Eve Gaudet

Quine on Meaning

The Indeterminacy of Translation

CONTINUUM STUDIES IN AMERICAN PHILOSOPHY

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To Alexandre

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CONTENTS

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CHAPTER 1

INTRODUCTION

PROBLEM

This book is about W.V. Quine's claim of an asymmetry between underdetermination of theory and indeterminacy of translation. As an introduction to the analysis of this claim, let me try to explain how Quine has been led to discuss both underdetermination of theory and indeterminacy of translation.

In *Epistemology Naturalized*, Quine tells us about the theoretical context of his interest in indeterminacy of translation. He recalls that "[e]pistemology is concerned with the foundations of science",[1] and that it is divided into two kinds of study: the conceptual kind, which treats of meaning, definitions, and clarification of concepts; and the doctrinal kind, which treats of justification of our knowledge of nature. Quine goes on to recall the empiricists' dream of reducing all natural knowledge to sense experience. On the conceptual side of the bifurcation, this project of reduction meant an explanation of the notion of body in sensory terms. On the doctrinal side, it meant a justification of knowledge in sensory terms. As we shall see, it is in connection with the conceptual side of epistemology that Quine's interest in indeterminacy of translation is to be understood, while his interest in undertermination of theory must be discussed in connection with the doctrinal side of epistemology. Before describing these theses and their connections with the two sorts of tasks for epistemology, let's follow Quine in some of his historical considerations in *Epistemology Naturalized*.

Quine recalls the development of an idea that has had an important effect on the development of the conceptual side of epistemology. The idea is contextual definition, or paraphrasis; a crucial step forward proposed by Jeremy Bentham. Contextual definition is a means of explaining a term without identifying an object to which the term refers and without identifying a synonymous word. The explanation of a term is rather achieved by showing how to translate sentences in which the term appears. This acknowledgment of the sentence as the primary vehicle of meaning is advantageous, for empiricists can continue to say that sense impressions are the only reality without having counter-intuitively to equate bodies with sense impressions themselves, as Hume did. Empiricists can continue to talk of bodies in terms of impressions, but without contradicting common sense by claiming that bodies *are* sense impressions; it now suffices to translate *sentences* about bodies into *sentences* about sense impressions. In *Our Knowledge of the External World* Russell used this idea of contextual definition, coupled with the resources of set theory, to propose "an account of the external world as a logical construct of sense data."[2] Carnap in *Der logische Aufbau der Welt* also tried to achieve this reduction, or translation, of sentences about the world into terms of sense data (observation), logic, and set theory.

On the doctrinal side of epistemology this project of reduction could not undo the acknowledgment, already made at the time, of the impossibility of *justifying* natural science by immediate experience. As Quine points out, "the mere fact that a sentence is *couched* in terms of observation, logic, and set theory does not mean that it can be *proved* from observation sentences by logic and set theory."[3] However, despite the fact that the Cartesian appetite for certainty was already deemed impossible to satisfy on the doctrinal side, Carnap continued to work on the reduction project on the conceptual side. The links between evidence and theory would not be certain, but, by being reduced to observation terms and logico-mathematical auxiliaries, at least theories and their evidence would become as clear as those observation terms and logico-mathematical auxiliaries. A question arises, however. Why insist on the project of such a rational reconstruction, a *creation*, of scientific discourse in terms of sense experience and logico-mathematical auxiliaries, if it is acknowledged that the only evidence we have for science is the stimulation of sensory receptors? Why not ask psychology simply to *describe* how scientific

discourse is built upon the stimulation of sensory receptors? The answer is that rational reconstruction enjoys an important advantage over psychology. For the translation of scientific language in terms of sense experience and logico-mathematical auxiliaries is not something that we naturally learn to do, so psychology cannot *describe* how it happens. Thus psychology cannot provide the translational reduction that Carnap was looking for.

So Carnap had a reason to persist in the project of a rational reconstruction and to refuse to settle for psychology: he was looking for a translational reduction. The problem was that his rational reconstruction did not provide the requisite translational reduction either. Thus Carnap had to renounce the project of definition, of elimination, of translational reduction, and thereby renounce the single advantage that rational reconstruction was supposed to have had over psychology.

Now, it is in regard to these results of Carnap's efforts that Quine's thesis of indeterminacy of translation becomes interesting. It is because of indeterminacy of translation that Carnap's project was aborted. Indeterminacy of translation states that we do not have an identity criterion for the majority of our utterances. For the majority of our utterances there is "no fund of experiential implications it can call its own." As Quine describes it in "Epistemology Naturalized", indeterminacy of translation arises from the combination of the thesis that the meaning of a sentence is what counts as evidence for its truth with the view that theoretical sentences have their evidence only as large sets of holistically interrelated sentences, not individually.[4] This combination leads to the abandonment of the project of reducing every sentence to a sentence in observational and logico-mathematical terms, for any translation of a sentence into another will be as correct as any other if the empirical implications of the holistically related set of sentences to which that sentence belongs remain the same. In other words, given a theory or set of holistically related sentences, there is no way to tell which translation of an individual sentence is "the right one". All that matters is that the empirical implications of the whole set of holistically related sentences remain the same. For instance, "[t]here will be no justification for pairing off the component English sentences with component Arunta sentences, except as these correlations make the translation of the theory as a whole come out right."[5] This means that we cannot

identify synonymy relationships between theoretical sentences taken individually, that is to say, we do not have an identity criterion for sentential meanings.

The despair of translating all our knowledge into the language of observation and logico-mathematical auxiliaries has been interpreted among philosophers as the end of traditional epistemology and the beginning of a naturalized epistemology, that is, epistemology as a chapter of empirical science. The dream of a reduction of knowledge to the language of observation and logico-mathematical auxiliaries was the last supposed advantage of rational reconstruction that allowed philosophers to bypass empirical psychology, and now this dream has died.

Thus, we see, Quine's indeterminacy thesis is crucial in the justification of the abandonment of traditional epistemology in favor of a naturalized epistemology, an epistemology conceived as a chapter of empirical psychology.

Quine's indeterminacy of translation thesis has itself been the target of many criticisms. Critics and commentators have wondered about the exact content of the thesis, about the arguments Quine offers to defend it, and about which of the many formulations found in Quine's texts we should take most seriously. Although these questions will be raised and tentatively answered in the present book, the main focus will be on the relation that Quine sees between indeterminacy of translation and underdetermination of theory. More precisely, the central question that will occupy us is this: what are Quine's arguments for the claim of an asymmetry between physics and translation, or, why does Quine say that there is a fact of the matter to physics but none to translation?

Now, what about underdetermination of theory? It is to be situated within the doctrinal side of epistemology for it concerns evidence. The content of the thesis has varied throughout Quine's writings, but we can adopt as definitive this elegant version proposed by Gibson:

> The doctrine of underdetermination of theory claims that theories about the world transcend all possible observations of the world, and, further, that different competing theories can be developed on the same observational basis. In a word, theories can be logically incompatible with one another, yet empirically equivalent.[6]

Thus, many theories can be built on the same observational basis, for there is a slack between theoretical talk and observational talk. As Quine says, "the observational criteria of theoretical terms are commonly . . . flexible and fragmentary."[7] Quine expects wide agreement on this doctrine,[8] and he is right. Underdetermination of theory is almost universally admitted, for, even intuitively, it is easy to acknowledge that there is creation on our part in the building of theories. That is to say, our theories are not mere mirrors of the world. *We* build them, and we are *active* through the building process, whence the possibility of creating different theories on the same observational basis.

Critics and commentators have reacted more strongly to Quine's description of the difference between indeterminacy of translation and underdetermination of theory. Quine claims that, in addition to underdetermination of theory, translation is subject to indeterminacy of translation. The claim of indeterminacy of translation is that the choice between two behaviorally equivalent translation manuals is immaterial; it is a pseudo-choice: there is no reality of sentential meaning, there are no sentential meanings as entities, about which the translations could be right or wrong. As long as the whole sets of holistically related sentences conform to the data, the pairing off of individual theoretical sentences can vary. In other words, insofar as the empirical implications of the whole set of holistically related sentences are preserved, there is no sense to the question which pairing off of *individual* sentences is uniquely the right one. On the other hand, the choice between empirically equivalent physical theories is real, material. For a realist like Quine, there is a world, a reality, to which the theories conform. Thus, the difference that Quine sees between indeterminacy of translation and underdetermination of theory is this: whereas there is no fact of the matter (reality) to the choice between empirically (behaviorally) equivalent translation manuals, there is a fact of the matter (reality) to the choice between empirically equivalent physical theories.

OUTLINE

In Chapters 2 through 6, I propose my interpretation of Quine's position on the issue of the asymmetry. I begin in Chapters 2 and 3 with an analysis of Quine's notion of fact of the matter. I explain

Quine's claim that fact of the matter is a question of ontology, a question of what there is, a question of extralinguistic reality. I also attempt to describe the relation that Quine sees between facts of the matter and truth: facts of the matter determine what is true.

In Chapters 4 through 6, I propose an analysis of indeterminacy of translation and of its difference from underdetermination of theory. Relying on the analysis of the notion of fact of the matter proposed in the first chapters, I attempt to explain why, for Quine, indeterminacy of translation, or the absence of facts of the matter to translation, amounts to an ontological lack: there are no meanings as entities because there is no individuation criterion for meanings. Since facts of the matter determine what is true, when there is no fact of the matter there is no question of what is true. This happens when we face two behaviorally equivalent translation manuals: there is no reality, there are no meanings as entities, enabling us to favor one manual over the other, and thus there is no question of which is the right or the true one. After a close look at Quine's ontological take on his indeterminacy of translation thesis, it becomes easy to see its difference from his underdetermination of theory thesis: the latter is an epistemological claim about the relation between observation and theory, whereas the former is an ontological claim about what there is.

In light of the proposed interpretation of Quine's notion of fact of the matter and of his indeterminacy of translation and underdetermination of theory theses, I undertake a review of the main critics of the claim of an asymmetry between physics and translation. I begin with those who are, I think, further away from Quine's position, and continue with those who I take to be closer to his position. I finish with a discussion of the denial of Quine's asymmetry based on the promises of cognitive science. This denial needs attention, given the confidence placed in cognitive science nowadays.

Chapters 7 and 8 discuss Chomsky's and Rorty's takes on the issue. Both of them make the most naïve of the mistakes analyzed in this book, insofar as they simply fail to situate the debate in the right discipline. Both of them think that Quine's indeterminacy of translation thesis amounts to an epistemological claim that is no different to underdetermination of theory. At first sight, Chomsky's position appears very different from Rorty's since, whereas Rorty thinks there is a fact of the matter to both physics and translation, Chomsky

thinks there is no fact of the matter in either case. However, despite this divergence of opinion, Rorty and Chomsky take a very similar line on the issue. They both agree on its *nature*, which they take to be epistemological.

In Chapter 9, Michael Friedman's position is analyzed. Friedman rightly sees the issue before him to be ontological, but he wrongly thinks that indeterminacy of translation is the claim of an irreducibility of linguistic facts to physical facts or an inexplicability of linguistic facts by physical facts. With indeterminacy of translation Quine addresses neither determinism, nor reductionism, nor explanation. Friedman reads "indeterminacy" as indicating a lack of cause or a lack of causal explanation. However, Quine's use of "indeterminacy" indicates a lack of identity (of individuation) of meanings, not a lack of cause or a lack of causal explanation.

In Chapter 10, Gibson's and Føllesdal's views of the debate are discussed. Gibson has pointed out that Føllesdal, in his article "Indeterminacy of Translation and Under-determination of the Theory of Nature" (1973), was, like Chomsky and Rorty, tempted by an epistemological reading of the issue. I argue that if Føllesdal hesitated in his reading of the issue and mixed up the content of, and the arguments for, indeterminacy of translation in his 1973 article, he later makes a very important contribution to the debate in "Indeterminacy and Mental States" (1990) by showing what really pertains to the issue. There he agrees with Gibson that the issue is ontological. Moreover, he shows that while the *content* of the indeterminacy of translation thesis is ontological *the arguments for it* are epistemological. He also shows that physicalism is not needed in the argumentation for indeterminacy of translation. This is an important step in the discussion that neither Gibson nor Quine had made before. Thus, I conclude, Føllesdal has given the clearest and most tenable account of what the asymmetry between physics and translation amounts to.

In Chapter 11, I discuss and evaluate what I take to be the major contemporary criticism of the asymmestry claim: the denial of it based on the promises of the cognitive science. Leading advocates of this denial claim that since cognitive science is a successful science and since it posits mental states, there seems to be no more reason to refuse to accept those mental entities than to eschew any other theoretical entities, such as numbers or magnetic fields. After presenting this position, I discuss two reasons to be skeptical towards it.

NOTES

1. Quine, W.V., *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969, p. 69.
2. Ibid., p. 74.
3. Ibid.
4. Ibid., pp. 80–1.
5. Ibid., p. 80.
6. Ibid., pp. 11–12.
7. Quine, W.V., "On the Reasons for the Indeterminacy of Translation," *Journal of Philosophy*, 67, 1970, p. 179.
8. "This is a point on which I expect wide agreement, if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary" (ibid.).

CHAPTER 2

FACTUALITY AND PHYSICALISM

Among philosophers there is little agreement on whether Quine's underdetermination of theory thesis and his indeterminacy of translation thesis are on an ontological par. Some believe that the only difference between the two is that one is concerned with physical theories whereas the other is concerned with manuals of translation. Other philosophers, including Quine himself, believe that the two theses are not on an ontological par. This suggests that there is a fact of the matter to physics but not so to translation. Before we can fully understand this claim, we need to clarify what Quine means by "fact of the matter," and in particular what relationship Quine sees between "fact of the matter" and physicalism; but let me begin with some preliminary remarks on "fact of the matter."

