points.<sup>33</sup>



theory, and hence it is meaningless from a scientific point of view.

But this apparently leaves us at an impasse. Given Carnap's view that the task of logic is to codify rules for rational inquiry, he can agree with Quine that analyticity has no "explanatory value" for logical theory<sup>36</sup> without giving up the claim that analyticity is important to "logic," if that term encompasses the methodology of science. Quine must go beyond his preliminary arguments in "Two Dogmas of Empiricism" if he is to show that Carnap's idea of a codification of rules for inquiry makes no scientific sense.<sup>37</sup>

### 71. Indeterminacy and Carnap's Analytic-Synthetic Distinction

We saw (§64) that Quine's conception of the starting point and task of logic is of a piece with his scientific naturalism: "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described."<sup>38</sup> Quine's arguments in §§1-4 of "Two Dogmas of Empiricism" show the consequences of Quine's scientific naturalism about logic. The reason that Carnap's idea of a codification of rules for inquiry makes no sense, according to Quine, is that from a naturalistic point of view Carnap's methodological perspective on science is unscientific.

Carnap maintains that the use of logical and mathematical techniques to construct precise languages for use in scientific work is itself part of science: it is the "logic of science," a part of logic, which is among the rigorous and clear sciences Carnap endorses. But Quine does not accept Carnap's logic of science as part of science. Quine sees Carnap's methodological conception of science as an illegitimate attempt to view science from a perspective higher and firmer than science.

In Quine's view, Carnap's unscientific methodological perspective on science must be replaced with naturalized epistemology. As we saw in Chapter Two, the motivating insight of naturalized epistemology is that "we can know external things only through impacts at our nerve endings."<sup>39</sup> Quine recasts epistemology by sketching a scientific picture of "evidence" and "theory" and the relationship between them. "Evidence" is described in terms of "impacts at our nerve endings," and "theory" in terms of the linguistic output that subjects deliver when prompted by triggerings

of their neural receptors. Viewed naturalistically, a scientific theory is a fabric of sentences variously associated with one another and linked to impacts at our nerve endings by the mechanism of conditioned response.<sup>40</sup>

These links between sentences and impacts at our nerve endings exhaust the natural facts relevant to "cognitive meaning," and this leads to Quine's indeterminacy thesis, according to which the totality of speech dispositions does not uniquely determine how idiolects are to be "translated." To take another investigator's words at face value is (in effect) to "choose" a homophonic manual of translation for his words, but there are many radically different manuals that preserve all his speech dispositions. Translation manuals are not like scientific theories: in addition to being underdetermined by all evidence, they are also indeterminate.

From Quine's naturalistic point of view, this shows that Carnap's methodological perspective on science is unscientific. Within pure semantics, according to Carnap, investigators use the same words when they specify the "semantic rules" of a language. But the indeterminacy thesis shows that it is an illusion—mere "make-believe"<sup>41</sup>—to think that the "semantical rules" articulated from the perspective of Carnap's pure semantics are scientifically legitimate. By taking for granted the homophonic translation manuals for their "shared" metalanguages, the perspective of pure semantics creates the illusion of scientific objectivity. But since the assumption of a homophonic translation manual is arbitrary from a scientific point of view, the perspective of pure semantics is illegitimate. Carnap's explications of the analytic-synthetic distinction can be understood only from the perspective of pure semantics, where investigators must use the same words when specifying "semantic rules" for using expressions of an object language. Like the perspective on which its explication depends, Carnap's analytic-synthetic distinction is arbitrary and unscientific.

### 72. Does Carnap Need a Criterion of Analyticity?

Quine's arguments in "Two Dogmas of Empiricism," together with naturalized epistemology and the indeterminacy thesis, show that from within Quine's naturalistic framework Carnap's analytic-synthetic distinction makes no sense. But Quine never succeeded in

convincing Carnap that he should accept Quine's naturalistic framework. For Carnap pure semantics is part of the science of logic, and since he understands science from within pure semantics, he cannot make sense of Quine's naturalistic point of view. Thus on the reading I have presented, Quine does not show on Carnap's terms that the analytic-synthetic distinction makes no sense.

According to Thomas Ricketts, however, Quine's objections to Carnap's analytic-synthetic distinction undermine Carnap's position from within.<sup>42</sup> Ricketts reconstructs Quine's reasoning as follows:

Quine's challenge to analyticity . . . begins with a concern about the applicability of th[is] notion to the theorizing of investigators, be they real scientists or idealized types thereof. *In order to judge the rationality of the theorizing of an investigator, we must be in a position to ascertain independently of such judgments what frameworks, what rules, the investigator has adopted. Unless we epistemic judges can apply this distinction, an investigator will always be able to escape criticism altogether by pleading he has been misunderstood.* We need then a criterion for attributing a linguistic framework to an investigator, what I shall call a *criterion of analyticity*. Without such a criterion, we have no bench from which to deliver our epistemic judgments, and so find ourselves caught in a position of epistemic solipsism destructive of the very ideal of rationality Carnap wants to articulate and vindicate.<sup>43</sup>

According to Ricketts, Quine's indeterminacy thesis shows that there can be no criterion of analyticity, and so Carnap's analytic-synthetic distinction cannot be drawn; "the mere acknowledgment" that Carnap must provide a criterion of analyticity "vitiates Carnap's conception of rational reconstruction."<sup>44</sup>

There are two ways of reconstructing Ricketts's reasons for asserting that Carnap must have a criterion of analyticity. On the most natural reconstruction, Ricketts reasons that investigators cannot evaluate each other's assertions until they first justify their assumption that they share a linguistic framework. This requires that they agree on a criterion by which they can find out whether they share a linguistic framework, and if so, which one.<sup>45</sup> I shall call

this the *epistemological requirement*. Ricketts also seems to think that Carnap needs to show that an investigator's speech dispositions, austere conceived, uniquely determine what semantical rules he is following, whether or not investigators are required to cite uninterpreted speech dispositions as "evidence" for their presupposition that they understand each other.<sup>46</sup> I shall call this the *metaphysical requirement*.

Ricketts does not clearly distinguish between these two requirements, but I will discuss them separately. As I see it, Carnap does not and should not accept either one.

Despite its air of plausibility, the epistemological requirement automatically leads to an infinite regress, without any help from Quine's arguments for indeterminacy. To see why, suppose A and B take for granted that they share a language, and propose a criterion of analyticity using this language. In order to justify their assumption that this "criterion" provides them with a "common bench before which to litigate their differences," they must find some independent justification for their initial assumption that they share the language they used to state the "criterion" itself. Thus if Ricketts's first challenge were legitimate, A and B would have to meet a second challenge. They would need to find a second criterion of analyticity for justifying their assumption that they understand the first criterion of analyticity in the same way. Suppose they seem to agree on a second criterion for understanding the first criterion. How can they be sure that they understand the second criterion in the same way? Presumably they must agree on a third criterion for understanding the second criterion for understanding the first criterion in the same way. And so on. We cannot avoid the regress by claiming that there is a stage at which A and B need not justify their assumption that they share a criterion of analyticity, for this would lead both A and B to "a position of epistemic solipsism destructive of the very ideal of rationality Carnap wants to articulate and vindicate."<sup>47</sup>

Carnap does not and should not accept any requirement that leads automatically to an infinite regress. As we saw at length in Chapter Four, Carnap has a pragmatic attitude toward the languages investigators use to codify rules for rational inquiry. In practice there is no need for an epistemological criterion of analyticity; it is enough that investigators use a common language whose words they find clear.

Ricketts might reply that Carnap needs to show that an investigator's speech dispositions uniquely determine what semantical rules he is following. This would not automatically lead to an infinite regress, since A's and B's speech dispositions might uniquely determine that they share a linguistic framework whether or not they could *justify* this to each other. If Carnap accepts this requirement, then Quine's indeterminacy thesis would directly challenge Carnap's conception of pure semantics and the analytic-synthetic distinction.

But Carnap does not accept Ricketts's metaphysical requirement either. As we saw in §§57–60, Carnap has a coherent defense of the claim that pure semantics makes no presuppositions about how linguistic behaviors are related to semantical rules. He says that pure semantics is related to descriptive semantics just as pure geometry is related to applied geometry. This means that an investigator's speech dispositions do not by themselves uniquely determine what semantical rules he is following. To describe the relationship between speech dispositions and semantical rules, we must first choose an appropriate set of coordinative definitions. For Carnap pure semantics is a science in its own right, not in need of any independent scientific or metaphysical support.

I conclude that Carnap does not need a criterion of analyticity to vindicate pure semantics and the analytic-synthetic distinction. Ricketts's epistemological and metaphysical "requirements" both rest on a misunderstanding of Carnap's project, and so Ricketts fails to show that Quine's arguments undermine Carnap's view of rational inquiry from within.

### 73. Holism and the Analytic-Synthetic Distinction

So far I have focused on §§1–4 of "Two Dogmas of Empiricism." But Christopher Hookway thinks Quine's central argument against Carnap's analytic-synthetic distinction occurs in §5 of "Two Dogmas of Empiricism," where Quine observes that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body."<sup>48</sup> In Hookway's view, this observation contains the seeds of a powerful objection to Carnap's analytic-synthetic distinction.

Hookway claims that Carnap introduced the analytic-synthetic

distinction "to explain why scientists agree about the bearing of observations and experiments upon their hypotheses."<sup>49</sup> The basic idea is that "shared rules determine which experiences refute, and which confirm, the claims made by scientists."<sup>50</sup> Statements whose truth values are determined solely by the rules are analytic or contradictory, whereas statements whose truth values also depend on evidence are synthetic.

The main difficulty for Carnap's analytic-synthetic distinction, according to Hookway, is that a scientific hypothesis is never tested in isolation. In 1906 Pierre Duhem noted that if a given hypothesis, in conjunction with our total theory of nature, entails an observation sentence that is apparently in conflict with our experience, this does not tell us which theoretical or observational beliefs to revise.<sup>51</sup> Carnap believed he could accommodate Duhem's thesis by distinguishing

between two kinds of readjustment in the case of a conflict with experience, namely, between a change in the language, and mere change in, or addition of, a truth-value ascribed to an indeterminate statement, (i.e. a statement whose truth-value is not fixed by the rules of language, say by the postulates of logic, mathematics, and physics). A change of the first kind constitutes a radical alteration, sometimes a revolution, and it occurs only at historically decisive points in the development of science. On the other hand, changes of the second kind occur every minute.<sup>52</sup>

There are no rules for making changes of the first kind, since these changes involve conventional choices of rules for a new language, and only pragmatic considerations can be relevant to such choices. In contrast, changes in the truth value ascribed to an indeterminate or synthetic sentence are governed by the semantical rules of the framework. So, in Carnap's view, Duhem's thesis amounts to the observation that in case of a conflict with experience, we are free to change our linguistic framework, or to change our ascription of truth values to some of our synthetic statements. Properly understood, Duhem's thesis does not undermine the analytic-synthetic distinction.

Against this, Hookway argues that if we accept Duhem's thesis, Carnap's analytic-synthetic distinction "loses its explanatory

force."<sup>53</sup> Hookway assumes that if Carnap's analytic-synthetic distinction is to have explanatory force, we must be able to distinguish between two kinds of "mechanisms" of belief revision, corresponding to Carnap's distinction between pragmatic changes in language systems and the reevaluation of truth values from within a language system: "Experience can prompt changes in our body of beliefs: systematic considerations of simplicity and fruitfulness have a role in deciding how to make these adjustments. There simply seems no basis for discerning two different mechanisms. The puzzle is less why Quine made this move than why Carnap didn't."<sup>54</sup> Hookway concludes that we must give up Carnap's analytic-synthetic distinction, since "there is nothing for the distinction of two different mechanisms to explain."<sup>55</sup>

There are several problems with this reconstruction of Quine's reasons for rejecting Carnap's analytic-synthetic distinction. The first and most serious problem is that Carnap did not introduce the analytic-synthetic distinction to provide a psychological or mechanistic explanation of "why scientists agree about the bearing of observations and experiments upon their hypotheses." As we have seen at length, the analytic-synthetic distinction is part of pure semantics, and hence independent of any empirical assumptions. Once this distinction is drawn, we can introduce coordinative definitions relative to which we can describe an investigator's sentences as analytic or synthetic. But Carnap does not assume that such descriptions posit the existence of rule-following "mechanisms" that determine an investigator's linguistic behavior. It is better to think of descriptive semantics as licensing redescription of a speaker's linguistic behavior in terms of semantical rules. For Carnap there is no further question of what "really" underlies a speaker's use of one language system rather than another. We understand all there is to this idea when we adopt coordinative definitions that give empirical content to the claims of descriptive semantics.

The second problem is that Carnap does not think that we must define the analytic-synthetic distinction directly in terms of sensory experience. For Carnap there can be no analytic-synthetic distinction at all unless we first specify, from within pure semantics, the rules of the language system we intend to use. These rules determine which sentences of the language system are synthetic, and

how these sentences are to be evaluated in light of empirical evidence. In Carnap's view, Duhem's thesis amounts to the observation that it is only in conjunction with our substantive beliefs that the rules of the language system we are using tell us what the empirical consequences of a given statement are. Carnap uses the analytic-synthetic distinction to *clarify* Duhem's thesis, and this shows that for Carnap the analytic-synthetic distinction is more fundamental than the question of whether scientific statements can be tested in isolation.

This second problem is linked with a third: Hookway misunderstands the role of Quine's argument in §5 of "Two Dogmas of Empiricism." Quine understood Carnap's views well enough to know that the points in §5 would not directly engage Carnap. As I see it, Quine's aim in §5 was to show that if we do not *already* have a clarification of the analytic-synthetic distinction from within pure logic or semantics, we cannot define the distinction in experiential terms. Carnap would surely agree with this. Yet from Carnap's point of view, Quine's argument in §5 is beside the main point, which is whether the notion of a semantical rule is a purely logical one. For Carnap the central arguments in "Two Dogmas of Empiricism" are in §4, where Quine attempts to show that the notion of a semantical rule is scientifically meaningless. Taken by itself, Quine's observation in §5 that there is a holism of theory testing does not challenge Carnap's analytic-synthetic distinction.<sup>56</sup>

#### 74. Carnap's Method versus Quine's Doctrine

In §§62-70 I explained why none of Quine's arguments in §§1-4 of "Two Dogmas of Empiricism" shows that we cannot use the notion of a semantical rule to codify rules for inquiry. To establish this strong conclusion, given his own starting point, Quine needs to show that the very idea of a codification of rules for inquiry is scientifically meaningless. For this he needs his indeterminacy thesis. But Quine's indeterminacy thesis does not undermine Carnap's position from within; instead it shows that Quine's scientific naturalism is fundamentally at odds with Carnap's conception of the starting point and task of philosophy. Whereas Carnap takes for granted that in the context of constructing language systems investigators use shared metalanguages to specify intersub-

jectively binding semantical rules, for Quine the perspective of investigators who take themselves to "share" language lies outside of natural science, and so the notion of an "intersubjectively binding semantical rule" that Carnap defines from this perspective is unscientific and illusory.

This clash of alternative points of view is reflected in Carnap's and Quine's contrasting views of the relationship between semantical descriptions of a speaker's utterances, on the one hand, and her speech dispositions, on the other. For Carnap, as we have seen (§§57-59), the relationship is mediated by our choice of coordinative definitions that relate concepts of pure semantics to linguistic behaviors. Carnap can agree with Quine that nothing determines which coordinative definitions we must use for descriptive semantics. But Carnap thinks we can use the methods of pure semantics to specify coordinative definitions that give empirical content to the claims of descriptive semantics. Carnap's conception of descriptive semantics is untouched by Quine's reflections about radical translation.

Quine's scientific naturalism fosters a fundamentally different view of what is objective about meaning. For Quine the cognitive content of a speaker's idiolect is exhausted by her dispositions to assent to and dissent from sentences under various prompting stimulations. The only objective constraint on a "translation" of one speaker's idiolect, or "theory," into another's, or even into itself, is that it "preserve the totality of speech dispositions."<sup>57</sup> According to Quine's indeterminacy thesis, there are many translation manuals that "preserve" the total pattern of associations between sentences and sensory stimulation, and hence capture the "empirical content" of sentences equally well. When two speakers of the same natural language use a homophonic manual of translation between their idiolects, they are in effect "choosing" one manual over countless others that would also "preserve" the total pattern of associations between their sentences and sensory stimulations.