First, I want to point out that Quine identifies fact of the matter with factuality:

> Barry startles me by asking how I individuate facts of the matter. Ever since my 1940 review of Russell, or earlier, I have lashed out against reification of facts. In my recent writings I should perhaps have forgone the breezy vernacular of "fact of the matter" and written of factuality.[1]

In order to be maximally charitable, I will take this comment into account throughout my analysis, and will read and use "factuality" and

"fact of the matter" indifferently. The same charity will be applied also to Quine's uses of "facts": where Quine uses facts, I will read "factuality" or "fact of the matter." The point here is that Quine's recognition of an extralinguistic determinant of truth and falsity (i.e. factuality) does not commit him to an ontology of facts. Quine can admit factuality without reifying facts, just as we can admit, for instance, that we can talk on someone's behalf without believing in or committing ourselves to "behalfs." Quine believes in a factual basis without pretending to be able to individuate facts. Objects, on the other hand, can be individuated and, contrary to facts, they are necessary for an account of the world:

> A neater plan is to posit *facts*, as correspondents of true sentences as wholes; but this still is a put-up job. Objects in abundance, concrete and abstract, are indeed needed for an account of the world; but facts contribute nothing beyond their specious support of a correspondence theory.[2]

Thus, Quine appeals to factuality or to objects (ontology) to talk about extralinguistic reality. As we will see in Chapter 3, how factual a language is covaries with how ontologically explicit this language is;[3] indeed, we will see that the factuality of a language increases with its regimentation into an ontologically explicit idiom, i.e., into an object-oriented idiom. Moreover, we will see in the present chapter that, for Quine, the ontology of the *overall conceptual scheme* is physical—primacy of microphysical states—which means that the factuality of a regimented idiom also increases as the idiom comes closer to the idiom of physics. Chemistry, for instance, being closer to physics than is biology, is thus the more factual of the two.

There are three differing contexts in which Quine applies the notion of fact of the matter. He applies it in his discussion of indeterminacy of translation,[4] in his discussion of inscrutability of reference,[5] and in relation to competing theories.[6] More generally, we can say that when Quine says there is no fact of the matter, it is in the context of a comparison between rival options:

> Harman's methodological reflections prompt me to explain what I mean when I say regarding some pair of alternatives that there is no fact of the matter. ... I am simply saying, as a physicalist, that no

> distribution of physical states over space-time would make one of the alternatives true and the other false.[7]
>
> [W]hat excuse could there be for supposing that the one manual conformed to any distribution of elementary physical states better than the other manual? What excuse, in short, for supposing there to be a fact of the matter?[8]

Thus, we must say that there is no fact of the matter in regard to some pair of alternatives when there is no reason to suppose that one of the alternatives conforms better than the other to any distribution of microphysical states, or when there is no reason to suppose that any distribution of physical states renders one of the alternatives true and the other false. If there is no difference between two sets of behaviors, for instance, there is no reason to suppose that the two sets are not physically equivalent; there is no reason to suppose that there is a difference in facts of the matter between the two. In other words, if there is no difference between two sets of behaviors, there is no basis for supposing that the arrangement of microphysical states discriminates between the two.

Now, let's turn to the main question of this chapter: what is the relation Quine sees between "fact of the matter" and physicalism? Quine connects all facts of the matter with distributions of microphysical states. This is what he calls his physicalism. "What now is the claim of physicalism? Simply that there is no difference in matters of fact without a difference in the fulfillment of the physical-state predicates by space-time regions."[9] Thus, "the proper construal of 'fact of the matter' is neither methodological (i.e., epistemological) nor transcendental; it is naturalistic and physicalistic."[10] First, let me insist on the claim that the notion is physicalistic. This means that Quine's notion of fact of the matter is to be understood in relation to his physicalism.

Quine's physicalism is an ontological position. This means that it is a position on what there is, on what extralinguistic reality is. More precisely, physicalism is materialism: "Physicalism ... is materialism, bluntly monistic except for the abstract objects of mathematics."[11] Usually, Quine appeals to physicalism against mind/body dualism, and against mentalistic semantics: "Usually I have cited it [physicalism] just by way of dissociating myself from dualism and mentalistic semantics."[12] The following passage is a good example:

> It is at this point that we must perhaps acquiesce in the psychophysical dualism of predicates, though clinging to our effortless *monism of substance*. It is what Davidson has called *anomalous monism*. Each occurrence of a mental state is still, we insist, an occurrence of a physical state of a body, but the groupings of these occurrences under mentalistic predicates are largely untranslatable into physiological terms. There is token identity, to give it the jargon, but type diversity. . . . The point of anomalous monism is just that our mentalistic predicate imposes on bodily states and events a grouping that cannot be defined in the special vocabulary of physiology. Each of those individual states and events is physiologically describable, we presume, given all pertinent information.[13] (First emphasis added)

These remarks on the contexts in which Quine appeals to physicalism, to his physicalistic notion of fact of the matter, will be of great importance in Chapters 4 through 6 on indeterminacy of translation. For now, I want to stay at the most general level to characterize the notion of fact of the matter, the applications of which will concern me later. Here I want to stress the following idea: for Quine to say that the notion of fact of the matter is physicalistic means that the notion refers to the substance ("monism of substance"), or to the extralinguistic physical reality, and not to the predicates used to talk about it. It concerns the material tokens, not the types. Thus, because of the monism of substance, each time we identify a difference, it means there has been a difference in the arrangement of the microphysical states, even if this difference is not identified in terms of those microphysical states. For instance, even if I say that raising my arm is a behavioral change, I can rest assured that this change depends on a change in microphysical states. The fact that there is or not a fact of the matter regarding some pair of alternatives is not related to the way we identify factual distinctions. It is determined by what there is, by the physical reality. Thus, the irreducibility of nonphysical predicates to physical predicates does not affect the existence or inexistence of facts of the matter.

Let me now dwell on the fact that Quine also says that the notion of fact of the matter is ontological:

> I have argued that two conflicting manuals of translation can both do justice to all dispositions to behavior, and that, in such a case,

> there is no fact of the matter of which manual is right. The intended notion of matter of fact is not transcendental or yet epistemological, not even a question of evidence; *it is ontological*, a question of reality, and to be taken naturalistically within our scientific theory of the world.[14] (Emphasis added)

What does it mean to say that the notion of fact of the matter is ontological? Does it commit us to a particular ontology? No. The ontology can change:

> I advanced an explicit standard whereby to decide what the ontological commitments of a theory are. But the question what ontology actually to adopt still stands open, and the obvious counsel is tolerance and an experimental spirit.[15]
>
> We can switch our own ontology too without doing violence to any evidence, but in so doing we switch from our elementary particles to some manner of proxies and thus reinterpret our standard of what counts as a fact of the matter. Factuality, like gravitation and electric charge, is internal to our theory of nature.[16]

The ontology can change, and acknowledging this does not involve an abandonment of physicalism, for it remains that facts of the matter always depend on the microphysical states, whatever they are, identified by the physics of the moment.[17] Physics could change its ontology radically while remaining the same enterprise of trying to say "what minimum catalogue of states would be sufficient to justify us in saying that there is no change without a change in positions or states."[18]

> If conclusive evidence of telepathy or even clairvoyance were forthcoming, I envision a scurry to the cyclotron, computers, and drawing boards to invent a new and more adequate theory, which would still be called physics. Even today the elementary particles are particles at all only thanks to a succession of ever more strained analogies. Physics can even be pursued as field theory, free of bodies, and no complaints. The name "physics" would survive the clairvoyance revolution too. Continuity of the enterprise is what matters.[19]

Physicalism is not the commitment to a certain kind of object, to a

given ontology, but rather the commitment to the variable ontology *of a certain enterprise*: that of trying "to find a minimum catalogue of states—elementary states, let us call them—such that there is no change without a change in respect to them."[20] To make that clear, Quine offers a reformulation of his physicalism. Instead of formulating it by reference to physical objects, he formulates it by reference to the physical vocabulary:

> Perhaps then our primary concern belongs with the truth of sentences and with their truth conditions, rather than with the reference of terms. If we adopt this attitude, questions of reference and ontology become incidental. Ontological stipulations can play a role in the truth conditions of theoretical sentences, but a role that could be played as well by any number of alternative ontological stipulations. The indecisiveness of ordinary language toward questions of reference is the more readily excused.
>
> What now of physicalism? To profess materialism, after all this, would seem grotesquely inappropriate; but physicalism, reasonably *reformulated*, retains its vigor and validity. Our last previous formulation came to this: there is no difference in the world without a difference in the number or arrangement or trajectories of atoms. But if we make the drastic ontological move last contemplated, all physical objects go by the board—atoms, particles, all—leaving only pure sets. The principle of physicalism must thereupon be formulated by reference not to physical objects but to physical vocabulary.[21] (Emphasis added)

This reformulation of physicalism in terms of vocabulary is not a change of position. It does not mean that now one has to use the physical vocabulary in order to be able to identify a difference in facts of the matter. It is just a way for Quine to say that the ontology of physics can change, but that, still, it is always the ontology of physics, the microphysical states identified by the physics of the moment, that will constitute the ultimate locus of facts of the matter.

So it is not the choice of a particular ontology that is determinant for facts of the matter, but the fact that the ontology of reference is that of the physics of the moment. That is to say, that the notion of fact of the matter is "taken naturalistically within our scientific theory of the world," which is physics.[22]

Why such a preference for the ontologies of physics? Why does physics have this special status? Ontologies of physics have this status because they are the ontologies of the over-all conceptual scheme that best accommodates science in the broadest sense.[23] A physicalistic worldview is the best choice of over-all conceptual scheme in regard to scientific standards like simplicity, generality, precision, explanatory power, etc., which also guide local choices of theories. Thus the reasons for Quine's preference for the (changing) ontology of physics are not different in kind from the reasons that lead any scientist to adopt a particular scientific theory (ontology). In other words, Quine's commitment to physicalism, or anomalous monism, or monism of substance, is not a metaphysical commitment. Quine bases his commitment to physicalism on the observation of what is working for the practice of science.

> Our tentative ontology for science, our tentative range of values for the variables of quantification, comes therefore to this: physical objects, classes of them, classes in turn of the elements of this combined domain, and so on up.
>
> We have reached the present stage in our characterization of the scientific framework not by reasoning a priori from the nature of science qua science, but rather by seizing upon traits of the science of our day. Special traits thus exploited include the notion of physical object, the four-dimensional concept of space-time, the classical mold of modern classical mathematics, the true-false orientation of standard logic, and indeed extensionality itself. One or another of these traits might well change as science advances. Already the notion of a physical object, as an intrinsically determinate portion of the space-time continuum, squares dubiously with modern developments in quantum mechanics. Savants there are who even suggest that the findings of quantum mechanics might best be accommodated by a revision of the true-false dichotomy itself.[24]

Thus physicalism is fallible, but it is unlikely that we would be led to abandon it. Quine says: "I see nothing sacrosanct about naturalism or physicalism; both are fallible, unless saved by vague edges."[25] And he allows for those vague edges:

> In general I tend to be impatient with the quest for precision in the names for disciplines and schools of thought: in asking what really counts as naturalism, epistemology, physics. Like our everyday terms, these are at best helpful makeshifts, vague around the edges, and no matter.[26]

The fact that Quine's ontological commitments are made immanently, and not transcendently, means that when Quine talks about what there is, he in fact talks about what we now say there is, about the ontological commitments of our theories of the moment. Factuality is extralinguistic, but, still, it is something that we posit within our theory of the moment. If Quine attributes a basic ontological role to microphysical states, if he commits himself to the microphysical states of the physics of the moment, it is because he already accepts, as his worldview, the theory of physics that posits the existence of those microphysical states.

Thus, to repeat, the (changing) ontology of physics is adopted as basic because physics as an over-all conceptual scheme best accommodates science in the broad sense and because it lives up to the standards of science. But there seems to be another reason for adopting a physicalistic worldview. It is the fact that physics is the last tribunal of causal explanation:

> The terms that play a leading role in a good conceptual apparatus are terms that promise to play a leading role in causal explanation; and causal explanation is polarized. Causal explanations of psychology are to be sought in physiology, of physiology in biology, of biology in chemistry, and of chemistry in physics—in the elementary physical states.[27]

If we were then to ask for the causal explanation of a phenomenon described by physics, there is nowhere else to look. Where should we look? Quine does not want to fall into metaphysics in order to answer that question, so he stops the causal explanation chain there, and concludes that physics is *ultimately* factual: "Physics is factual in my view simply for want of a higher tribunal. As Gibson urges, naturalism is the key."[28] Thus, physics is not a scientific enterprise like any other: as the last tribunal, it provides the most general explanation of the world. It provides descriptions of change in terms of that

"minimum catalogue of states—elementary states, let us call them—such that there is no change without a change in respect to them."[29] Instead of looking for explanations outside those of the last scientific tribunal and thus falling into a first philosophy, Quine adopts physics as the widest frame of reference of causal explanation available to us: "There is no legitimate first philosophy, higher and firmer than physics, to which to appeal over physicists' heads."[30]

Besides the fact that the science of our day indicates physicalism as the most accommodating option for a scientific worldview, and besides the fact that physics is the last resort in the quest for causes, there is another good reason that justifies Quine's choice of physicalism as a worldview, that is, his choice of seeing the ontology of physics as basic, namely, the extensionality of physics.[31] The fact that physics is extensional means that it can be regimented into the canonical notation, into the language of truth-functional connectives and quantification,[32] a language in which the use of the existential quantifier is the most explicit. This is an advantage for a scientific worldview that is to replace a first philosophy because it provides physics with a better answer than other scientific disciplines to the question "what exists?" Extensionality is an advantage, indeed, since the logic of the existential quantifier is, for Quine, the logic of existence. The logic of the existential quantifier is the notation that makes our ontological commitments explicit. It is through the use of this notation that we make clear what we think there is. Thus, physics is a good choice of scientific frame of reference in replacement of a first philosophy. It is a good choice of overall scientific conceptual scheme because it is the scientific theory that is the clearest and the most explicit about what there is. Moreover, as we have seen, physics is not only the clearest and the most explicit but also the most general scientific position on what there is: it provides the minimal catalogue of states involved in all changes.[33]

I want to insist on the fact that, even if Quine sometimes argues for physicalism by appealing to the fact that causal explanation is polarized in physics, physicalism itself is not a position on what constitutes a good or the best causal explanation. Physicalism is a position on what there is.[34] Although we can have epistemological reasons to adopt physicalism, physicalism itself is not an epistemological view. It is not a view on explanation.

To further pursue the case against an epistemological reading of physicalism, let me mention that some people see a reductionism in Quine's physicalism.[35] Quine repeatedly says that he does not defend a reductionism:

> Harman's methodological reflections prompt me to explain what I mean when I say regarding some pair of alternatives that there is no fact of the matter. I am not protesting, as a positivist might, that the choice would not be reflected in future experience. Nor am I protesting, as some intemperate sort of physical reductionist might, that the alternatives have not been stated in the vocabulary of physics. I am simply saying, as a physicalist, that no distribution of physical states over space-time would make one of the alternatives true and the other false. I can have reason to believe, with regard to some matter, that there is in this sense no fact of it, without dreaming of anyone's paraphrasing the matter into terms of microphysical states.[36]

There is, then, no reductionism involved in Quine's physicalism. Physicalism does not say that we have to *identify* a difference in physical microstates in order to *identify* a difference in matters of fact. It says that there has to *be* a difference in microphysical states if there is to *be* a difference in matters of fact. Thus, physicalism tells us that, if we identify a difference in matters of fact, it is because there has been a difference in microphysical states, even though we cannot say *what* difference. Physicalism tells us that when there is a difference in facts of the matter, when something happens, it means that some change in the arrangement of microphysical states has occurred. Physicalism does not require that we be able to identify the change of microphysical states in question. Once again, physicalism is not an epistemological position but a position on what there is, or, more precisely, on which discipline tells us what there is.