In Quine's view the only transparent semantical notions are disquotational. We are each in a position to use disquotational paradigms to ascribe truth or reference to the sentences and predicates of our own idiolect, if we "choose" the homophonic mapping from our idiolect onto itself. It is only for our own idiolects that we can take the homophonic manual for granted without danger of empir-

ical error, and use disquotational truth and reference. All ascriptions of truth or reference to another speaker's words rest on "hypotheses" about how to "translate" her idiolect, even if we both speak the "same" ordinary language, such as English. For Quine each individual has her own language, or idiolect, and what we ordinarily call "language," such as English or French, is a more or less loose association of idiolects linked together by entrenched but scientifically arbitrary assumptions about how to "translate" between them.

In contrast, the "ultimate parameter" for Carnap's methodological conception of science is set by our use of a common metalanguage to specify syntactical and semantical rules for the proper use of linguistic expressions. Since for Carnap our only understanding of scientific content and method is given by our specifications of semantical rules for using expressions of a shared language, empirical discoveries cannot challenge the scientific utility and clarity of the notion of a semantical rule.

These fundamentally opposed views of language and semantics reflect Carnap's and Quine's different conceptions of the starting point and task of philosophy. Carnap's perspective is pragmatic and methodological: he aims to clarify and thereby help to resolve the agreements and disagreements of practicing scientists. Quine's perspective is naturalistic and doctrinal: all there is to science is explanation and prediction. Since for Quine the notion of a semantical rule has no explanatory or predictive value, Quine concludes that Carnap's analytic-synthetic distinction is scientifically meaningless.

Ultimately, Quine's "argument" against Carnap's analytic-synthetic distinction is that we must reject Carnap's conception of the starting point and task of philosophy, and replace it with scientific naturalism. Quine's impressive rhetoric, combined with what seems to be a blindness to Carnap's point of view, gives his "argument" the flavor of a demonstration that Carnap is wrong. But we can now see why Carnap was always puzzled by Quine's insistence that pure semantics makes no sense: it is impossible to state Quine's objections to Carnap's analytic-synthetic distinction in Carnap's own terms.

## 76. The Roots and Strategy of Putnam's Arguments

Before we look at details, it will help to discuss the roots and strategy of Putnam's arguments against the analytic-synthetic distinction.

We saw in Chapter Four that Carnap designed his analytic-synthetic distinction to solve the positivists' problem of reconciling our knowledge of logic and mathematics with empiricism (§51). Carnap's "solution" to the positivists' problem was shaped by his "motivating insight" that if investigators are to agree or disagree at all, they must share precise rules for evaluating their assertions. Once the rules for a language *L* are precisely specified, the truth values of some of the sentences of *L* may be deduced from the rules alone, whereas the truth values of the other sentences of *L* can be discovered only through empirical investigation. The former sentences are either analytic or contradictory, and the latter are synthetic. Carnap proposed that we view logical and mathematical truths as among the analytic or contradictory sentences of language systems we are free to adopt. Since the truth values of analytic and contradictory sentences of a language can be deduced from the rules of that language, we can evaluate such sentences without appeal to empirical evidence or a special faculty of pure reason.

In this "solution" of the positivists' problem, Carnap took for granted that if the truth of a sentence *S* can be deduced from the rules for *L*, it is reasonable to accept *S* without any empirical evidence, and to hold *S* immune from disconfirmation by all empirical evidence. In Carnap's view, investigators are free to adopt any rules they like, and thereby to commit themselves to accepting without evidence any statement whose truth can be deduced from those rules. Once we have adopted a set of rules for a language system *L* in which the truth of a given sentence *S* may be deduced without appeal to any evidence, to know that *S* is true is just to see that *S* follows from the rules of *L*.

Unlike Quine, Putnam does not directly question this aspect of Carnap's "solution" to the positivists' problem. Putnam finds the notion of a semantical rule for an artificial language unproblematic, and he accepts that if we specify rules for using the expressions of a language *L*, some of the sentences of *L* may be said to be true in virtue of those rules.<sup>2</sup>

## SIX

### Putnam's Reasons for Rejecting Carnap's Analytic-Synthetic Distinction

#### 75. Putnam and the Participant Perspective

In this chapter I recast Putnam's central arguments in "The Analytic and the Synthetic"<sup>1</sup> as criticisms of Carnap's analytic-synthetic distinction. On my reconstruction, Putnam starts by taking our linguistic practices at face value; using examples from the history of science, and sketching new ways of thinking about belief and reference, Putnam shows that the analytic-synthetic distinction prevents us from properly describing our linguistic practices.

My reconstruction highlights fundamental differences between Putnam's and Quine's reasons for rejecting the analytic-synthetic distinction. As I see it, Putnam's conception of the starting point and task of philosophy is fundamentally at odds with Quine's scientific naturalism and indeterminacy thesis. Working from Putnam's starting point, I explain why Quine's scientific naturalism prevents us from properly describing our linguistic practices. If we start by taking at face value our actual practices of agreeing, disagreeing, evaluating assertions, and resolving disputes, and we gradually remove obstacles to our understanding of these practices, we may eventually overcome the attractions of entrenched frameworks from which our participant perspective looks philosophically unimportant. My aim in this chapter is to begin this gradual process.

Putnam's central arguments in "The Analytic and the Synthetic" may be seen as criticisms of Carnap's view that if a sentence of a language  $L$  is not true in virtue of the rules of  $L$ , then it is *synthetic*—true or false in virtue of "empirical" observations. To understand this aspect of Carnap's analytic-synthetic distinction, it is important to keep in mind that for Carnap there is no conception of empirical evidence apart from a precise statement of the rules for using sentences of a language system. The reason is that Carnap's "motivating insight"—that if investigators agree or disagree they must share precise rules for evaluating their assertions—together with his proposals for codifying rules of language systems, determines what counts in any given language system as "empirical" confirmation or disconfirmation. So built into Carnap's "solution" to the positivists' problem is the principle that if a sentence is not true in virtue of rules, then its truth or falsity *must* be open to "empirical" confirmation or disconfirmation. In other words, in Carnap's view it is *reasonable to hold a sentence  $S$  of language  $L$  immune from disconfirmation by all empirical evidence only if  $S$  is true in virtue of the rules for  $L$* . I call this *Carnap's empiricist principle*.

To challenge Carnap's empiricist principle, Putnam exhibits false sentences that physicists at one time held immune from disconfirmation by all empirical evidence. These sentences are not analytic, since they are false, nor should they be understood as synthetic in Carnap's sense, since reasonable investigators held them immune from empirical disconfirmation. Putnam's examples apparently undermine Carnap's empiricist principle: we must either give up this principle, or redescribe Putnam's examples.

As we will see, however, Putnam's descriptions of his examples are compelling enough to raise serious questions about Carnap's analytic-synthetic distinction. The reason is that the descriptions are rooted in our perspective as participants in ongoing ordinary and scientific inquiries. From the participant perspective, we take at face value our actual practices of agreeing, disagreeing, evaluating assertions, and resolving disputes. If we take seriously this participant point of view, we may agree with Putnam that the most important task for philosophy is to remove obstacles to the proper description of these practices.<sup>3</sup> We are then in position to see the force of Putnam's claim that the positivists' analytic-synthetic dis-

tinction should be rejected because it prevents us from attaining a clear view of our inquiries.<sup>4</sup>

From Carnap's point of view, of course, Putnam's purported counterexamples to the empiricist principle look unconvincing. To remain faithful to the "motivating insight" that if investigators agree or disagree they must share precise rules for evaluating their assertions, Carnap would reject Putnam's descriptions of our everyday and scientific inquiries.

But anyone who is not dogmatically committed to Carnap's "motivating insight" should be convinced by Putnam's counterexamples. I agree with Putnam that one important goal of philosophy is to remove obstacles to our understanding of our actual practices of agreeing, disagreeing, evaluating assertions, and resolving disputes. In my view, Putnam's counterexamples and descriptions show that Carnap's analytic-synthetic distinction prevents us from properly describing these practices. Once we appreciate these points, Carnap's analytic-synthetic distinction loses whatever initial appeal it may have had for us.

## 77. Changes in Belief versus Changes in Reference

In "The Analytic and the Synthetic" Putnam discusses a number of examples that challenge Carnap's empiricist principle. I will focus on Putnam's discussion of the methodological role of definitions of energy in pre-relativistic and relativistic physics.

Putnam observes that before Einstein ' $e = \frac{1}{2}mv^2$ ', the pre-relativistic equation for kinetic energy, might have seemed true by definition:

Certainly, before Einstein, any physicist might have said, ' $e = \frac{1}{2}mv^2$ ', that is just the definition of 'kinetic energy'. There is no more to it than that. The expression 'kinetic energy' is, as it were, a sort of abbreviation for the longer expression 'one-half the mass times the velocity squared'.<sup>5</sup>

Let us now briefly consider how Carnap might reconstruct the methodological role of ' $e = \frac{1}{2}mv^2$ ' in pre-relativistic physics. The equation ' $e = \frac{1}{2}mv^2$ ' is expressed in the language of physics, which Carnap would count as a "natural" language, since the rules for the correct use of its expressions are not explicitly stated in one of his

formalized language systems. To reconstruct the methodological role of  $'e = \frac{1}{2}mv^2'$  we must then specify precise semantical rules for using  $'e = \frac{1}{2}mv^2'$ , together with coordinative definitions that correlate the semantical rules with the linguistic behaviors of the physicists who accepted  $'e = \frac{1}{2}mv^2'$ . The description of the methodological role of the equation rests on coordinative definitions, and so more than one reconstruction is compatible with all the physicists' speech behaviors. But since we would naturally say that the physicists treated the equation as immune from disconfirmation, the obvious choice would be to describe the equation as a definition of the term  $'e'$  (or as a logical consequence of other definitions). This could be done by treating  $'e = \frac{1}{2}mv^2'$  as a postulate of the language system in which the reconstruction is given. Relative to this reconstruction  $'e = \frac{1}{2}mv^2'$  is true in virtue of the rules of the language system.

Putnam notes that to accommodate Einstein's principle that all physical laws must be Lorentz-invariant a different equation was required: "Einstein . . . changed the definition of kinetic energy. That is to say, he replaced the law  $'e = \frac{1}{2}mv^2'$  by a more complicated law. If we expand the Einstein definition of energy as a power series, the first two terms are  $'e = mc^2 + \frac{1}{2}mv^2 + \dots'$ "<sup>16</sup> Since physicists now treat this equation as immune from disconfirmation, the obvious choice for Carnap would be to describe the new equation as a new definition of the term  $'e'$  (or as a logical consequence of other new definitions). On this reconstruction  $'e = mc^2 + \frac{1}{2}mv^2 + \dots'$  is also true in virtue of the rules of the language.

Putnam argues that if we view these two equations as true in virtue of the rules of the language, we will misunderstand the actual role of the term *energy* in the language of physics as we use it. He observes that "whatever the status of the energy definition may have been before Einstein, in revising it, *Einstein treated it as just another natural law*. . . . Among the equations that had to be revised . . . was the equation  $'e = \frac{1}{2}mv^2'$ ."<sup>17</sup> To see the the equation  $'e = \frac{1}{2}mv^2'$  as just another natural law is to see that the scientists who revised  $'e = \frac{1}{2}mv^2'$  disagree with those who accepted it. From within the evolving language of physics, this change in belief is signaled by a change in the equation for  $'e'$ , and by the continued use of the same term  $'e'$  to refer to *energy*.

Putnam's central objection, then, is that if we accept Carnap's

analytic-synthetic distinction, we will not see that the scientists who accept the relativistic equation disagree with those who accepted the earlier one. The problem may be traced to Carnap's empiricist principle that it is reasonable to hold a sentence S immune from disconfirmation by all empirical evidence only if S is true in virtue of the rules for L. Since prior to Einstein it was reasonable to hold  $'e = \frac{1}{2}mv^2'$  immune from disconfirmation by all empirical evidence, Carnap's empiricist principle leads him to view the equation as true in virtue of the rules. For the same reason, he will also view the relativistic equation as true in virtue of the rules. But an unblinkered look at our use of these two equations shows that they make conflicting claims about *energy*. We must therefore reject Carnap's empiricist principle, and with it the analytic-synthetic distinction from which it stems: "to speak of Einstein's contribution as a redefinition of kinetic energy is to assimilate what actually happened to a wholly false model."<sup>18</sup>

## 78. Law-cluster Concepts and Terms

To make sense of the actual evolution of our theories of kinetic energy, Putnam reminds us that it is natural to see  $'e'$  as referring to the same quantity in both  $'e = \frac{1}{2}mv^2'$  and  $'e = mc^2 + \frac{1}{2}mv^2 + \dots'$ . To remind us of this familiar aspect of our linguistic practices, Putnam offers the following sketch of a commonsense alternative to Carnap's view of the role of the term  $'e'$ : "The extension of the term 'kinetic energy' has not changed. If it had, the extension of the term 'energy' would have to have changed. But the extension of the term 'energy' has not changed. The forms of energy and their behavior are the same as they always were, and they are what physicists talked about before and after Einstein."<sup>19</sup> To help make sense of this alternative Putnam introduces the notion of a 'law-cluster concept.'<sup>20</sup>

Law-cluster concepts are expressed by law-cluster terms. Such terms occur in statements of many different scientific laws, any of which may be given up without changing the concept expressed by that term. Once we view  $'e'$  as a law-cluster term, we can accept that the reference of the term  $'e'$  did not change when the old equation was given up, and we can view our acceptance of the new relativistic definition of energy as reflecting a change in our beliefs about



energy.<sup>11</sup> This new model of the semantic role of terms like 'e' helps us to see that those who accepted the relativistic energy equation disagree with those who accepted the earlier one.

### 79. Framework Principles and the Contextually A Priori

If we accept Putnam's description of the role of terms like 'e', we can no longer view 'e =  $\frac{1}{2}mv^2$ ' as true in virtue of rules, and so we can no longer accept Carnap's account of why it was reasonable for physicists to hold 'e =  $\frac{1}{2}mv^2$ ' immune from empirical disconfirmation. Putnam's alternative is rooted in his descriptions of our actual practices of evaluating and revising beliefs. By reminding us of how physicists respond to empirically wrong answers and false predictions, Putnam tries to persuade us that it was reasonable for physicists to hold 'e =  $\frac{1}{2}mv^2$ ' immune from empirical disconfirmation because they did not know of a plausible alternative.

Putnam takes for granted that we have no grasp on what is reasonable or unreasonable apart from our actual practices of evaluating and revising beliefs. So to support his alternative to Carnap's views about what is reasonable or unreasonable, Putnam reminds us of what we actually say and do in response to unexpected observations. He observes that

if a physicist makes a calculation and gets an empirically wrong answer, he does not suspect that the mathematical principles used in the calculation may have been wrong (assuming that those principles are themselves theorems of mathematics) nor does he suspect that the law 'f = ma' may be wrong. Similarly, he did not frequently suspect before Einstein that the law 'e =  $\frac{1}{2}mv^2$ ' might be wrong. . . . These statements, then, have a kind of preferred status. They can be overthrown, but not by an isolated experiment. They can be overthrown only if someone incorporates principles incompatible with those statements in a successful conceptual system.<sup>12</sup>

Here Putnam describes how physicists *actually* revise their beliefs, and hints at a way of making sense of these revisions. He observes that a physicist does not revise his mathematical theorems when he "makes a calculation and gets an empirically wrong answer." In Putnam's view, this shows that it is reasonable in those circum-

stances to hold the theorems immune from disconfirmation. Similarly, if a physicist knows of no plausible alternatives to 'e =  $\frac{1}{2}mv^2$ ' he will not reject it even if he gets a false prediction from the theory that contains it. In Putnam's view this is enough to show that it is reasonable in these circumstances to continue accepting 'e =  $\frac{1}{2}mv^2$ ' despite a false prediction.

This deflationary view of when it was reasonable for physicists to hold 'e =  $\frac{1}{2}mv^2$ ' immune from empirical disconfirmation may be extended to hold for what Putnam calls "framework principles":

there are many, many principles—we might broadly classify them as 'framework principles'—which have the characteristic of being so central that they are employed as auxiliaries to make predictions in an overwhelming number of experiments, without themselves being jeopardized by any possible experimental results. This is the classical role of the laws of logic; but it is equally the role of certain physical principles, e.g. 'f = ma', and . . . the law 'e =  $\frac{1}{2}mv^2$ ', at the time when those laws were still accepted.<sup>13</sup>

By reminding us of how physicists respond to empirical results, Putnam aims to persuade us that it is reasonable to hold framework principles immune from empirical disconfirmation.