Another way to put this is to say that Quine's formulation of physicalism is acknowledged as incomplete, but that this is not a problem.

> This formulation, "fulfillment of physical-state predicates by space-time regions," is decidedly unfinished. . . . The physical-state predicates are the predicates of some specific lexicon, which I have only begun to imagine, and which physicists themselves are not ready to

> enumerate with conviction. Thus I have no choice but to leave my formulation of physicalism incomplete.[37]

In other words, one cannot say that the physical lexicon is necessary for making factual distinctions. How is this conceivable, given that "there is no difference in matters of fact without a difference in the fulfillment of the physical-state predicates by space-time regions"? The answer is that while it is true that we cannot identify *directly* the factual distinctions without using the physical lexicon, we can, however, talk in a way that is *directed* to factual distinctions without recourse to that lexicon:

> Now factuality is similar [similar to questions of reference]. Ordinary language is only loosely factual,[38] and needs to be variously regimented when our purpose is scientific understanding. The regimentation is again not a matter of eliciting a latent content. It again is a free creation. We withdraw to a language which, though not limited to the assigning of elementary physical states to regions, is visibly *directed* to factual distinctions—distinctions that are unquestionably *underlain* by differences, however inscrutable, in elementary physical states. This demand is apt to be met by stressing the behavioral and the physiological.
>
> Within these limits there is still much scope, of course, for *better and worse*.[39] (Emphasis added)

What does it mean to say that we withdraw to a language that is only *directed to* factual distinctions, a language that is only indirectly factual? Perhaps it means that in such a language, factuality is not identified in the terms of physics. Physics allows *direct* distinctions or identification of factuality because the terms used to identify differences are the terms which specify what there is, that is, physical terms. As soon as we depart from the vocabulary of physics, we can only talk indirectly of factuality or in a way that is only "directed to factual distinctions" because we do not talk in terms of what there is, because we do not talk in terms of microphysical states. There is a place for better and worse in how directly the identification of factuality is made, says Quine. Factuality can be more or less directly identified in a language, according to how promising for causal explanation the terms of that language are. The more the terms of a

language promise to play a leading role in causal explanation, the more this language is factual, or the more directly factuality is identified, for, Quine says, causal explanation is polarized in physics. In other words, factuality is a matter of degree.[40] The closer a language is to physical explanation (as chemistry is closer to physics than is biology), the more directly factuality is identified in that language, i.e., the more factual that language is.

Thus, even if we cannot reduce our talk of mental states or of behaviors, for instance, to the terms of neurology, or, ultimately, to the language of physics, we have to believe that there is a physical difference underlying the differences in the mental or in the behavioral: "When we talk of mental states or events subject to behavioral criteria, we can rest assured that we are not just bandying words; there is a physical fact of the matter, a fact ultimately of elementary physical states."[41] *Any* fact of the matter is fixed one way or another by the arrangement of microphysical states. The behavioral identifies objectively, though indirectly, the existence of facts of the matter, even if the reduction of behavioral talk to physical talk is not achieved.

Let me point out here something that will prove to be of great importance in the chapters on indeterminacy of translation: for Quine, the possibility of identifying factuality objectively in terms other than the physical is crucial for semantics since semantics is stuck at the behavioral level. In other words, meaning is public. Semantics, beyond the identification of factuality in terms of behavior, is not factual. Progress in neurology would not benefit semantics. Thus, even though "the terms that play a leading role in a good conceptual apparatus are terms that promise to play a leading role in causal explanation," and even though "[c]ausal explanations of psychology are to be sought in physiology, of physiology in biology, of biology in chemistry, and of chemistry in physics—in the elementary physical states,"[42] it remains the case that Quine does not want to defend a causal approach to meaning:

> He [Nozick] ponders a causal approach to meaning. The suggestion seems to be that the meaning of a term is the thing or mechanism that causes the stimulatory data that lead us to apply the term. I have three problems here: how much causal background should we include? how does the suggestion work for terms for whose appli-

> cation there are no separable data? and when there are such separable data, why not just take them as the meaning instead of the causes? Evidently I have not grasped the idea. . . .
>
> [E]ven a full understanding of neurology would in no way resolve the indeterminacy of translation.[43]

Thus, even if differences in verbal behavior are directed to, or underlain by, differences in microphysical states, even if differences in verbal behavior objectively represent differences in microphysical states, semantics has to do only with the behavioral.

Up to now we have seen that fact of the matter (or factuality) is for Quine a question of what there is, a question of ontology, which means that, when Quine talks of factuality or facts of the matter, he talks about extralinguistic reality. Since ontology is determined immanently to our current operative theory of the world, factuality is also determined immanently to that theory. Since factuality is determined immanently to our current operative theory of the world, factuality is ultimately physicalistic, for the current operative theory of the world, or overall conceptual scheme, is physics. Physics is the last tribunal. We have seen that Quine has several reasons to say that the current operative theory of the world is physics. We have also seen that, although we cannot reduce all our idioms to the idiom of physics, all changes in factuality depend on changes in microphysical states, and we can identify factuality objectively, though indirectly, in non-physical terms.

NOTES

1. Barrett, R. and Gibson, R., *Perspectives on Quine*. Oxford: Basil Blackwell, 1990, p. 334.
2. Quine, W.V., *Pursuit of Truth*. Cambridge, MA: Harvard University Press, 1990, p. 80.
3. How factual a language is depends on how strictly ascriptions of factuality can be made in that language.
4. "Next let us turn to my notion of a fact of the matter in what was for me its primary application, namely, to translation. ... My thesis of the indeterminacy of translation is that mutually incompatible manuals of translation can conform to all the same distributions of speech dispositions. But the only facts of nature that bear on the correctness of translation are speech dispositions. Thus mutually incompatible man-

uals of translation can conform to all the same overall states of nature, hence all the same distributions of microphysical states. Yet, being incompatible, both manuals can scarcely be right. Which one is, if either? I say there is no fact of the matter" (Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W. V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 429). Cf. "[T]he conformity of a translation manual to speech dispositions is decidedly a matter of fact. It is only the choice between certain rival manuals that lacks factuality" (ibid., pp. 459–60).

5. "[T]he inscrutability of reference is not the inscrutability of a fact; there is no fact of the matter. But if there is really no fact of the matter, then the inscrutability of reference can be brought even closer to home than the neighbor's case; we can apply it to ourselves" (Quine, W.V., *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969, p. 47). Cf. "As for reference, what my argument from proxy functions shows is that there is no fact of the matter except relative to a target language and a manual of translation" (Hahn and Schilpp, *The Philosophy of W. V. Quine*, p. 429).
6. "Factuality, or matterhood of fact, is likewise immanent. We do not adjudicate between our aggregate system of the world and a rival system by appeal to a transcendent standard of truth or factuality" (ibid., p. 367).
7. Ibid., p. 187.
8. Shahan, R.W. and Swoyer, C. (eds), *Essays on the Philosophy of W. V. Quine*. Norman: University of Oklahoma Press, 1979, p. 167.
9. Ibid., p. 166. In the same spirit Quine also writes: "[t]his illustrates my identification of facts of the matter with distribution of microphysical states' (Hahn and Schilpp, *The Philosophy of W. V. Quine*, p. 429), or "[w]here positions and states of bodies do not matter, there is no fact of the matter" (Shahan and Swoyer, *Essays on the Philosophy of W. V. Quine*, p. 162).
10. Ibid., p. 146.
11. Quine, W.V., *From Stimulus to Science*. Cambridge, MA: Harvard University Press, 1998, p. 15.
12. Barrett and Gibson, *Perspectives on Quine*, p. 334.
13. Quine, *From Stimulus to Science*, pp. 87–8.
14. Quine, W.V., *Theories and Things*. Cambridge, MA: The Belknap Press of Harvard University Press, 1981, p. 23.
15. Quine, W.V., *From a Logical Point of View: 9 Logico-Philosophical Essays* (revised edn). Cambridge, MA: Harvard University Press, 1980, p. 19.
16. Quine, *Theories and Things*, p. 23. By "factuality" here Quine does not mean the external world. That, of course, is not internal to our theory. He means the *nature* of the external world, how things stand according to our theory of nature.

17. "Incidentally, one of Barry's complaints about facts of the matter is traceable to my not making it clear that I meant the microphysical states in question to be the few basic ones recognized by today's physicists" (Barrett and Gibson, *Perspectives on Quine*, p. 334).
18. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 164.
19. Orenstein, A. and Kotatko, P. (eds), *Knowledge, Language, and Logic: Questions for Quine*. The Netherlands: Kluwer Academic Publishers, 2000, p. 411.
20. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 166.
21. Ibid., p. 165.
22. As Gibson writes: "[t]he current theory of the world is physicalistic—and with good reason, Quine thinks. And this physicalistic world-view settles, for the present, the physical facts of the matter" (Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 152).
23. "Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. *Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense*; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from considerations which determine a reasonable construction of the whole" (Quine, *From a Logical Point of View*, pp. 16–17; emphasis added).
24. Quine, W.V., *The Ways of Paradox and Other Essays*. New York: Random House, 1966, pp. 231–2.
25. Orenstein and Kotatko, *Knowledge, Language, and Logic*, p. 411.
26. Ibid.
27. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, pp. 168–9.
28. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 155.
29. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 166. "Finally I should like to clarify what Putnam and others have called my scientism. I admit to naturalism, and even glory in it. This means banishing the dream of a first philosophy and pursuing philosophy rather as part of one's system of the world, continuous with the rest of science. And why, of all natural sciences, do I keep stressing physics? Simply because it is the business of theoretical physics, and of no other branch of science, to say what . . . minimum catalogue of states would be sufficient to justify us in saying that there is no change without a change in positions or states" (Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 430).

30. Davidson, D. and Hintikka, J. (eds), *Words and Objections: Essays on the Work of W.V. Quine*. Boston: D. Reidel Publishing Company, 1969, p. 303.
31. "A context is *extensional* if its truth value cannot be changed by supplanting a component sentence by another of the same truth value, nor by supplanting a component predicate by another with all the same denotata, nor by supplanting a singular term by another with the same designatum. Succinctly, the three requirements are substitutivity of *covalence*, of *coextensiveness*, and of *identity*, *salva veritate*. A context is *intensional* if it is not extensional" (Quine, *From Stimulus to Science*, p. 90).
32. "Extensionality is much of the glory of predicate logic, and it is much of the glory of any science that can be grammatically embedded in predicate logic. I find extensionality necessary, indeed, though not sufficient, for my full understanding of a theory. In particular it is an affront to common sense to see a true sentence go false when a singular term in it is supplanted by another that names the same thing. What is true of a thing is true of it, surely, under any name" (ibid., pp. 90–1).
33. "Why, Goodman asks, this special deference to physical theory? This is a good question, and part of its merit is that it admits of a good answer. The answer is not that everything worth saying can be translated into the technical vocabulary of physics; not even that all good science can be translated into that vocabulary. The answer is rather this: nothing happens in the world, not the flutter of an eyelid, not the flicker of a thought, without some redistribution of microphysical states. It is usually hopeless and pointless to determine just what microphysical states lapsed and what ones supervened in the event, but some reshuffling at that level there had to be; physics can settle for no less. If the physicist suspected there was any event that did not consist in a redistribution of elementary states allowed for by his physical theory, he would seek a way of supplementing his theory. Full coverage in this sense is the very business of physics, and only of physics" (Quine, *Theories and Things*, p. 98).
34. Or, more precisely, a position on which discipline tells us what there is.
35. Stroud, for instance, defends the following view: "Assuming a physical vocabulary fixed somehow, the metaphysical payoff of physicalism for Quine is largely to be found in its bearing on the nature of the mental. If the world is through and through physical, then there is no distinctively mental realm. It was easy enough to see how this would go when it was a question of objects, of ontology: there are only physical objects in the world, and no minds or mental objects. But now even physical objects are out of the way, and the thesis must be formulated in terms of predicates or lexicon alone. It can scarcely be the thesis that the physical predicates are the only predicates we have. It is rather the

idea that the physical lexicon is 'fundamental' in some way, or more 'fundamental' than others. The physical sentences must somehow be the only sentences that are determinately true, or the only ones that are 'fundamentally' true, so that strictly speaking there are only physical facts. The physical lexicon must somehow be sufficient to express everything that is the case" (Barrett and Gibson, *Perspectives on Quine*, p. 327).

36. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 187. Another example: "What now of my physicalism? Simply that there is no difference in matters of fact without a difference in the fulfillment of the physical-state predicates by space-time regions. Again this is not reductionism in any strong sense. There is no presumption that anyone be in a position to come up with the appropriate state predicates for the pertinent regions in any particular case" (Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 166).
37. Ibid.
38. What should we understand by "loosely factual"? Perhaps Quine says that ordinary language is loosely factual because in ordinary language we talk in a way that is only analogous to real talk about objects (identified). In ordinary language, we do posit bodies, but we also develop ways of talking that are only parallel to the ways we talk of bodies. We find comfort in this analog or parallel way of talking without asking the question whether we have an identity criterion for that of which we are talking as though we were sure to be dealing with objects: "Bodies are assumed, yes; they are the things, first and foremost. Beyond them there is a succession of dwindling analogies. Various expressions come to be used in ways more or less parallel to the use of the terms for bodies, and it is felt that corresponding objects are more or less posited, *pari passu*; but there is no purpose in trying to mark an ontological limit to the dwindling parallelism" (ibid., p. 160). I come back in the next chapter to the idea that ordinary language is only loosely factual.
39. Ibid., pp. 168–9.
40. "My suggested standard for facts of the matter is directed rather at concrete situations, and pales progressively as we move upward and outward. Evidently then the upshot is that the factual and the mathematical stand apart, for me as for Carnap; but for me, unlike Carnap, the separation is a matter not of principle but of degree" (Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 430).
41. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quin*, p. 167.
42. Ibid., pp. 168–9.
43. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 365.