We can summarize these points by saying that framework principles are *contextually a priori* and *revisable*. To say that a statement S is contextually a priori is to say that S is "necessary relative to a body of knowledge": "when we say that a statement is necessary relative to a body of knowledge, we imply that it is included in that body of knowledge and that it enjoys a special role in that body of knowledge. For example, one is not expected to give much of a reason for that kind of statement. But we do not imply that the statement is necessarily *true*, although, of course, it is thought to be true by someone whose knowledge that body of knowledge is."<sup>14</sup> In Putnam's view, if S is contextually a priori it is reasonable to hold S immune from disconfirmation even if S is not analytic.

### 80. An Objection from Carnap's Point of View

As a step toward further clarification of Putnam's alternative, let us consider an objection from Carnap's point of view. We saw in Chapter Four that for Carnap there is no conception of "empirical

For Carnap this is unacceptable. If under these circumstances A and B assume that they disagree, each will *feel* he is right, even though he knows that the other does not agree with him about how to settle the dispute. As we saw in Chapter Four, this is exactly the kind of situation Carnap deplores, both in traditional metaphysics and in contemporary debates in the foundations of mathematics. In his view, to suppose that there are "disagreements" of this kind leads to obscurity and confusion. His "motivating insight" is that investigators can disagree with each other only if they share rules for settling their disagreement. From the start Carnap rejects the *very idea* of "disagreements" between investigators who do not agree about how to decide whether their assertions are true or false.

We may sum up what I shall call Carnap's objection as follows: Putnam's picture is unacceptable because it allows for the possibility of intersubjectively irresolvable disagreements between investigators.

### 81. A Reply from Putnam's Point of View

There is no neutral position from which to evaluate this objection. From Putnam's point of view, Carnap's "motivating insight" keeps Carnap from seeing that investigators can disagree even if they do not share precise rules for settling their disagreement. If we accept Putnam's starting point and his view of the task of philosophy, we will conclude that Carnap's central mistake is to decide in advance what can count as agreement and disagreement.

Properly viewed, Putnam's kinetic energy example shows that *we often commit ourselves to the outcome of investigations into the truth of a given sentence S even when we don't know of any precise rules for discovering the truth value of S*. To appreciate this aspect of our actual practices of agreeing and disagreeing, we must take seriously the evolution of our beliefs as a result of investigations that extend over time; we must take a *diachronic* view of inquiry, not merely a *synchronic* one. Carnap's synchronic view of inquiry supports his motivating insight that investigators can agree or disagree only if they share precise rules for evaluating their assertions. All changes in our procedures for evaluating statements are then seen as merely pragmatic, and considered irrelevant to the reconstruction of how

evidence" apart from a precise statement of the rules for using sentences of a language system, and that this reflects Carnap's "motivating insight" that investigators can agree or disagree only if they share rules for evaluating their assertions. Taking Carnap's "motivating insight" for granted, I'll develop an objection on Carnap's behalf to Putnam's description of the methodological status of ' $e = \frac{1}{2}mv^2$ '. The objection amounts to an argument for the following claim: *if we accept Putnam's description of the methodological status of ' $e = \frac{1}{2}mv^2$ ', then two investigators might disagree about whether ' $e = \frac{1}{2}mv^2$ ' is true even though they do not agree about how to settle their dispute*. Given the "motivating insight," this claim reduces Putnam's position to absurdity in Carnap's eyes.

To see the source of the argument, recall that according to Putnam it was reasonable for physicists to hold ' $e = \frac{1}{2}mv^2$ ' immune from disconfirmation because they did not know of any plausible alternative. Built into this historical example is the fact that physicists now agree that relativistic physics is more plausible than pre-relativistic physics, and so they accept the relativistic energy equation and reject ' $e = \frac{1}{2}mv^2$ '. The objection I have in mind is based in the thought that on Putnam's view this agreement seems to be just an accident of history. For all Putnam says, it seems that there could be two investigators who disagree about which of these alternative theories is more plausible, and who therefore disagree about ' $e = \frac{1}{2}mv^2$ ', even though they do not agree about how to settle their dispute.

To illustrate this possibility, suppose that investigator A finds pre-relativistic physics more reasonable than relativistic physics, and investigator B takes the opposing view. They reach conflicting verdicts on the sentence ' $e = \frac{1}{2}mv^2$ ': A affirms it, and B rejects it. Suppose also that A and B know that their disagreement is not due to a mistake in deductive reasoning, or to a disagreement about any empirical observation. For instance, A agrees with B that if relativistic physics is true, then ' $e = \frac{1}{2}mv^2$ ' is false, and B agrees that if pre-relativistic physics is true, then ' $e = \frac{1}{2}mv^2$ ' is true. They realize that their disagreement comes down to a dispute about which physical theory it is more reasonable to accept. But they do not agree about how to resolve this dispute. Putnam's picture commits us to accepting that under these circumstances A and B genuinely *disagree* about whether ' $e = \frac{1}{2}mv^2$ ' is true.

our statements are evaluated. From this synchronic point of view, Carnap can't see the shared commitments that are expressed in our continuing participation in an evolving inquiry over a long stretch of time.

In our illustration of Carnap's objection, we supposed that A and B disagree about whether it is reasonable to accept ' $e = \frac{1}{2}mv^2$ ', even though they agree that if relativistic physics is true, then ' $e = \frac{1}{2}mv^2$ ' is false. Since A knows of the relativistic alternative, it is not reasonable for A to dismiss B's reasons for rejecting ' $e = \frac{1}{2}mv^2$ '. Nevertheless A is not convinced that B's alternative is more plausible, nor is B convinced that A's alternative is more plausible. Carnap assumes that in such a situation A's and B's dispute will always degenerate into pointless bickering.

But Carnap overlooks the possibility that through discussion and further inquiry, A may persuade B or vice versa. As a result of ongoing discussion and inquiry, our beliefs often evolve in unexpected ways. Since Putnam views our inquiries diachronically, he sees such changes in belief as rational, not merely pragmatic.<sup>15</sup>

Naturally, Putnam's diachronic picture of shared commitments can't rule out sheer *stubbornness*. An investigator may insist on accepting a given statement even after hearing a very convincing case for giving it up. If he is unable to give any reasons for not rejecting the disputed statement, his stubbornness becomes an obstacle to further inquiry. If his stubbornness survives all criticism, it will effectively isolate him from other investigators. In some rare and extreme cases we would no longer say that he disagrees with the other investigators. This is the grain of truth behind Carnap's objection.

There is of course a difference between being merely stubborn and having the courage of one's convictions. An authoritative and unyielding personality may persuade others of his views by claiming the high ground, and refusing to take their alternatives seriously. But to remain persuasive an investigator must be open to other points of view; he will not remain persuasive for long if he dogmatically refuses to reevaluate his views.

Simply put, the answer to Carnap's objection is that despite his "motivating insight," we often disagree about the truth value of a given statement even when we don't agree on precise rules for settling our dispute.

## 82. Analyticity and One-Criterion Words

Despite these arguments against Carnap's empiricist principle and motivating insight, Putnam thinks there are analytic sentences in natural languages. He starts with a paradigm example of an "analytic" sentence—"Bachelors are unmarried men"—and offers a "rationale" for saying that such sentences are analytic.<sup>16</sup> His rationale is that the subject terms of analytic sentences are "one-criterion words," not law-cluster terms: since analytic sentences contain no law-cluster terms, we can't give them up without changing the subject. For example, the word 'bachelor' is true of  $x$  if and only if  $x$  is an unmarried human adult male. In fact there is no other criterion for being a bachelor, and so if we give up this criterion, we change the meaning of 'bachelor'.

From this description it might seem that unlike framework principles such statements as *Bachelors are unmarried men* could serve as paradigms of knowledge based solely on meaning, independent of any substantive "empirical" beliefs. These paradigms could then help us to "solve" the positivists' problem of reconciling our knowledge of logic and mathematics with empiricism. In Putnam's view, however, when we judge that 'Bachelors are unmarried men' is analytic, we take for granted that there are no *laws* about bachelors, and that 'bachelor' is a one-criterion word. So ordinary "analytic" statements like *Bachelors are unmarried men* are not paradigms of belief based solely on meaning. Like framework principles, these ordinary "analytic" statements also provide counterexamples to Carnap's empiricist principle that it is reasonable to hold a sentence  $S$  immune from disconfirmation only if  $S$  is true in virtue of the rules for  $L$ , independent of any substantive "empirical" beliefs.

In Putnam's view, we have good reason to believe, for example, that 'bachelor' is a one-criterion word, and that the sentence 'Bachelors are unmarried men' is an "analytic definition." But these beliefs go beyond the positivists' picture of truths based on meaning, independent of "empirical" fact. For instance, we can imagine that unknown to us there are empirical laws about bachelors, currently under investigation by an obscure group of experimental psychologists. If this seems too implausible, we can also imagine a possible future in which psychologists discover such laws.<sup>17</sup> In this imagined future, philosophers familiar with

Putnam's distinction between one-criterion and law-cluster terms would say, "just as physicists used to think that the kinetic energy definition was true by definition, so they used to think that 'Bachelors are unmarried men' was true by definition. In both cases they were wrong, for 'kinetic energy' and 'bachelor' are law-cluster terms."

Despite these possibilities, when we classify a sentence as analytic in Putnam's sense, as he says, "we use what we know." Putnam's point is better expressed in terms of belief: it is because we believe that there are and will be no laws about bachelors, and that 'bachelor' is a one-criterion word, that we classify 'bachelors are unmarried men' as analytic.<sup>18</sup>

This is an inevitable consequence of Putnam's starting point. On my reconstruction, Putnam begins by taking our actual linguistic practices at face value. Our participation in these practices—hence any satisfactory description of the methodological role of both framework principles and analytic statements—always presupposes a background of beliefs.

### 83. The Method behind Putnam's Criticisms

I have proposed that we view Putnam's criticisms of the analytic-synthetic distinction as attempts to show that the distinction prevents us from properly describing our linguistic practices. To prepare for the next section, in which I explain why Putnam's and Quine's criticisms of the analytic-synthetic distinction are incompatible, I will now consolidate and summarize my view of the method behind Putnam's criticisms.

Putnam invites us to engage in a three-stage process of reflecting about how we should describe our use of language. In the first stage Putnam uses examples to remind us of familiar aspects of our use of language; in the second he explains why these aspects seem mysterious if we are in the grip of the analytic-synthetic distinction; and in the third he sketches a new way of looking at the examples, showing us how to describe them in a broader context within which we find their familiar aspects clear and unproblematic.

For instance, Putnam invites us to engage in a three-stage process of reflecting about how we should describe the change in our theoretical beliefs about kinetic energy. In the first stage he reminds us that prior to Einstein's theory of relativity it was reasonable for

physicists to hold ' $e = \frac{1}{2}mv^2$ ' immune from disconfirmation, even though we now believe the equation is false. In the second stage Putnam notes that if we are in the grip of the positivists' analytic-synthetic distinction, we feel compelled to describe this equation as true in virtue of meaning. He points out that if we describe the equation as true in virtue of meaning, we fail to see that ' $e = \frac{1}{2}mv^2$ ' is false, and that the physicists who accept the relativistic energy equation *disagree* with the scientists who accepted ' $e = \frac{1}{2}mv^2$ '. To help us make sense of this aspect of the kinetic energy equations, in the third stage Putnam proposes that we view ' $e$ ' as a law-cluster term—a term that expresses a law-cluster concept. Once we view ' $e$ ' as a law-cluster term, we can describe the revision of ' $e = \frac{1}{2}mv^2$ ' as the linguistic expression of a radical change in our beliefs about kinetic energy. This description is further clarified when we view ' $e = \frac{1}{2}mv^2$ ' as what Putnam calls a "framework principle"—a statement that is so central to a body of beliefs that it is held practically immune from disconfirmation. So in the third stage of his reflections about how to describe the kinetic energy equations, Putnam tries to demystify the natural thought that our acceptance of the new relativistic definition of energy marked a change in our beliefs about kinetic energy. The effect of Putnam's third stage is to show that a commitment to Carnap's analytic-synthetic distinction would prevent us from properly describing the evolution in our theoretical beliefs about kinetic energy.

Putnam's discussion of the bachelor example may also be seen as an attempt to persuade us that the analytic-synthetic distinction prevents us from properly describing our use of language. In the first stage Putnam reminds us of such statements as 'Bachelors are unmarried men', which seem analytic. In the second stage we recall that positivists would view such statements as true solely in virtue of meaning, completely independent of "empirical" facts. In the third stage Putnam offers an alternative description of such statements as 'Bachelors are unmarried men'. He agrees that they are "analytic," in the sense that we cannot give them up without changing the subject, but he denies that our judgment that we can't give them up without changing the subject is completely independent of our background beliefs about bachelors. The reason we can't revise the statement *Bachelors are unmarried men* without changing the subject is that the only criterion for  $x$ 's being a bachelor is that  $x$  be an unmarried man, and so 'bachelor' is what

Putnam calls a "one-criterion" word. When we see that 'bachelor' is a "one-criterion" word, we can see why we find it natural to say that 'Bachelors are unmarried men' is analytic. Yet our judgment that "bachelor" is a one-criterion word reflects our "empirical" belief that there are no natural laws about bachelors. Putnam concludes that a commitment to Carnap's analytic-synthetic distinction would prevent us from properly describing the methodological role of trivial analytic statements like 'All bachelors are unmarried'.

To highlight the method behind these arguments, it is helpful to compare Carnap's analytic-synthetic distinction with framework principles such as  $e = \frac{1}{2}mv^2$ . Putnam emphasizes that framework principles "can be overthrown only if someone incorporates principles incompatible with those statements in a successful conceptual system."<sup>19</sup> Carnap's analytic-synthetic distinction is like a framework principle in the sense that we can overthrow it only if we incorporate rival descriptions of our language use into a coherent system. Unlike a framework principle, however, Carnap's analytic-synthetic distinction is not part of a scientific theory and so we should not think of it as true or false. To discredit Carnap's philosophical picture of inquiry it would be enough to sketch a more plausible philosophical picture.

But Putnam goes further than this: he presents his alternative as a clarification of what in a sense we *already knew* about how our language works. To "overthrow" Carnap's analytic-synthetic distinction Putnam tries to remind us of familiar ways in which ordinary aspects of our use of language fit together in a natural and coherent system. Putnam's talk of "law-cluster concepts," "framework principles," and "one-criterion words" highlights aspects of our use of language that we can't see clearly if we are under the spell of Carnap's analytic-synthetic distinction. In this way Putnam aims to break the spell of the analytic-synthetic distinction, and to change the way we look at our linguistic practices.

#### 84. Why Putnam's Critique of the Analytic-Synthetic Distinction Is Incompatible with Quine's

We are now in a position to see that despite superficial similarities, Putnam's and Quine's criticisms of Carnap's analytic-synthetic distinction are incompatible.

Let's start with the similarities. The most important one is that

both Putnam and Quine challenge Carnap's descriptions of our ordinary and scientific practices. As we saw in Chapter Five, Quine's scientific naturalism leads him to reject Carnap's notion of a semantical rule and, with it, Carnap's analytic-synthetic distinction. And in §§75-83 we have seen that Putnam's descriptions of our actual practices of agreeing, disagreeing, evaluating assertions, and resolving disputes challenge Carnap's motivating insight that investigators can agree or disagree with each other only if they share precise rules for evaluating their assertions.