CHAPTER 3

FACT OF THE MATTER AND TRUTH

Another important question to ask about the notion of fact of the matter concerns its relation with the notion of truth. How does Quine conceive the relation between truth and factuality? It seems that the first thing to say is that the relation is not one of identity. One important indication that Quine distinguishes truth and factuality is his claim that factuality is relative to our *theory* of nature and that truth is relative to a *language*:

> *Factuality*, like gravitation and electric charge, is internal to our theory of nature.[1] (Emphasis added)
>
> When he [Gibson] has me relativizing *truth* "to a theory (or language)", however, I grant language but balk at theory. A theory that I hold true may turn out false; such is usage, and I accept it. Insofar, truth indeed goes transcendental; but I acquiesce in this as a linguistic effect.[2] (Emphasis added)

Before going on with the comparison of factuality and truth, let me note that Quine has also said that truth, just like factuality, is immanent to a theory: "But truth, as Tarski appreciated, is meaningfully predicated, rightly or wrongly, only within the theory that is operative at the time. Similarly for factuality."[3] Is truth then immanent both to a language and to a theory? Are we facing an important interpretation problem here? No. There is no problem insofar as we dis-

tinguish two questions about truth: 1) saying what it is for a sentence to be true; 2) saying that a sentence is true. Saying what the truth conditions of the sentences of a language L are is different from saying which of those sentences are true.

When Quine says that truth is relative to a language, not to a theory, we must recognize that he is talking about the first question of truth, that is, the question of saying what it is for the sentences of a language L to be true. In other words, saying that truth is relative to a language means that knowledge of a language L provides us with knowledge of the truth conditions of the sentences of L. This is to be understood in the context of Tarski's account of truth; truth is relative to a language in a Tarskian sense: the truth conditions of a sentence are stated in the terms of that sentence. Thus, Quine, following Tarski, would say that "Snow is white" is true if and only if snow is white,[4] and this goes for every other sentence of L. So, when Quine says that truth is relative to a language, it seems that he must mean that the question "what is it to be true?" or "what is truth?" gets no general—translinguistic—answer. Rather, the question is "what is it for each sentence of L to be true?" or "what is truth for each sentence of L?," and the answer is given for each sentence of L. What it is for a sentence S1 to be true is stated in the terms of S1, what it is for a sentence S2 to be true is stated in the terms of S2, etc. Thus we get no general account of truth, of what it is to be true; the knowledge of a language L is necessary to the knowledge of what it is to be true for the sentences of L since the truth conditions are given for each sentence of L, and they are stated in the terms of each of those sentences, respectively.

Now, while knowing a language L gives us access to the truth conditions of the sentences of L, it does not tell us which sentences are true, and which sentences are false. It is rather the business of our theories to tell us what is true, or which sets of (holistically related) sentences are true. In other words, languages tell us what may be said in them; but whether truly or falsely is another question, since for every sentence that a language contains it always contains the denial of that sentence too. On the other hand, when a consistent theory contains a sentence, it never contains its negation too, for accepting a given theory is accepting an assignment of truth values to each of its sentences. Thus, when Quine says that truth is relative to a theory, it seems that he must mean that *what is true* is relative to a theory, that

which sentences are true is relative to a theory: accepting a given theory is accepting a systematic assignment of truth values to a set of related sentences.

Thus, there is no problem in Quine's saying both that truth is relative to a language and that truth is relative to a theory: in the first case "truth" refers to the question of what it is for the sentences of a given language to be true, and in the second case "truth" refers to the question of what is true (which sentences are true).

Thus, when Quine says: "truth, as Tarski appreciated, is meaningfully predicated, rightly or wrongly, only within the theory that is operative at the time. Similarly for factuality,"[5] we must understand, it seems, that what is true and what reality is (factuality) are both relative to a theory. That is to say that what is true and what reality is are ascribed within the theory that is operative at the time: "Disavowing as I do a first philosophy outside science, I can attribute reality and truth only within the terms and standards of the scientific system of the world that I now accept; only immanently."[6]

Even though both are made within the theory that is operative at the time, ascriptions of reality (what reality is) and ascriptions of truth (what is true) seem to be two different things, for Quine distinguishes the two (see the previous quotation). But what is the difference between the two sorts of ascriptions, and how are they related? *At first sight*, the difference between ascriptions of truth and ascriptions of factuality seems to be the following: On the one hand, when we ascribe truth, we are concerned with our sentences, with the linguistic reality; after all, sentences are what are true or false. On the other hand, when we ascribe factuality, we are rather concerned with the world, with the extralinguistic reality. Such is, at first sight, the difference between ascriptions of truth and ascriptions of factuality. However, Quine refuses to see that big a difference between those two kinds of ascriptions. In order to explain why, let me recall some passages from *Philosophy of Logic*. Here, Quine makes it clear that he does not want truth (what is true) to be a matter of language.[7] Truth is a matter of reality, which means that if a statement is true, it is because reality renders it true: No sentence is true but reality makes it so. The sentence "Snow is white" is true, as Tarski has taught us, if and only if real snow is really white. The same can be said of the sentence "Der Schnee ist weiss"; language is not the point.[8] In other words, the truth of a statement is not a matter of language; a true statement is

not true in virtue of the language. Talking about the truth of a statement is only a detour or "indirection", says Quine. We should just assert the sentence, for, in doing so, it would not seem that we are talking about language anymore; it would become clear that we are in fact talking about reality (extralinguistic), even when we *mention* statements: "We do better simply to say the sentence and so speak not about language but about the world."[9]

For Quine, truth (what is true) depends on reality, but this is not a reason to deny that sentences are what convey truth. The truth predicate is useful precisely on those occasions on which we are led, because of technical complexities, to mention sentences (semantic ascent) despite the fact that we have reality in sight. The truth predicate recalls that reality continues to be the essential concern. In Quine's words:

> Truth hinges on reality; but to object, on this score, to calling sentences true, is a confusion. Where the truth predicate has its utility is in just those places where, though still concerned with reality, we are impelled by certain technical complications to mention sentences. Here the truth predicate serves, as it were, to point through the sentence to the reality; it serves as a reminder that though sentences are mentioned, reality is still the whole point.[10]

In other words, and still paraphrasing Quine, we can say that the ascent to a mention of language is only a temporary retreat out of the world, for the truth predicate's function is precisely to cancel the reference to language.[11] The truth predicate reminds us that our interest is in the extralinguistic reality, even though we make a technical ascent in mentioning sentences.[12] The typical Tarskian example—"Snow is white" is true if and only if snow is white—renders explicit this cancellatory function of the truth predicate. In other words, the truth predicate is a means for cancelling the quotation marks, which make, Quine says, "all the difference between talking about words and talking about snow."[13]

We have acknowledged that what are true or false are the sentences, but some precisions are in order. Strictly speaking, what are true or false are not the sentences themselves[14] but their utterances, for the utterance of a sentence can be true on one occasion and false on another.[15] So, what makes a concrete occurrence of a sentence true

or false? Quine says that reality is what renders our sentences true or false. Thus reality (factuality) determines truth (what is true); but what more can we say? *How* does reality render our sentences true? *How* does reality determine truth (what is true)?

To treat this question of how factuality determines truth, I will first explore Alex Orenstein's suggestion that Quine establishes a relationship of "correspondence" between truth and ontology:

> The semantic—correspondence inspired—theory of truth provides a perspective for viewing Quine's work, in particular the close interdependence of questions of truth and questions of ontology. To accept a correspondence theory is to be involved in problems of ontology. For, according to it, the truth of a sentence reflects the way the world is and truth claims are ontological claims. . . . The strength of Quine's position on the nature of ontological commitment lies in its connection with this eminently defensible realist theory of truth. Sentences are true because of the way in which they reflect reality and the quantificational sentences are simply the ones which most explicitly reflect what there is. Whether Quine describes quantification in terms of naming, predicating, or Tarskian satisfaction does not matter, in a sense; all of these provide arguments for the existential significance of quantification and do so as part of a modern vision of the correspondence style account of truth.[16]

In other words, Quine's Tarskian theory of truth amounts to a correspondence theory of truth. It links our words with the world, with what there is, i.e., with factuality: "As already hinted by the correspondence theory, the truth predicate is an intermediary between words and the world. What is true is the sentence, but its truth consists in the world's being as the sentence says."[17] A true sentence corresponds to factuality,[18] to what the world is. What the world is will be most explicit in languages regimented into the canonical notation because it is in that notation that our existential quantifier, which indicates our ontological commitments, is explicit. In the language of truth-functional connectives and quantification, our existential commitments are explicit.[19]

A precision seems in order here, so I will make a small digression. Quine is clear on the fact that, in order to have an ontology, we need to use the regimented language of truth functional connectives and

quantification, for, in order to have an ontology, we need to have variables: "to be is to be the value of a variable." That is not to say that in ordinary language, where we do not have an ontology, we cannot make ontological claims. If, for example, Tom says "I believe in angels," the belief he expresses is an ontological one. He makes an ontological claim, as opposed to, for instance, an epistemological one. Tom makes an ontological claim although we cannot impute an ontology to him until he regiments his language into the canonical notation:

> The ontological question for such a language [idioms of propositional attitudes], as for ordinary language generally, makes sense only relative to agreed translations into ontologically regimented notation. A language is not necessarily defective in being thus ontologically indecisive; it is just not a language of the object-oriented type.[20]

Thus it seems appropriate to say that ordinary language is only loosely ontological,[21] for, in ordinary language, we can make ontological claims without having an ontology. Thus Quine can say, and he does, that ordinary language is factual, but only loosely: ordinary language is only loosely factual, just as it is only loosely ontological.[22] In order to be strictly factual, in order to give access to factuality strictly speaking, a language has to be regimented into the canonical notation and thus render the speaker's ontology explicit.

How does truth, which is determined by factuality, fit into this picture? Should we say that truth is ascribed only loosely in languages that are only loosely factual? Yes. We can say that the truth predicate increases in clarity as we regiment the language in which it is used. Quine indeed says that the truth predicate can vary in clarity: it is as clear as the sentence to which it applies:

> It [the disquotational account of truth] tells us what it is for any sentence to be true, and it tells us this in terms just as clear to us as the sentence in question itself. We understand what it is for the sentence "Snow is white" to be true as clearly as we understand what it is for snow to be white.[23]

As that quotation expresses, the difference of clarity of the truth predicate, between let's say, its use in ordinary language and its use in

the canonical notation, is the difference between the clarity of those two forms of languages. What it means for a given sentence to be said to be true is as clear as the sentence to which the truth predicate is applied. If we agree with Quine that the sentences of the canonical notation are clearer than the sentences of ordinary language, we can say that what it is for those sentences to be true is clearer than what it is for the sentences of ordinary language to be true. As a language gains in clarity, what it is for its sentences to be true also gains in clarity.

To bring my digression to an end, and return to the question of how factuality determines truth, let me mention that, even if truth can be more or less loosely or strictly ascribed according to how loosely or strictly factual the language in which the ascription is made is, it remains the case that the truth predicate links our words to the world. Whatever the language we are using, it remains the case that the relationship between factuality (what there is) and truth (what is true) is one of correspondence. True sentences "correspond" to what there is.

But of course, "correspondence" must be clarified. Let me start with a remark from Orenstein:

> According to this theory [the correspondence theory of truth], a sentence is true when it corresponds, or is adequate, to reality.
>
> Tarski conceived of this very correspondence concept of truth as a constraint (he called it a material adequacy condition) on his definition; moreover, he succeeded in formulating the intuition behind the traditional conception in a far clearer and less problematic manner than had hitherto been achieved. . . .
>
> In the traditional statement we would have said something like "Snow is white" is true if and only if "Snow is white" corresponds to reality. But it is precisely this traditional version which has been subject to serious criticism. After all, what does one mean by "correspondence" or by "reality"? Tarski's treatment, which Quine warmly espouses, avoids these criticisms by avoiding any but transparent notions, namely, some device for referring to the sentence itself and the "if and only if" locution of our canonical notation.[24]

Thus the term "correspondence" appears unproblematic since it states a strict bi-conditional relationship with the "if and only if," and since

it does not require the establishment of a relationship with the world other than the legitimacy of the affirmation of the very same sentence to which the truth predicate is applied. In other words, since the correspondence between the sentence and the world is described in the exact terms of the sentence, the relation of correspondence is not mysterious: a sentence corresponds to reality if and only if that very sentence, when affirmed, makes a legitimate claim. Thus the real question of truth is not about the correspondence between a true sentence and reality, which is established by definition, but about the legitimacy of our claims: "Evidently one who puzzles over the adjective 'true' should puzzle rather over the sentences to which he ascribes it. 'True' is transparent."[25] In other words, we now know that a true sentence is true because reality renders it true, or, to put it differently, because it corresponds to the world; but we still do not know which sentences reality renders true. We know what it takes for our sentences to be true: they must fulfill the strict condition stated by the "if and only if"; but we do not yet know how to tell whether this condition is satisfied in given cases. We do not yet know how to tell which of our claims are warranted or which sentences to call true, i.e., which ascriptions of truth are warranted. We know that "Snow is white" is true if and only if snow is white, but we may not know whether "Snow is white" is a legitimate affirmation. We may not know whether we can legitimately ascribe truth to "Snow is white." We may not know whether it is the case that snow is white. In a nutshell, what is true (correct ascriptions of truth) depends on the legitimacy of our claims. It is not the other way around.

Thus the relationship of correspondence between a true sentence and the world is established by definition, by the definition of truth, and what is to be discovered is rather how we get to know whether to affirm (legitimately) a given sentence. Quine expresses well that, in order to answer that question, we have to look for the grounds of warranted belief:

> [I]f to call a sentence true is simply to affirm it, then *how can we tell whether to affirm it*?
>
> The lazy answer is "That all depends on what the sentence is. In the case of 'Snow is white' you just look at snow and check the color." The more sympathetic answer is a general analysis of the grounds of warranted belief, hence scientific method.[26] (Emphasis added)

Thus, to the question "how do we know what is true?," or "how do we know which of our ascriptions of truth are warranted?," or "how can we tell whether we can (legitimately) affirm a sentence?," Quine's answer lies in his empiricism,[27] or, "a general analysis of the grounds of warranted belief, hence scientific method."[28]

We can be more specific. If we consider Quine's empirical psychology, it appears, as we shall see, that what provides legitimacy (what provides meaning and evidence) to our talk about the world is the fact that its relation with the world is one of responding. This precision brings an important nuance to the idea of correspondence. In fact, some might even say that the term "correspondence" is unwelcome. Some might prefer, as does Gibson, to abandon the expression "correspondence" altogether:

> Quine does not regard this link [between theory and world] as one of "correspondence". . . . For Quine the link is, rather, a matter of conditioning, verbal dispositions conditioned to stimuli. . . .
>
> This relation is not one of mirroring, or corresponding, nor yet even of referring. It is one of responding, of holophrastic observation sentences conditioned to intersubjectively appreciable ranges of physical stimuli. The moral is that the tug towing the ship of theory is *securely moored* to the shoreline. This is the lesson of mitigated holism and empirical psychology.[29]

If we prefer to keep the expression "correspondence" since Tarski and Quine use it, we still can make good use of Gibson's point and suggest that the concept of correspondence is explained by an analysis of the conditions in which our talk about the world is warranted. This analysis is provided by Quine's empirical approach to language: our talk about the world is warranted, and thus "corresponds" to the world, if its relation to the world is one of responding. Of course, this is oversimplified: appealing to a conditioning process explains how *observation sentences* relate to the world (factuality), but it leaves the question open for our theoretical sentences and our theories generally. How do theoretical sentences and theories get warranted? On what basis can they be said to "correspond" to the world (factuality)?