A related similarity is that Quine and Putnam both highlight pragmatic aspects of our acceptance and revision of statements. In "Carnap and Logical Truth,"<sup>20</sup> Quine claims that there is no important methodological distinction to be drawn between statements that were introduced by acts of legislative definition or postulation,<sup>21</sup> on the one hand, and well-attested hypotheses, on the other:

Suppose a scientist introduces a new term, for a certain substance or force. He introduces it by an act either of legislative definition or of legislative postulation. Progressing, he evolves hypotheses regarding further traits of the named substance or force. Suppose now that some such eventual hypothesis, well attested, identifies this substance or force with one named by a complex term built up of other portions of his scientific vocabulary. We all know that this new identity will figure in the ensuing developments quite on a par with the identity which first came of the act of legislative definition, if any, or on a par with the law which first came of the act of legislative postulation. Revisions, in the course of further progress, can touch any of these affirmations equally. Now I urge that scientists, proceeding thus, are not thereby slurring over any meaningful distinction. Legislative acts occur again and again; on the other hand a dichotomy of the resulting truths themselves into analytic and synthetic, truths by meaning postulate and truths by force of nature, has been given no tolerably clear meaning even as a methodological ideal.<sup>22</sup>

On the surface this passage appears compatible with Putnam's observation that the languages we use in our everyday and scientific inquiries include "law-cluster" terms that occur in a large number of laws, many of which can be revised without a change in subject. A passage from chapter 2 of Quine's *Word and Object*

strengthens the impression that Quine's and Putnam's descriptions of our scientific inquiries are compatible:

in theoretical science, unless as recast by semantics enthusiasts, distinctions between synonymies and "factual" equivalences are seldom sensed or claimed. Even the identity historically introduced into mechanics by defining 'momentum' as 'mass times velocity' takes its place in the network of connections on a par with the rest; if a physicist subsequently so revises mechanics that momentum fails to be proportional to velocity, the change will probably be seen as a change of theory and not particularly of meaning. [Here Quine cites the passage I just quoted from "Carnap and Logical Truth".] Synonymy intuitions do not emerge here, just because the terms are linked to the rest of language in more ways than words like 'bachelor' are.<sup>23</sup>

To add to the appearance of compatibility, Quine endorses Putnam's observation that 'Bachelors are unmarried men' is analytic because 'bachelor' is a one-criterion word: "One looks to 'unmarried man' as semantically anchoring 'bachelor' because there is no socially constant stimulus meaning to govern the use of the word; sever its tie with 'unmarried man' and you leave it no very evident social determination, hence no utility in communication."<sup>24</sup> Quine cites Putnam's paper "The Analytic and the Synthetic," and endorses Putnam's account of what Quine calls "the synonymy intuition" that 'Bachelors are unmarried men' is analytic: "My account fits with [Putnam's] and perhaps adds to the explanation."<sup>25</sup> So it appears that Quine's and Putnam's reasons for rejecting Carnap's analytic-synthetic distinction are compatible.

This appearance is misleading. For Quine's methodological reflections in the passages just quoted are of a piece with his scientific naturalism and his "motivating insight" that "we can know external things only through impacts at our nerve endings." When Quine claims in the passage from *SX* of "Carnap and Logical Truth" that the analytic-synthetic distinction "has been given no tolerably clear meaning even as a methodological ideal," he takes for granted that the only "meaningful distinctions" to be found in our actual scientific practices are those that are determined by speech dispositions. We have already seen that for Quine "the ques-

tion whether two expressions are alike or unlike in meaning has no determinate answer, known or unknown, except insofar as the answer is settled in principle by people's speech dispositions, known or unknown."<sup>26</sup> By these standards there is an indeterminacy of translation. In Quine's view, our habitual use of homophonic "translations" of our fellow English speakers merely reflects our subjective preference for one of many inequivalent "translations" that preserve all speech dispositions, and hence capture all there is to meaning.

In contrast, Putnam's criticisms are inextricably linked to our commonsense view that the references of words of a public language are the same for all competent speakers of the language. In the kinetic energy case, for instance, Putnam takes for granted our ordinary judgments that the two equations make incompatible statements about kinetic energy, and that 'e' has the same reference in both equations. From our perspective as participants in ongoing inquiries, these judgments look secure. But from Quine's point of view, these judgments rest on our scientifically arbitrary "choice" of the homophonic translation manual between idiolects.<sup>27</sup>

Quine would say that Putnam's observation that the term 'e' has the same reference in both equations reflects Putnam's "choice" of a homophonic translation between his idiolect and those of the physicists who accepted 'e = 1/2mv<sup>2</sup>'. In Quine's view, this homophonic translation is not the only acceptable one. We may choose to map the earlier physicists' term 'e' to an entirely different term of the later physicists' vocabulary; if this mapping is part of a systematic "translation" that preserves all speech dispositions, then by Quine's standards we are not "slurring over any meaningful distinction."<sup>28</sup> So when Quine observes that "if a physicist subsequently so revises mechanics that momentum fails to be proportional to velocity, the change will probably be seen as a change of theory and not particularly of meaning,"<sup>29</sup> he is just saying that the "translation" between the earlier and later physical theories will probably in practice be close to homophonic, with the differences between the two theories viewed as differences in belief, not meaning.

One might be tempted to see this as just like Putnam's observations that the physicists' revisions of the pre-relativistic kinetic energy equation reflect a change in belief, not reference. But this

temptation reflects a misreading of either Quine or Putnam. For Quine's main point in chapter 2 of *Word and Object*, where this discussion of Putnam appears, is that for sentences that are not firmly tied to sensory stimulation, our "intuitive" distinctions between changes in belief and changes in meaning merely reflect our preference for one of many empirically adequate "translations" between speakers. Strictly speaking, for Quine, the physicists' speech dispositions do not determine that they should use the homophonic manual between the earlier and later physical theories, since "unless pretty firmly and directly conditioned to sensory stimulation, a sentence S is meaningless except relative to its own theory; meaningless intertheoretically."<sup>30</sup> Mappings between the sentences and terms of earlier and later physical theories always reflect our tacit "choice" of one set of "analytical hypotheses" over another empirically adequate set.

So despite superficial similarities between Putnam's and Quine's rejections of the analytic-synthetic distinction, their conceptions of the starting point and task of philosophy are incompatible. Starting from our perspective as participants in actual linguistic practices, Putnam shows that the analytic-synthetic distinction prevents us from properly describing our ordinary and scientific inquiries. In contrast, Quine's scientific naturalism inevitably leads to his indeterminacy thesis, according to which there are many inequivalent ways to "mesh" our dispositions with those of other investigators. Hence for Quine the *actual* evolution of our theoretical beliefs illustrates just one of many inequivalent ways in which our dispositions would "mesh" with those of previous investigators.

### 85. Against Indeterminacy

But Quine's indeterminacy thesis cuts both ways, depending on whether one starts with Quine's scientific naturalism or with our perspective as participants in actual inquiries. Quine's scientific naturalism is inextricable from his "motivating insight" that "we can know external things only through impacts at our nerve endings," and this "insight" underlies Quine's indeterminacy thesis. But there is no more reason to embrace Quine's naturalistic empiricist than there is to embrace Carnap's analytic-synthetic distinction.

Defenders of Quine's indeterminacy thesis typically focus on the

question of whether there are facts that determine how our words are to be translated.<sup>31</sup> Some philosophers accept Quine's naturalistic picture, and so they feel they must find such facts.<sup>32</sup> In my view this approach is fruitless. Instead of looking for facts that determine how our words must be translated, we should examine Quine's reasons for thinking that there are countless radically different "translations" that capture all that is objective to meaning. If we abandon the assumptions that lead Quine to this conclusion, there is no need to find facts that determine how our words are to be translated. Without any special reason to question our actual linguistic interactions, both *within* natural languages and *between* them, we have no reason to accept Quine's indeterminacy thesis.

Quine's "insight" that "we can know external things only through impacts at our nerve endings" underlies his naturalized epistemology and his view of what is objective in translation. The same "insight" is reflected in Quine's claim that the objective empirical meaning of a sentence S of a speaker's idiolect is determined by his dispositions to assent to and dissent from S and other sentences under various prompting stimulations. Since between any two idiolects there are countless different translations that "preserve the totality of speech dispositions," Quine concludes that between any two idiolects there are countless different manuals of translation. Almost all of these "translations" would be automatically ignored or dismissed from our perspective as participants in actual linguistic practices. Quine nevertheless insists that any manual of "translation" that preserves the totality of speech dispositions thereby preserves what is "objective" in linguistic meaning. The result is indeterminacy.<sup>33</sup>

Although Putnam wrote "The Analytic and the Synthetic" before Quine first published his argument for the indeterminacy of translation,<sup>34</sup> when properly viewed Putnam's examples can help to disarm Quine's indeterminacy thesis. To "overthrow" Carnap's analytic-synthetic distinction, Putnam reminds us of familiar ways in which ordinary features of our use of language fit together in a natural and coherent system. Once we take our participant view of language seriously, we can no longer accept Carnap's "motivating insight" that if investigators are to agree or disagree they must share precise rules for evaluating their assertions. Similarly, to "overthrow" Quine's indeterminacy thesis, we may appeal to

Putnam's examples, together with his talk of "law-cluster concepts," "framework principles," and "one-criterion words," to show how our ordinary judgments about agreement, disagreement, sameness and difference of reference fit together. From our perspective as participants in actual linguistic practices, there is no good reason to accept Quine's "motivating insight" that "we can know external things only through impacts at our nerve endings,"<sup>35</sup> or his thesis that between any two speakers there are countless radically different "translations" that capture all that is objective to meaning.<sup>36</sup>

Like Putnam's rejection of the analytic-synthetic distinction, this rejection of Quine's indeterminacy thesis is only as convincing as Putnam's alternative descriptions of our linguistic practices. The only way to evaluate Putnam's alternative is to investigate and refine his picture of how familiar aspects of our use of language fit together when they are described from the participant perspective. Now that we have a strategy for resisting Carnap's and Quine's "motivating insights" about how our scientific inquiries *must* be described, we are in a position to take our participant perspective more seriously, and to begin to develop a new way of looking at our use of language in science and ordinary life.