In order to answer that question, let me turn to Quine's externalized empiricism or genetic approach to evidence:

> We see, then, a strategy for investigating the relation of evidential support, between observation and scientific theory. We can adopt a genetic approach, studying how theoretical language is learned. For the evidential relation is virtually enacted, it would seem, in the learning. This genetic strategy is attractive because the learning of language goes on in the world and is open to scientific study. It is a strategy for the scientific study of scientific method and evidence. *We have here a good reason to regard the theory of language as vital to the theory of knowledge*.[30] (Emphasis added)

The first thing that I want to point out about this genetic approach to evidence is that it makes Quine's behavioral view of language vital to his theory of knowledge, to his views on scientific method and evidence. Thus, in the spirit of this genetic approach to evidence, Quine proposes to drop the question of how our theories are linked to observation, or sensation, or environing situation, and replace it by the question of the relation between observational sentences and theoretical sentences:

> It consists in talking neither of sensation nor of environing situation, but of language: talking of language at the observational end no less than at the theoretical end. I do not suggest that observations themselves are something verbal, but I propose that we drop the talk of observation and talk instead of observation sentences, the sentences that are said to report observations: sentences like "This is red," "This is a rabbit." No matter that sensations are private, and no matter that men may take radically different views of the environing situation; the observation *sentence* serves nicely to pick out what witnesses can agree on.[31]

Thus, as Gibson explains in *Enlightened Empiricism*,[32] the problem of evidence becomes the problem of accounting for the relation between the theoretical talk[33] and the observational talk.[34] Gibson reports that, for Quine, "[t]he channels by which, having learned observation sentences, we acquire theoretical language, are the very channels by which observation lends evidence to scientific theory."[35] The relation between observational talk and theoretical talk, Gibson points out, has two aspects: an evidential one and a semantical one. Thus, the problem of the relation between the two kinds of talk can

be couched in terms of the two following questions: 1) "how is it that one sentence can serve as evidence for another?"; and 2) "how do sentences acquire whatever meanings they can be said to have?"[36]

Still following Gibson, the answer to both these questions begins with the role of observation sentences. Observation sentences bring both meaning and evidence to other kinds of sentences (the theoretical ones) of the theory. Gibson points out that, by definition, observation sentences enjoy unanimous acceptance within a given speech community. This is why they play an *evidential role*. Unanimous acceptance provides observation sentences with evidential force. Also, Gibson recalls that, for Quine, "whatever evidence there *is* for science *is* sensory evidence."[37] Thus, in case of disagreement on the truth value of a theoretical sentence, it is always possible to "descend to the level of observation sentences, and find a common ground for assessing relevant evidence."[38]

Second, observation sentences play a *semantical role* in theories because they are the sentences associated with the non-verbal circumstances intersubjectively appreciated.[39] Gibson recalls that, for Quine, "all inculcation of meanings of words must rest ultimately on sensory evidence."[40] Thus, Gibson concludes, observation sentences are, for Quine, the "gateway to language and hence to science—insofar as scientific theories are conceived of as systems of sentences."[41] Again, it is Quine's genetic approach to evidence that matters: *For the evidential relation is virtually enacted, it would seem, in the learning.*"[42] (Emphasis added)

Thus, to recall, the question of evidence becomes that of accounting for the relation between observational talk and theoretical talk. This relation has a semantical aspect and an evidential aspect. Observation sentences' (stimulus-) meaning and evidence are the stimuli to which the sentences themselves are conditioned responses. Theoretical sentences' "meanings" and evidence are the theoretical sentences' relations (holistic) with observation sentences.[43]

Let me link these results with the question that motivated their report. We first saw that, whereas Quine thinks that *what it is to be true* is relative to a *language*, he thinks that *what is true* and *what is real* are both determined within the *theory* that is operative at the time. To the question of the link between factuality and truth (what is true), Quine's answer, as we saw, is that factuality (extralinguistic reality) determines truth (what is true), that reality is what renders our true

assertions true. Yet, we did not know *how* that happened; we did not know exactly what the relation had to be between factuality and our ascriptions of truth (or affirmations) for those ascriptions (or affirmations) to be warranted. The examination of the suggestion according to which true sentences "correspond" to reality did not bring much light to that question besides the idea that the "correspondence" relation is in fact a relation of responding. This appeal to conditioned response gave us a clue about *how* factuality determines the truth of observation sentences. It gave us a clue about how factuality can serve as meaning and evidence for *observation sentences*. It is Quine's genetic approach that makes the relationship between *theoretical talk* and factuality clear: factuality provides meaning and evidence to theoretical sentences indirectly, that is, via the multifarious relations that those sentences have with observation sentences.

We also saw that the only difference between ascriptions of truth and ascriptions of factuality is the technical device used to ascribe truth: the quotation marks (semantic ascent); for Quine insists on the fact that, even in semantic ascent, what we are concerned with is reality. Indeed, to ascribe truth to our sentences (just to say, for instance, that "Snow is white" is a true sentence) and to make claims about reality (just say that snow is white) is the same, except for the technical detour in the first case. Finally, we saw that ascriptions of truth and ascriptions of factuality are only loosely made in ordinary language. It is only in the language of truth-functional connectives and quantification that truth (what is true) and factuality (what reality is) can be *strictly* ascribed because it is in this notation that our ontological commitments are explicit.

Before concluding this chapter, let me relate what has been said previously on the question of underdetermination of theory.

Can underdetermination still appear if factuality (what there is) determines truth? Yes. Underdetermination is the following doctrine:

> The doctrine of underdetermination of theory claims that theories about the world transcend all possible observations of the world, and, further, that different, competing theories can be developed on the same observational basis. In a word, theories can be shown to be logically incompatible with one another, yet empirically equivalent.[44]

That means that underdetermination is a matter of the evidence we have for our theories (ontologies). The doctrine says something about the link between our theories and their evidence. Truth, however, is an ontological matter, that is to say, as we saw, that the truth predicate establishes a relation between our theories and the world, not between our theories and their evidence. Truth claims are ontological claims. Reality (extralinguistic reality), not evidence, is what renders our true theories true. If a theory is true, it is in virtue of the world, in virtue of factuality, not in virtue of its evidence. Thus, to say that reality determines the truth of our theories allows underdetermination, i.e., the fact that *many* theories can be built on the same observational/evidential basis. And the fact that underdetermination can appear, that many theories can be equally well warranted, does not affect the fact that if *a* theory is true, it is so in virtue of factuality, in virtue of the world. If we were to admit that two conflicting theories are true the truth of each theory would be determined by factuality.[45] In other words, if we were to admit two theories as true, it would not be because truth is a matter of evidence (which is not the case) but because of underdetermination of theory, because many theories (whether we qualify all of them as true or not) can be built on the same observational basis. So, the fact that factuality determines truth allows for underdetermination. The fact that underdetermination appears does not mean that truth is not determined by factuality, nor does it mean that truth is an evidential matter.

Underdetermination of theories does not imply that there is no fact of the matter to the truth of theories either. If a theory is true, it is so in virtue of the world (factuality), so if a theory is true, there is, by Quine's definition of truth, a fact of the matter to its truth. If a theory is true, its truth is *determined* by factuality. Thus, despite underdetermination, there is always a fact of the matter to the truth of a true theory. If we have underdetermination, it is because we can have many *equally warranted* theories; it is not because those theories could all be said to be true (*if* they could all be said to be true: again, Quine oscillates on that question, as we have seen in note 45). Even if we were to admit that two conflicting theories are true, we could not say that there is no fact of the matter to their truth. The truth of each theory would be a matter of fact. Thus, underdetermination of theories does not imply that there is no fact of the matter to the truth of theories.

CONCLUSION

In the analysis of Quine's notion of factuality or fact of the matter presented in the previous chapter, it was shown that, for Quine, factuality is a question of ontology, a question of what there is. In other words, to talk of factuality is to talk about extralinguistic reality. Ontology is determined immanently to the theory that is operative at the time, and so is factuality. The theory that is currently operative, according to Quine, is physics. Factuality or microphysical facts of the matter are immanent to that overall theory of nature and the special status of that theory confers on them their primacy. It is because of Quine's acceptance of physics as the wider frame of reference, as the overall theory of nature, that the ontology of physics is rock-bottom, and that factuality is physical.

In the present chapter I have compared factuality and truth and attempted to explain the difference between the two, in particular, why Quine says that truth is relative to a language whereas factuality is relative to a theory. The distinction between language and theory was the starting point. In a nutshell, language and theory are distinguished because, whereas a language provides us with the truth conditions of its sentences, a theory purports to tell us what is true or what reality is. Whereas a language informs us about what it is for its sentences to be true, a theory tells us which sentences, or which sets of holistically related sentences, are taken to be true, i.e., which sentences "correspond" to reality. When Quine says that truth is relative to a language, we should understand him as saying that what it is for our sentences to be true is relative to a language. When Quine says that truth is relative to a theory, just as factuality is, we should understand him as saying that what is true is relative to a theory, just as factuality is.

What is the difference between what is true and factuality (what reality is)? The difference, Quine insists in *Philosophy of Logic*, is not that in the first case we are concerned with language and in the second with reality. In both cases, he says, we are concerned with reality even if in the first case, in order to attribute truth, we mention sentences. The only difference is that, when we use the truth predicate, we are taking a technical detour (semantic ascent) to talk about the world. Instead of saying "'Snow is white' is true," we could just assert "Snow is white"; we could just claim that snow is white.

When a statement is true, it is so because of reality. No statement is true that reality has not rendered true. Thus factuality determines truth (what is true). In order to be able to say more about *how* factuality determines truth, about how factuality provides meaning and evidence to our ascriptions of truth (theories), I have looked at the idea of correspondence. This brought enlightenment insofar as it told us that the correspondence is in fact a relation of responding to the world—directly for observation sentences and indirectly (via observation sentences) for theoretical sentences. The stimuli to which the observation sentences are related via conditioning constitute the (stimulus-) meaning of and the evidence for observation sentences. Theoretical sentences are related to factuality only indirectly, through their multifarious relations with those observation sentences. The relations between the theoretical sentences and the observation sentences constitute both the meaning of and the evidence for the theoretical sentences. So, finally, it is by an analysis of the role of the observation sentences that the relation between factuality and truth (what is true) has been clarified. Understanding the semantical and the evidential role of the observation sentences is the key to understanding how reality renders our theories true, i.e., how reality determines what is true, how reality provides meaning and evidence to our ascriptions of truth.

NOTES

1. Quine, W.V., *Theories and Things*. Cambridge, MA: The Belknap Press of Harvard University Press, 1981, p. 23.
2. Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 685.
3. Ibid., p. 367. Here are two other examples: "Disavowing as I do a first philosophy outside science, I can attribute reality and truth only within the terms and standards of the scientific system of the world that I now accept; only immanently." (Ibid., p. 316.) "But it is a confusion to suppose that we can stand aloof and recognize all the alternative ontologies as true in their several ways, all the envisaged worlds as real. It is a confusion of truth with evidential support. Truth is immanent, and there is no higher. We must speak from within a theory, albeit any of various" (Quine, *Theories and Things*, pp. 21–2).
4. "'Snow is white' is true if and only if snow is white. To ascribe truth to the sentence is to ascribe whiteness to snow; such is the correspon-

dence, in this example. Ascription of truth just cancels the quotation marks. Truth is disquotation. So the truth predicate is superfluous when ascribed to a given sentence; you could just utter the sentence" (Quine, W.V., *Pursuit of Truth*. Cambridge, MA: Harvard University Press, 1990, p. 80).

5. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 367.
6. Ibid., p. 316.
7. On the other hand, as we just saw, *what it is to be true* is relative to a language.
8. Quine, W.V., *Philosophy of Logic* (second edn). Cambridge, MA: Harvard University Press, 1970, p. 10.
9. Ibid., p. 11.
10. Ibid.
11. Ibid., p. 12.
12. Ibid.
13. Ibid.
14. Quine adds that we can say that the sentences themselves are true or false only if they are eternal sentences, i.e., sentences that remain true or are always false regardless of the particular circumstances in which they occur. Eternal sentences are the sentences of arithmetic, the laws of physics, and any statement to which we add indications of person (names) and time (dates), and in which we neutralize the verb tenses. Of course a sentence is eternal relatively to a particular language, at a particular time. Hence the advantage of assigning truth values to concrete occurrences or signs of sentences and of considering only the language of whoever is speaking at the time he or she is speaking. In practice, we might find it useful to talk of the truth value of eternal sentences, but we must keep the relativity to the linguistic habits of the moment in mind. (For more details, see Quine, *Philosophy of Logic*, pp. 13–14.)
15. See ibid., p. 13.
16. Orenstein, A., *W.V. Quine*. Princeton: Princeton University Press, 2002, p. 33.
17. Quine, *Pursuit of Truth*, p. 81.
18. I will analyze the expression "corresponds" later in the present chapter.
19. Here is how Orenstein puts it: "A salient reason why Quine regards this language as being 'canonical' is that it is here that our use of the existential quantifier '$(\exists x)$' is most explicit. To discover the existence assumptions, the ontological commitments, of a theory, we first state it in the language of truth functional connectives and quantification, and then look to the existential quantifications we have made. On Quine's view, 'Quantification is an ontic idiom *par excellence*.' The logic of '$(\exists x)$' is the logic of existence, and a notation that makes '$(\exists x)$' explicit accord-

ingly makes our existence assumptions/ontology explicit" (Orenstein, *W.V. Quine*, p. 23). In other words, the interpretation of the particular quantifier as the existential quantifier within the usage of the notation of modern logic is the key to our making ontological commitments explicit. Quine's ontological slogan, we can recall, is: to be is to be the value of a variable.

20. Shahan, R.W. and Swoyer, C. (eds), *Essays on the Philosophy of W.V. Quine*. Norman: University of Oklahoma Press, 1979, pp. 161–2.
21. In ordinary language our concern in not mainly with what there is but with all sorts of other matters. It is not the primary purpose of ordinary language to tell us what there is.
22. "There is an instructive parallel between questions of reference, on the part of ordinary language, and questions of factuality. Let me recall what I have said earlier when discussing ontology. Ordinary language is only loosely referential, and any ontological accounting makes sense only relative to an appropriate regimentation of language. The regimentation is not a matter of eliciting some latent but determinate ontological content of ordinary language. It is a matter rather of freely creating an ontology-oriented language that can supplant ordinary language in serving some particular purposes that one has in mind.

    Now factuality is similar. Ordinary language is only loosely factual, and needs to be variously regimented when our purpose is scientific understanding. The regimentation is again not a matter of eliciting a latent content. It again is a free creation. We withdraw to a language which, though not limited to the assigning of elementary physical states to regions, is visibly directed to factual distinctions—distinctions that are unquestionably underlain by differences, however inscrutable, in elementary physical states. This demand is apt to be met by stressing the behavioral and the physiological" (ibid., pp. 168–9).
23. Quine, *Pursuit of Truth*, pp. 81–2.
24. Orenstein, *W.V. Quine*, pp. 32–3.
25. Quine, *Pursuit of Truth*, p. 82.
26. Ibid., p. 93.
27. "The most notable norm of naturalized epistemology actually coincides with that of traditional epistemology. It is simply the watchword of empiricism: *nihil in mente quod non prius in sensu*. This is a prime specimen of naturalized epistemology, for it is a finding of natural science itself, however faillible, that our information about the world comes only through impacts on our sensory receptors" (ibid., p. 19).
28. Ibid., p. 93.
29. Gibson, R., *Enlightened Empiricism: An Examination of W.V. Quine's Theory of Knowledge*. Tampa: University of South Florida Press, 1988, p. 81.

30. Quine, W.V., "The Nature of Natural Knowledge," in S. Guttenplan (ed.), *Mind and Language: Wolfson College Lectures*. Oxford: Clarendon Press, 1975, pp. 74–5.
31. Quine, W.V., *The Roots of Reference*. La Salle, IL: Open Court, 1974, p. 39.
32. See Gibson, *Enlightened Empiricism*, pp. 6–7.
33. He says: "sentences learned by analogic synthesis."
34. He says: "sentences learned by ostension."
35. Quine, "The Nature of Natural Knowledge," p. 74.
36. Gibson, *Enlightened Empiricism*, p. 6.
37. Quine, W.V., *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969, p. 75.
38. Gibson, *Enlightened Empiricism*, p. 6.
39. There must be those shared non-verbal reference points to which some utterances are associated if language learning is to be explained by a behavioral approach, that is to say, if language is to be learned, as Quine thinks, from other people in shared circumstances.
40. Quine, *Ontological Relativity and Other Essays*, p. 75.
41. Gibson, *Enlightened Empiricism*, p. 7.
42. Quine, "The Nature of Natural Knowledge," pp. 74–5.
43. Gibson, *Enlightened Empiricism*, pp. 54–5.
44. Ibid., pp. 11–12. As Gibson points out, "when Quine is talking of underdetermination, he is doing so only in connection with global world theories and not in connection with any lesser theories" (ibid., p. 116).
45. We know that Quine oscillates about the possibility of admitting two conflicting theories to be true: he oscillates between the ecumenical and the sectarian view. He writes: "In the first passage I had held that one of two systems of the world must be deemed false even if we know them to be empirically equivalent. I shall call this the *sectarian* position. My reason for it was my naturalism: my disavowal of any higher tribunal than science itself. In the later and conflicting passage, as Gibson relates, I opted for truth of both systems of the world, finding it offensive to my empiricist sensibilities to declare otherwise. This I shall call the *ecumenical* position" (Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 156).

CHAPTER 4

THE INDETERMINACY THESIS

Now that Quine's notion of fact of the matter has been clarified, we should be able to clarify Quine's indeterminacy thesis, that is, his claim that there is no fact of the matter vis-à-vis rightness of translation manuals that are equally compatible with speech dispositions. However, having a good take on the notion of fact of the matter might not be sufficient for such a clarification since, as we shall see, the indeterminacy thesis does not always appeal to the notion of factuality or fact of the matter. In the second section of the present chapter, I will examine whether these differences in the argumentation for the thesis are of importance, whether they are differences in ways of talking, changes in emphasis, or genuine changes of mind (substantial changes). But, in a preliminary section, I will review some formulations of the thesis to clarify their content and to try to evaluate the importance or otherwise of the variations that occur.

FORMULATIONS

Indeterminacy of translation is a thesis or doctrine. Quine offers arguments for it, not a factual illustration, nor a deductive proof.[1]

What does this doctrine claim exactly? At first sight, when we look at various formulations of it, we might be tempted to conclude that it is not altogether clear what Quine is defending in the indeterminacy thesis, for its content seems to have varied. Thus, before discussing the fact that Quine's *argumentation* for indeterminacy of translation

varies in terms of whether or not an appeal is made to the notion of factuality and physicalism, I will indicate and review some variations in the *formulations* of the thesis in order to see if, despite the appearance of a variation in their content, they have a common core.

First, some formulations of the indeterminacy of translation thesis claim that, given two or more incompatible translation manuals that conform to all the same speech dispositions, there is no basis for saying *which is right and which is wrong*.[2]

> My thesis of the indeterminacy of translation is that mutually incompatible manuals of translation can conform to all the same distributions of speech dispositions. But the only facts of nature that bear on the correctness of translation are speech dispositions. Thus mutually incompatible manuals of translation can conform to all the same overall states of nature, hence all the same distributions of microphysical states. Yet, being incompatible, *both manuals can scarcely be right. Which one is, if either?* I say there is no fact of the matter. This illustrates my identification of facts of the matter with distribution of microphysical states.[3] (Emphasis added)

> My position was that either manual could be useful, but as to *which was right and which was wrong* there was no fact of the matter. . . . I speak as a physicalist in saying that there is no fact of the matter. I mean that both manuals are compatible with the fulfillment of just the same elementary physical states by space-time regions.[4] (Emphasis added)

By those formulations of the indeterminacy thesis Quine asserts that there is no basis for saying which manual is right and which one is wrong. One could raise the question: does Quine suppose that one manual must be right and that one must be wrong, or does he mean that the question of knowing which one is right and which one is wrong is a pseudo-question? Chapter 2 provided at least part of the answer,[5] but, for now, I shall let the question remain open, for all I want to indicate is that some of Quine's formulations of his thesis can easily permit one to wonder about how Quine judges (right or wrong) the two manuals.

Second, from the fact that Quine insists on the incompatibility of the manuals, one can conclude that he thinks at least one of the two

incompatible translation manuals that conform to all the same speech dispositions must be wrong. Two manuals are incompatible if and only if at least one of them must be wrong.

> Or suppose they [two field linguists] both compiled manuals of translation, and both manuals proved successful in translating some long native monologue into coherent English. Then suppose we translate it again using the two manuals alternately, sentence by sentence. Would the result be coherent? If not, and given no basis for saying *which manual is at fault*, we have what I have called the indeterminacy of translation.[6] (Emphasis added)

Third, the following formulation insists on the fact that of two manuals that fit all the same speech dispositions, there is no basis for saying that one manual is *better than the other*:

> [M]y doctrine of indeterminacy had to do with hypothetical manuals of translation both of which fitted all behavior. Since translators do not supplement their behavioral criteria with neurological criteria, much less with telepathy, *what excuse could there be for supposing that the one manual conformed to any distribution of elementary physical states better than the other manual*? What excuse, in short, for supposing that there is a fact of the matter?[7] (Emphasis added)

In the same spirit, Quine here seems to nuance what he has said in the first set of formulations:

> A pioneer manual of translation has its utility as an aid to negotiation with the native community. Success in communication is judged by smoothness of conversation, by frequent predictability of verbal and non-verbal reactions, and by coherence and plausibility of native testimony. *It is a matter of better and worse manuals rather than flatly right and wrong ones*.[8] (Emphasis added)

Perhaps it is implicit in the first of these two passages that none of the manuals need be better or worse than any of its rivals. This reading would square well with the next reading too: many manuals may be right as long as they conform to all checkpoints of verbal behavior.

Fourth, then the following formulation now suggests that, if there

is no basis to choose between the two manuals, it is because they are both right. "Indeterminacy means not that there is no acceptable translation, but that there are many. A good manual of translation fits all checkpoints of verbal behavior, and what does not surface at any checkpoint can do no harm."[9] Thus, sometimes, the formulation of the indeterminacy thesis allows for the rival manuals both to be right: "The indeterminacy thesis ... tells us that right translations can sharply diverge."[10]

Finally, in the following formulation, there is no way to misread Quine as supposing that one of the rival manuals must be right and that one must be wrong. There is no way either to read him as saying that at least one of the manuals must be wrong. There is no way to see the claim that one manual is better than the other, or the claim that both manuals are right. In fact, there is no evaluative claim on Quine's part about the manuals. This time the formulation provides the translators' perspective (whether the manuals are acceptable or not according to the translators), and it claims that two radical translators could reject one another's manuals.

> These reflections leave us little reason to expect that two radical translators, working independently, would come out with manuals acceptable to both. Their manuals might be indistinguishable in terms of any native behavior that they gave reason to expect, and yet each manual might prescribe some translation that the other translator would reject. Such is the thesis of the indeterminacy of translation.[11]

This claim about translators rejecting one another's manuals is not equivalent to the claim about incompatibility of the manuals. The former is a pragmatic claim about translators, and the latter is a semantic claim about translations. While translators might appeal to mental or attitudinal grounds, the translations can appeal only to factual or natural grounds.

We can see that in all the formulations, indeterminacy concerns two (or more) rival hypothetical manuals of translation that are behaviorally equivalent. What varies through the diverse formulations is the way that Quine qualifies those manuals: if we acknowledge that the first reading (one manual must be right and one must be wrong) cannot be seriously considered (for it supposes an epistemological

reading of the notion of fact of the matter), it remains that sometimes Quine claims 1) that at least one of the manuals must be wrong (for he says that they are incompatible); 2) that it is not a case of right or wrong but of better or worse; 3) that despite the conflict, all such manuals are equally right; and 4) that each translator involved may accept his own manual and reject all the others (without any judging from Quine of the manuals being right or wrong).

Is this variation through the formulations of importance? Maybe not, for the core of the thesis seems to remain constant: there is no basis for preferring or choosing among the two (or more) rival manuals. In all cases, no matter how Quine qualifies or evaluates the rival manuals, it remains the case that there is no basis for preferring or choosing among them. Thus, if we want to be maximally charitable and attribute a maximum of constancy to Quine's attitude toward indeterminacy of translation, it seems that the best way to describe the thesis is to say that it claims that what is indeterminate is the preference or the choice between rival translation manuals, regardless of how we judge the manuals (i.e., regardless which of the four possibilities mentioned in the previous paragraph we select). In other words, the indeterminacy thesis does not concern the correctness or rightness or truth of translation manuals taken individually. It does not concern the cognitive value of individual translation manuals. It concerns only the situation in which there are two or more behaviorally equivalent translation manuals: the indeterminacy thesis claims that the choice among them is indeterminate. The following passage is clear on that point:

> Farther along he [Paul A. Roth] seems to ascribe to me a thesis . . . to the effect that "there is no warrant . . . for attributing a fact of the matter to semantics theory." On the contrary, the conformity of a translation manual to speech dispositions is decidedly a matter of fact. It is only the choice between certain rival manuals that lacks factuality.[12]

In other words, indeterminacy of translation does not constitute an *evaluative* claim about the translation enterprise or its result (the manuals). As I will try to show in more detail in Chapter 5, indeterminacy of translation is rather a claim about meaning. It expresses a *fact* about language. Thus, we can conclude, the analysis of the

variations in the formulations of the thesis indicates that the core of the thesis is not the *evaluation* of translation or translation manuals (this element varies throughout the formulations), but rather the *fact* that the choice among the rival manuals is indeterminate. However, this way of talking can be misleading. It does not mean that our psychology (mental states or attitudes) or beliefs about the rival manuals are indeterminate. Saying that the preference or choice between the rival manuals is indeterminate means that *nature* (factuality) does not decide among the manuals. It means that factuality does not favor one manual over the other.[13] More precisely, it means that the ontology of our actual theory of nature does not favor one manual over the other. In other words, choosing among rival translation manuals is not a question of what nature is, not a question of what there is, not a question of ontology or factuality. Whatever the state of the world, it is indifferent to our choice between rival translation manuals; whatever objects we say there are in the world, they are indifferent to our choice among rival translation manuals.

ARGUMENTS

In order to clarify further Quine's treatment of indeterminacy, I shall now look more closely at the arguments Quine has offered for it. Why is it that our theory of nature does not favor one manual over the other? Why is the state of the world indifferent to our choice among rival translation manuals? Looking at some passages, it is not easy to answer those questions, for the core of the argumentation is hard to find. Consider the following:

> My thesis of the indeterminacy of translation is that mutually incompatible manuals of translation can conform to all the same distributions of speech dispositions. But the only facts of nature that bear on the correctness of translation are speech dispositions. Thus mutually incompatible manuals of translation can conform to all the same overall states of nature, hence all the same distributions of microphysical states. Yet, being incompatible, both manuals can scarcely be right. Which one is, if either? I say there is no fact of the matter. This illustrates my identification of facts of the matter with distribution of microphysical states.[14]

I find this passage (and others) on indeterminacy of translation particularly puzzling, and the question that comes to mind is: what is the core of the argument for indeterminacy? Is it Quine's physicalistic view of nature or his behavioristic view of language? In order to understand more fully Quine's argumentation for indeterminacy, let me start with that question, which, at least at first sight, seems hard to answer, for Quine seems to have switched his emphasis from physicalism to behaviorism in his argumentation for indeterminacy of translation. I think that in order to identify the core of his argument, it is important to judge whether physicalism is central, whether behaviorism is central, or whether Quine's indeterminacy thesis needs both.

When Quine first argued for indeterminacy of translation, he has emphasized physicalism. In these passages (see pages 45 and 46 above), he argued for indeterminacy by appealing to the physicalistic notion of fact of the matter and by showing that there is no such physical fact of the matter to the choice between two hypothetical rival manuals.