#### 86. Four Preliminary Concerns

My aim in this chapter has been to uncover the roots of our perspective as participants in rule-following practices and to sketch a strategy for resisting the attractions of entrenched frameworks from which our participant perspective looks unimportant. To consolidate central themes and prepare for further developments, I will end by addressing four preliminary concerns about my methods and conclusions.

The first may be expressed as follows: 'Putnam's talk of 'agreement', 'disagreement', 'law-cluster concepts', 'framework principles', and 'one-criterion terms' is imprecise and unscientific. We can't be sure that this talk is free of obscure metaphysical assumptions unless we recast it in terms of an appropriate special science such as psychology. Yet it seems unlikely that Putnam's examples and descriptions could be recast in scientific terms. For example, there is apparently no scientific method for determining whether one speaker 'disagrees' with another.'<sup>37</sup> We may feel that we under-

stand Putnam's descriptions of our language use, but if we can't make them scientifically precise, we should not take them seriously."

My reply has two parts. First, Putnam's preliminary sketches of our participant perspective play a useful role in Putnam's argument against the analytic-synthetic distinction even though these sketches can't by themselves prevent obscure metaphysical ideas from distorting our thinking about language. The crucial point of Putnam's argument against the analytic-synthetic distinction is that if we want to understand how we evaluate our assertions in science and everyday life, we must take seriously our actual judgments about whether or not a belief is reasonable, when two investigators disagree, and when they are talking past each other. Putnam's talk of "agreement," "disagreement," "law-cluster concepts," "framework principles," and "one-criterion terms" reminds us of how we actually evaluate our assertions, and thereby helps us to see that Carnap's analytic-synthetic distinction prevents us from properly describing our everyday and scientific inquiries. Once reminded, we are in a position to address any misunderstandings fostered by Putnam's own descriptions of our inquiries.

Second, sciences such as psychology may help us to understand how we evaluate our assertions, but Putnam's arguments strongly suggest that scientific descriptions can't *replace* the descriptions we give from our perspective as participants in actual inquiries. To understand how we actually evaluate our assertions, we must understand the difference in practice between agreement and disagreement. This distinction can be drawn properly only from our perspective as participants in actual inquiries. If we restrict ourselves to descriptions couched solely in scientific terms, we in effect abandon the participant perspective, and thereby prevent ourselves from seeing how we evaluate our assertions.

Putnam's view of inquiry rests on judgments we make from our perspective as participants, judgments about what it is reasonable for an investigator to believe in a given context of inquiry. For example, Putnam reminds us that it was reasonable for physicists before Einstein to hold ' $e = \frac{1}{2}mv^2$ ', immune from disconfirmation, since they did not know of a plausible alternative. He thinks we must take these judgments seriously even if we can't describe in precise scientific terms when it is reasonable to hold a particular belief immune from disconfirmation.



These observations may prompt a second concern: "In Putnam's view, we must take seriously our judgments about what it is 'reasonable' for an investigator to believe in a given context of inquiry even if those judgments can't be redescribed or justified in purely scientific terms. So in Putnam's view, our judgments about whether a belief is 'reasonable' are higher and firmer than the standards expressed in natural science. But there are no legitimate standards higher or firmer than those expressed in natural science. Hence Putnam's view is unacceptable."

This objection is based in a mistake. Putnam aims to remove obstacles to the proper description of our actual practices of agreeing, disagreeing, evaluating assertions, and resolving disputes. In his view, we have no understanding of what is reasonable or unreasonable apart from our actual practices of evaluating our assertions in science and everyday life, and there is no legitimate perspective from which to proclaim that one of these entrenched practices is higher and firmer than any other.

Yet it may seem too restrictive to say that we have no understanding of what is reasonable or unreasonable apart from our actual practices of evaluating our assertions in science and everyday life. We should be able to question even our most deeply entrenched beliefs. This idea may foster a further concern about Putnam's view: "We should not rule out the possibility that an entire group of investigators cling irrationally to statements they ought to give up. But in Putnam's view, we are never in a position to question whether it is reasonable to hold a deeply entrenched belief immune from disconfirmation. This radical *conservatism* guts the very idea of reasonable standards for evaluating assertions."

This objection rests on the mistaken assumption that in Putnam's view if every member of a given linguistic community holds a statement S immune from disconfirmation, then it is reasonable to hold S immune from disconfirmation. In some contexts it is *unreasonable* to hold a statement immune from disconfirmation, even if no one actually recognizes this.

Putnam's observations about "framework principles" show that our judgments of whether or not a statement S is contextually a priori are based in subtle evaluations of whether it is reasonable for an investigator to treat S as contextually a priori given her other beliefs, and her evaluation of the plausibility of available alterna-

tives. To accept an individual's judgment that a statement S is contextually a priori, it is not enough for us to see that from her point of view S looks more plausible than other alternatives she is aware of. We must also determine whether her assessment of the plausibility of those alternatives is itself reasonable, and whether she has spent a reasonable amount of time and energy trying to discover alternatives. If her assessment of the alternatives is unreasonable, or her failure to become acquainted with alternatives is based in psychological needs and biases that by her own standards she should master and overcome, then it is not reasonable for her to treat S as contextually a priori. But if by her own standards the beliefs she holds immune from disconfirmation are in fact contextually a priori, then there are no legitimate grounds for criticizing her acceptance of those beliefs. Putnam's view leaves room for both of these possibilities. His descriptions of our actual practices of evaluating our assertions always leave open the possibility of doubting or revising even our most deeply entrenched beliefs.

This strongly suggests that Putnam is committed to metaphysical realism, where truth is "correspondence" and the "external world" is conceived completely independently of any of our beliefs. This may lead to another objection: "On Putnam's view we can make sense of the possibility that even our most deeply entrenched beliefs may be false. This requires that we think of truth in terms of 'correspondence' with an 'external' world, conceived independently of any of our beliefs. So even though Putnam starts by taking seriously the judgments we make as participants in actual inquiries, he ends up with an obscure metaphysical picture of truth and objectivity."

Some philosophers are happy to embrace metaphysical realism. They would say that this last "objection" is not really an objection at all. These philosophers are inclined to reconstruct Putnam's argument against positivism as follows: "Truth is one thing, and reasonable belief is another. We must reject Carnap's analytic-synthetic distinction, because it conflates truth with reasonable belief."

But in my view, this is not how Putnam criticizes the analytic-synthetic distinction. His criticism is not based in a free-floating "intuitive" understanding of truth and reasonable belief. Instead he uses examples to show that in our actual linguistic practices we distinguish between truth and contextually a priori belief. In Put-

nam's view, we have no understanding of truth and reasonable belief apart from these practices.

In Chapters Seven and Nine I will argue that, properly viewed, Putnam's descriptions of our practices of agreeing, disagreeing, evaluating assertions, and resolving disputes actually *undermine* the persistent idea that we can conceive of an "external" world completely independently of any of our substantive beliefs. For now I just want to emphasize that Putnam's criticisms of the analytic-synthetic distinction are more subtle and complex than the stripped-down version sketched two paragraphs ago. To understand the relationship between truth and reasonable belief, we must carefully investigate our actual practices. As we work through the details and consequences of our participant perspective on meaning and assertion, we should not take for granted that we can even *make sense* of the metaphysical realist's idea that our statements are true if and only if they "correspond" with an external world conceived independently of any of our beliefs.

### III

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## ANTI-INDIVIDUALISM