In later passages (see pages 46 and 47 above) the emphasis changes: no reference to physicalism or to the notion of fact of the matter is made. Instead of insisting on the fact that there is no physical fact of the matter to the choice among the rival manuals, Quine emphasizes the fact that behavior does not decide among them. In other words, he now stresses the public and conventional nature of language and the fact of the translator's own input: "What the indeterminacy thesis is meant to bring out is that the radical translator is bound to impose fully as much as he discovers."[15]

Does this difference in the argumentation imply a change of mind or only a change of emphasis? Some might think that we should take it as a change of mind, if they take seriously the following concessions on Quine's part:

> Dagfinn has illuminated the indeterminacy thesis by clearing away what does not pertain. What matters is just that linguistic meaning is a function of observable behavior in observable circumstances. Dagfinn divides this into two: that meaning is the product of the evidence by which it is learned, and that that evidence is public.[16]

> Broader behaviorism is irrelevant; *physicalism is irrelevant*; monism is irrelevant. One can wallow in the rankest mentalistic ontology

without affecting the indeterminacy of translation.[17] (Emphasis added)

Thus, Quine has claimed that physicalism is irrelevant to indeterminacy. The core of the argumentation for indeterminacy, then, might rather be his behavioristic approach to language. At least, we can say that this is Quine's view in *Perspectives on Quine* (1990). It is also his view in "Indeterminacy of Translation Again" (1987), where he claims to undertake a "succinct over-all clarification" of his view on indeterminacy of translation after having suffered 25 years of critics addressing the first version of the thesis presented in *Word and Object*. In this clarification there is no appeal to physicalism either. Once again, Quine appeals only to behaviorism. Let's look at his argumentation in that text.

> Critics have said that the thesis [indeterminacy of translation] is a consequence of my behaviorism. Some have said that it is a *reductio ad absurdum* of my behaviorism. I disagree with the second point, but I agree with the first. I hold further that the behaviorist approach is mandatory. In psychology one may or may not be behaviorist, but in linguistics one has no choice. Each of us learns his language by observing other people's verbal behavior and having his own faltering verbal behavior observed and reinforced or corrected by others. We depend strictly on overt behavior in observable situations. As long as our command of our language fits all external checkpoints, where our utterance or our reaction to someone's utterance can be appraised in the light of some shared situation, so long all is well. Our mental life between checkpoints is indifferent to our rating as a master of the language.
>
> There is nothing in linguistic meaning, then, beyond what is to be gleaned from overt behavior in observable circumstances. In order to exhibit these limitations, I propounded the thought experiment of radical translation.[18]

So, in order to show that there is nothing more to meaning than what can be gleaned from overt behavior in observable circumstances, in order to critique the idea of sameness of meaning (mental entities),[19] or to put it differently, to show that we do not have identity criteria for meanings,[20] Quine proposes the thought experiment of

radical translation. Let me sum up how he describes it in "Indeterminacy of Translation Again,"[21] where, I propose, he claims to have made an overall clarification of his views on indeterminacy.

Quine's thought experiment consists in imagining a translator who wants to translate Jungle (the source language) into English (the target language) and to whom we allow as data only the native utterances and their observable circumstances. (It is an artificial situation in the sense that ordinarily a translator would be helped by a bilingual speaker.) The translation manual is developed by extrapolation of those data, but confirmation is meager because, in most cases, an observable situation does not enable the translator to predict what the native will say: often what we say bears little relevance to the observable circumstances in which it is uttered. What we say bears about as much relevance to our past experience as to our ongoing practical goals for instance. Apart from observational sentences, most utterances will not allow a correlation with observable circumstances. The translator thus has to take the initiative, try his sentences in various situations, and see if the natives assent or dissent. Of course, no correlation is immediately forthcoming. The next step is to construe analytical hypotheses: the translator, by guess-work, dissects the observational sentences and pairs off the parts with other parts in ways that seem appropriate according to the models (occurrences of observation sentences). Thus accumulating information and conjecturing on it, the translator builds a tentative vocabulary (with English translations) and a tentative grammar (a system of rules of construction of new sentences in Jungle), while continually testing the efficacy of his construction to help him in communicating with the native. To help guide his construction, the translator can rely on psychological conjectures. For instance, he takes into account what the native is likely to believe, he supposes that the language is simple enough to have been learned naturally by the natives, he supposes that the natives' minds are like his. The translator also practices empathy, that is, he tries to put himself into the natives' shoes as much as he can. Moreover, the translator will observe the natives' reaction. If conversation runs smoothly, it is a sign that the translation is good. If the natives seem surprised, it is a sign that translation might be on the wrong tack.

Things of that sort are all that the radical translator can rely on. The meagerness of these resources, Quine says, leaves us little reason

to think that two translators who worked independently on the native's language would come out with manuals that were mutually acceptable even if the manuals are indistinguishable "in terms of any native behavior that they gave reason to expect." This is the claim of the indeterminacy of translation thesis in "Indeterminacy of Translation Again":

> Their manuals might be indistinguishable in terms of any native behavior that they gave reason to expect, and yet each manual might prescribe some translation that the other translator would reject. Such is the thesis of indeterminacy of translation.[22]

We see that here then, in "Indeterminacy of Translation Again," where Quine claims to have made an overall clarification of his ideas on indeterminacy of translation, the argument appeals not to physicalism but only to behaviorism.

Thus, it seems fair to say that, *at least*, Quine has changed his emphasis from physicalism to behaviorism in his argumentation for indeterminacy of translation. At first, his physicalistic view of nature appears as the most important element of the argumentation, but later it seems to have become irrelevant (according to Quine himself): the behavioristic approach is finally presented as sufficient by itself to entail indeterminacy of translation. Is this change a dramatic change? No, for despite the fact that behaviorism was not what Quine had first emphasized, it was part of the argument from the beginning, and it was what really entailed indeterminacy. In other words, even if it was not acknowledged as such, the behavioristic approach to language already adopted by Quine was the source of indeterminacy. Indeed, when Quine says that there is no physical fact of the matter or factuality to a choice among translation manuals, it is because he already sees that behavioral factuality is the only factuality relevant to semantics—it is because he already sees that semantics is stuck at the behavioral level.

More importantly, it seems that this change in emphasis is not of consequence because the difference lies in the arguments for indeterminacy and not in the content of the thesis. Whether Quine insists on physicalism or on behaviorism in his argumentation for indeterminacy, indeterminacy remains what it was: given two rival translation manuals that allow fluent conversation with the native

equally well (they fit all the same distributions of speech dispositions) there is no basis (neither physical nor behavioral) to favor one manual over the other. The core of the thesis remains the same whether one agrees because, as a physicalist, one insists on the fact that physics (our theory of nature towards which one is realistic) is irrelevant to the question, or because one (as a behaviorist in semantics) insists on the fact that behavior is all that is relevant to the question.

The thesis thus means that there is neither a physical nor a behavioral fact of the matter to the choice among rival translation manuals. When a manual is right, there is a behavioral fact of the matter (which depends on physical microstates) to its rightness. If *ex hypothesi* two manuals both conform to all the speech dispositions, they are both right, and the rightness of each is behaviorally (and ultimately physically) factual also. What lacks factuality (behavioral or physical) is the choice among the two manuals of which the rightness is factual. There is no other kind of factuality above behavioral factuality to which we can appeal to decide between the two behaviorially equivalent manuals, because no other kind of factuality is relevant to semantics.

NOTES

1. I will present Quine's arguments in the second section. On factual illustration cf.: "[T]he full or holophrastic indeterminacy of translation draws too broadly on a language to admit factual illustration" (Quine, W.V., *Pursuit of Truth*. Cambridge, MA: Harvard University Press, 1990, p. 50). On proof cf.: "My argument for the indeterminacy of translation, or for that matter my argument against analyticity in 'Two dogmas', is not a proof by cases. It is not a proof at all" (Barrett, R. and Gibson, R., *Perspectives on Quine*. Oxford: Basil Blackwell, 1990, p. 198). Quine talks of proof only in logical contexts: "I have engaged in proofs in logical contexts, but not in empirical ones" (ibid.).
2. Throughout the discussion, I will talk of two translation manuals, but it could be more than two. If there are more than two manuals, we can say that they are pair-wise incompatible if and only if each is incompatible with each of the others, which is true if and only if at least all but one manual must be wrong.
3. Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 429.

4. Shahan, R.W. and Swoyer, C. (eds), *Essays on the Philosophy of W.V. Quine*. Norman: University of Oklahoma Press, 1979, p. 167.
5. One of the conclusions of Chapter 2 is that the notion of fact of the matter is ontological, so we might already prefer the idea that Quine means that the question is a pseudo-question and not that one manual must be right and one must be wrong. Indeed, in order to say that Quine supposes that one manual must *be* right and one manual must *be* wrong, we need to read "there is no fact of the matter" in an epistemological sense, i.e., as saying simply that although one manual is right and one is wrong, which manual is right and which one is wrong is not known, or not knowable. But this reading of "there is no fact of the matter" has been rejected in Chapter 2. The ontological reading of "there is no fact of the matter," which we have accepted, rather suggests that if two manuals are behaviorally equivalent, the question of which one is right and which one is wrong is misplaced or, to put it differently, that, in this situation, asking which one is right and which one is wrong is a misapplication of the predicates "right" and "wrong." In other words, the meaning of those predicates requires the existence of a fact of the matter if one is to predicate either of them.
6. Quine, W.V. (1998), *From Stimulus to Science*. Cambridge, MA: Harvard University Press, p. 82.
7. Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 167.
8. Barrett and Gibson, *Perspectives on Quine*, p. 4.
9. Quine, W.V., "Indeterminacy of Translation Again." *Journal of Philosophy*, 84, 1987, p. 9.
10. Barrett and Gibson, *Perspectives on Quine*, p. 198.
11. Quine, "Indeterminacy of Translation Again," p. 8.
12. Hahn and Schilpp, *The Philosophy of W.V. Quine*, pp. 459–60. This makes sense since: "[w]hen we talk of mental states or events subject to behavioral criteria, we can rest assured that we are not just bandying words; there is a physical fact of the matter, a fact ultimately of elementary physical states" (Shahan and Swoyer, *Essays on the Philosophy of W.V. Quine*, p. 167) and "[w]hat is utterly factual is just the fluency of conversation and the effectiveness of negotiation that one or another manual of translation serves to induce" (Barrett and Gibson, *Perspectives on Quine*, p. 4).
13. Thus, it seems that, finally, Quine should not want to retain a formulation of indeterminacy that talks about the translators and their judgments (including mental or attitudinal grounds) on each other's manuals, i.e., a formulation that makes a practical claim about the translators. It seems that he would rather want to retain a formulation that talks not about the translators but about the translations (and their

relation to factuality), i.e., a formulation that makes a semantical claim about translations.

14. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 429.
15. Barrett and Gibson, *Perspectives on Quine*, p. 5.
16. Ibid., p. 110.
17. Ibid.
18. Quine, "Indeterminacy of Translation Again," p. 5.
19. "What I have challenged is just an ill-conceived notion within traditional semantics, namely, sameness of meaning" (ibid., p. 10).
20. "The point of my thought experiment in radical translations was philosophical: a critique of the uncritical notion of meanings and, therewith, of introspective semantics" (ibid., p. 9).
21. Ibid., pp. 5–8.
22. Ibid., p. 8. Quine recalls that indeterminacy of translation applies to sentences holophrastically conceived. It must be distinguished from indeterminacy of translation of terms, to which the example of gavagai was exclusively directed (see ibid., pp. 8–9).

CHAPTER 5

BEHAVIORISM, NEUROLOGY, AND INDETERMINACY OF TRANSLATION

To say that only behavioral factuality is relevant for semantics implies that any explanation of verbal behavior in terms of neurology would not entail progress in semantics; that is to say, that neurology would not provide a basis for establishing synonymy relationships, that it would not provide identity criteria for meanings. In other words, it is not because of our incapacity to reduce behavioral talk to neurological talk that Quine concludes there is no fact of the matter to the choice among rival translation manuals. Rather, it is because neurology is of no use (is irrelevant) to the identification of meanings or, which is the same, to the establishment of synonymy relationships.

Quine rejects the suggestion that an appeal to neurology could tell us what people mean and help us in favoring one of two behaviorally equivalent manuals over the other. Examining neurological states, so some philosophers think, could let us know what people really mean, and thereby could help us choose one manual over the other. Thus, it is claimed, although two rival manuals allow for equal fluency of dialogue with the natives, although both are equally in conformity to verbal behavior, a translator could prefer one over the other on the basis of information provided by neurology. The problem with this suggestion is that, in fact, neurology brings nothing to the identification of meaning; for meaning, according to Quine, is nothing

more than what can be gleaned from overt behavior in observable circumstances:

> Discussion with Dreben helped to clarify these consequences of my new stance. In *Word and Object* I had already pointed out that communication presupposes no similarity in nerve nets; verbal behavior is inculcated to the strength only of surface stimulation. Such was my parable of the trimmed bushes (p. 8), alike in outward form but wildly unlike in their inward twigs and branches. Save the surface, in the paintmaker's words, and you save all.[1]

> [E]ven a full understanding of neurology would in no way resolve the indeterminacy of translation.[2]

Is this refusal to appeal to neurology in semantics at odds with the physicalistic view of explanation presented in Chapter 2? It is worth asking, indeed, since we saw that for Quine:

> The terms that play a leading role in a good conceptual apparatus are terms that promise to play a leading role in causal explanation; and causal explanation is polarized. Causal explanations of psychology are to be sought in physiology, of physiology in biology, of biology in chemistry, and of chemistry in physics—in the elementary physical states.[3]

Why, when it comes to semantics, is Quine confident that progress in neurology would not resolve the indeterminacy of translation? Because indeterminacy of translation is linked to the question of what linguistic meaning *is* and not to what its causes are. In other words, the behavioral approach to linguistic meaning that Quine adopts does not prevent the appeal to neurology when what is sought is *causal explanation*. What behaviorism does is tell us where to start in our study of meaning—it tells us what meaning is and where to find it, that is, what the data for semantics are. Appealing to behaviorism in semantics is not meant to provide an *explanation*. It only says what kind of data (behavioral) is relevant for the enterprise. It does not provide the cause of (does not explain) meaning:

> Segal chafes at my linguistic behaviorism. Let me then stress its

> limits. *It disciplines data, not explanation.* On the explanatory side my readers are familiar rather with my recourse to innate endowments. I cite instinct and hence natural selection to explain induction, and to explain also our innate subjective standards of perceptual similarity and their preestablished intersubjective harmony. All this is essential to language readiness. Behaviorism welcomes genetics, neurology, and innate endowments. It just excludes mentalistic explanation. It defines mentalistic concepts rather, if at all, by their observable manifestations in behavior.[4] (Emphasis added)

What the appeal to behaviorism does is discipline the *data* of semantics. This has the effect of preventing any appeals to mentalistic explanation, introspection, or telepathy in the study of meaning.

We see then that there need be no tension between Quine's refusal to appeal to neurology and his physicalistic view of explanation if we acknowledge his claim that behaviorism disciplines the data for semantics, not explanation, and acknowledge that the question of what meaning *is* must be distinguished from the question of its *causes*. We could get all the explanations we wish to have about the underlying psychological process causing verbal behavior; still, the study of meaning would remain at the behavioral level, for verbal behaviors are the only data relevant to semantics: "Whatever the best eventual theory regarding the inner mechanisms of language may turn out to be, it is bound to conform to the behavioral character of language learning: the dependance of verbal behavior on observation of verbal behavior."[5]

Knowledge of the variations in neurological states is indifferent to a person's credentials as one who understands the language. All that he needs to be counted among the masters of a language is that his verbal behaviors fit all the external checkpoints. In other words, if we want to know what people *mean*, we have to look at what they actually say, or, as Quine puts it, we must take verbal behaviors as the identity criteria for mental contents. Looking at their brain states does not help us to know what people mean. Let's say that neurology makes progress in telling us how the functioning of certain parts of the brain affects verbal behavior. Let's say that neurologists establish strong correlations between the occurrences of some sentences and the activity of some regions of the brain, thus suggesting that such and such part of the brain is responsible for such and such verbal

behavior. All that this tells us is that when such and such verbal output occurs, such and such brain events occur. It still does not tell us anything about what people think, i.e., it does not tell us about mental states or meanings. Thus it appears that knowledge of brain states is irrelevant to semantics. Moreover, there is the problem of individuation. We saw that progress in the description of brain states cannot diminish the indeterminacy of translation, for it cannot provide individuation criteria for meanings. Again: "how much causal background should we include? how does the suggestion work for terms for whose application there are no separable data? And when there are such separable data, why not just take them as the meaning instead of the causes?"[6]

Thus, progress in neurology does not eliminate nor even reduce indeterminacy of translation, for, according to the thesis, the only factuality relevant to translation and semantics is behavioral, and *ex hypothesi* the rival manuals are equivalent in respect to that factuality. *Ex hypothesis* the two manuals conform equally to all checkpoints of verbal behavior, and thus are semantically equivalent. Hence the choice among them is indeterminate, for they mean the same. The two manuals are equivalent in regard to behavioral checkpoints, and there is no other semantical standard to which they could or should conform. That is to say, if two manuals are behaviorally equivalent, they are semantically equivalent, for meaning is nothing more than what can be gleaned from the observation of overt behavior in observable circumstances. If the two manuals are semantically equivalent, if they mean the same, the choice between them is indeterminate; even full knowledge of the state of the world (physiology, neurology, or anything else) cannot help determine the decision.

Thus, for Quine, if the question is one of how we *explain* (give the causes of) verbal behaviors (and hence meaning), then "behaviorism welcomes genetics, neurology, and innate endowments."[7] But if the question is what linguistic meaning *is*, or what the data for semantics are, the answer is provided by behaviorism, not by neurology, not by genetics, and, of course, not by telepathy, nor by introspection (I will come back to mentalism later). Behaviorism in semantics is not at odds with neurology, it is rather the contrary: by telling semantics where to start, i.e., what meaning is, where it is (in behavior), behaviorism assures that when it comes to explanation the route taken will not be mentalist but materialist (neurology, genetics, etc.). In limiting

the data of semantics to verbal behavior in observable circumstances, behaviorism paves the way for neurological *explanation.* But, again, although *any* difference depends (causally) ultimately on the arrangements of microphysical states, the study of the *nature* of meaning gains nothing from the knowledge of physics or neurology.

Thus the source of indeterminacy of translation lies in what Quine sees as the nature of linguistic meaning, i.e. in the behaviorist nature of semantic data. That is to say, it is linked to Quine's decision to adopt an empirical and behavioral approach to language; i.e., the crucial element for Quine's indeterminacy thesis is his naturalistic-behavioristic (NB) thesis:

> The crucial point [for indeterminacy of translation] is that according to the NB conception of language the linguists have only the behavioral dispositions of the foreigner upon which to base their translations, and if no possible behavior disposition can settle the question "What did the foreigner really mean by 'S'?" then the question is senseless.[8]

Gibson reviews many passages where Quine expresses this naturalistic-behavioristic thesis:

(1) "Language is a social art. In acquiring it we have to depend entirely on intersubjectively available cues as to what to say and when" (*WO*, ix).
(2) "Language is a social art which we all acquire on the evidence solely of other people's overt behavior under publicly recognizable circumstances" (*OR*, 26).
(3) "A language is mastered through social emulation and social feedback, and these controls ignore any idiosyncrasy in an individual's imagery or associations that is not discovered in his behavior" (PPLT, 4).
(4) "Language is socially inculcated and controlled; the inculcation and control turn strictly on the keying of sentences to shared stimulation. Internal factors may vary *ad libitum* without prejudice to communication as long as the keying of language to external stimuli is undisturbed" (EN, 81).[9]

This thesis is naturalistic, says Gibson, in that it makes the study of

language accessible to empirical study, and it is behavioristic in that it relies on behavior as the substance of observable data. Gibson points out that the thesis has a substantive element: it makes a factual claim about how we learn language. It has also a heuristic element: it proscribes mentalistic theories of language learning and meaning so that the psychological mechanisms of language learning and any "meaning" not discoverable behaviorally may be safely ignored.[10] The NB thesis thus circumscribes the limits of semantics. It keeps the search for linguistic meaning within the scope of an empirical inquiry.

MENTALISM AND INDETERMINACY OF TRANSLATION

It is not only the physiology or neurology of verbal behavior that Quine's behavioral semantics leaves aside. As Quine wants to study meaning empirically, the correlative mental activity is also ignored, for he wants to deal only with what is observable. Thus, there is something residual over and above the physical and the behavioral that the naturalistic-behavioristic approach to language leaves aside on purpose: the idiosyncratic or intensional internal variations that are unobservable. "I reject introspection as an objective criterion, however invaluable heuristically."[11]

Thus, for Quine, data for semantics remain at the behavioral level *despite* the variant internal factors, *despite* the idiosyncrasy in individuals" imagery and associations, for Quine's semantics is empirical. These internal factors are the residues that are not captured by the observation of verbal behavior, or, as Quine puts it, we must take the verbal behaviors as their identity criteria—despite the loss of information that thus occurs—for there is no other way to individuate mental contents as such and to render them observable.

It is because this idiosyncratic residue is unobservable that, for an empiricist like Quine, the question of what a person *really* means is senseless, that the relations of synonymy are impossible to establish, that meanings or propositions are impossible to individuate, and that translation is indeterminate.

In other words, all the empiricist interested in meaning has to go on is what he can observe, all the data he has are what people actually say, and this is not enough to establish what they mean. It is not enough to establish, for instance, that when someone says "Snow is white" *he means* "La neige est blanche." It is not enough to say that

there is a mental entity (propositional meaning) that is what "Snow is white" and "La neige est blanche" have in common. All the empiricist can legitimately say is that "Snow is white" and "La neige est blanche" are usually asserted in the same observable circumstances. That does not imply a denial of the existence of the idiosyncratic/internal activity that goes on in the process of learning and of using a language, but only the acknowledgment that, in a rigorously empirical approach of meaning, we must leave aside the internal variations, and that the only data we have (all that is strictly speaking observable[12]) are verbal behaviors in publicly recognizable circumstances. And, as we saw, even though some of the internal factors (the brain states) can be observed, they do not tell us what people mean by what they say, they do not provide identity criteria for meanings or propositions, they do not allow the establishment of synonymy, and they do not determine translation.

Thus, to summarize, we see that the source of indeterminacy of translation lies in Quine's behavioristic approach to language, i.e., in his decision to ignore as data the internal factors, mental and neurophysiological. Neurology, genetics, and innate endowments are relevant only for the causal story about language, for identifying the causes of verbal behavior, but they are irrelevant for an empirical study of linguistic meaning. And as for introspection, Quine seems to attribute to it only a heuristic value.

THE POINT OF INDETERMINACY OF TRANSLATION

The indeterminacy thesis is, then, a means of expressing an important fact about language: the point of indeterminacy, as Quine has repeatedly said, is to challenge the notion of synonymy, and hence, the reification of meanings. To give weight to our conclusions, here are some passages in which Quine expresses explicitly the point of his thesis:

> [T]he indeterminacy of translation was always a conjecture, albeit a plausible one. It is a dismissal neither of translation nor of meaning. I have questioned the reification of meanings, plural, as abstract entities, and this not on the score of their abstractness, but of their individuation; for there is no entity without identity. Seeing mean-

> ing as vested primarily in the sentence and only derivatively in the word, I sought in vain an operational line on sameness of sentential meaning by reflecting on the radical translation of sentences.[13]
>
> The point of my thought experiment in radical translations was philosophical: a critique of the uncritical notion of meanings and, therewith, of introspective semantics. I was concerned to expose its empirical limits. A sentence has a meaning, people thought, and another sentence is its translation if it has the same meaning. This, we see, will not do.[14]
>
> My thought experiment in radical translation, in *Word and Object*, was meant as a challenge to the reality of propositions as meanings of cognitive sentences. Since there is no entity without identity, no reification without individuation, I needed only to challenge *sameness* of meaning of cognitive sentences. For pure sameness of meaning, unsullied by shared origins of words or mutual influences of cultures, where better to look than in radical translation?[15]

Apart from saying that the point of the indeterminacy of translation thesis is to critique synonymy and reification of meanings, we can also say, and it comes down to the same thing, that the point of the thesis is to tell us that we cannot determine what people *mean* independently of a chosen translation manual. For all that is observable is what people actually say, which we always translate (an arbitrary decision is made on our part) in our own ways of talking (whether different or the same). In other words, "[d]oubts about intensions come from reflecting on radical translation."[16]

> Translation is fine and should go on. "All of this" occasions no crisis in linguistics such as the antinomies occasioned in set theory. What "all of this" does occasion, if grasped, is a change in prevalent attitudes toward meaning, idea, proposition. And in the main the sad fact is, conversely, that "all this" escapes recognition precisely because of the uncritical persistence of old notions of meaning, idea, proposition. A conviction persists, often acknowledged, that our sentences express ideas, and express these ideas rather than those, even when behavioral criteria can never say which. There is the stubborn notion that we can tell intuitively which idea someone's sentence expresses, our sentence anyway, even when the intuition is irre-

ducible to behavioral criteria. This is why one thinks that one's question "What did the native say?" has a right answer independent of choices among mutually incompatible manuals of translation. In asking "But why should all of this occasion any surprise or concern?" Chomsky did not dismiss the point. He missed it.[17]

NOTES

1. Barrett, R. and Gibson, R., *Perspectives on Quine*. Oxford: Basil Blackwell, 1990, p. 4.
2. Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 365.
3. Shahan, R.W. and Swoyer, C. (eds), *Essays on the Philosophy of W.V. Quine*. Norman: University of Oklahoma Press, 1979, pp. 168–9.
4. Orenstein, A. and Kotatko, P. (eds), *Knowledge, Language, and Logic: Questions for Quine*. The Netherlands: Kluwer Academic Publishers, 2000, p. 417.
5. Quine, W.V., "Philosophical Progress in Language Theory," *Metaphilosophy*, 1, 1970, p. 4.
6. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 365.
7. As we saw in Chapter 2, when he seeks for explanation, Quine looks to physics ultimately, via physiology, biology, and chemistry.
8. Gibson, R., *Enlightened Empiricism: An Examination of W.V. Quine's Theory of Knowledge*. Tampa: University of South Florida Press, 1988, p. 9.
9. Ibid., p. 2. Quotations from *Word and Object*, *Ontological Relativity*, "Philosophical Progress in Language Theory" and "Epistemology Naturalized."
10. Gibson, *Enlightened Empiricism*, p. 2.
11. Orenstein and Kotatko, *Knowledge, Language, and Logic*, p. 418.
12. This excludes introspection.
13. Ibid., p. 409.
14. Quine, W.V., "Indeterminacy of Translation Again," *Journal of Philosophy*, 84, 1987, p. 9.
15. Orenstein and Kotatko, *Knowledge, Language, and Logic*:, p. 419.
16. Barrett and Gibson, *Perspectives on Quine*, pp. 198–9.
17. Davidson, D. and Hintikka, J. (eds), *Words and Objections: Essays on the Work of W.V. Quine*. Boston: D. Reidel Publishing Company, 1969, p. 304.

CHAPTER 6

INDETERMINACY OF TRANSLATION AND UNDERDETERMINATION OF THEORY

I have argued that the change from an emphasis on physicalism to an emphasis on behaviorism in Quine's argumentation for indeterminacy of translation is not a radical change, because the content of the thesis has remained the same, and because, although it was not what was emphasized in Quine's earliest argumentations for indeterminacy, the behavioristic approach to meaning was there from the start, and was what entailed indeterminacy anyway. Now, I want to add another reason why it seems that the change of emphasis from physicalism to behaviorism is not of consequence: in both cases, it remains the case that indeterminacy of translation is additional to underdetermination of theory. Quine himself points out that he has argued for the distinction between the two theses by appealing to a realistic view of nature (physicalism), but that he does not need to. In the following passage, he contrasts indeterminacy of translation with underdetermination of theory first by appealing to physicalism, and then without appealing to it:

> The indeterminacy of translation differs from the underdetermination of science in that there is only the natives' verbal behavior for the manuals of translation to be right or wrong about; no claims are laid regarding hidden neural mechanisms. If transla-

> tors disagree on the translation of a Jungle sentence but no behavior on the part of the Jungle people could bear on the disagreement, then there is simply no fact of the matter. In the case of natural science, on the other hand, there is a fact of the matter, even if all possible observations are insufficient to reveal it uniquely. The facts of nature outrun our theories as well as all possible observations, whereas the traditional semantics outruns the facts of language.
>
> In thus contrasting the underdetermination of natural science with the indeterminacy of translation I have taken a realistic view of nature [physicalism], which indeed I hold. But I have elsewhere drawn the contrast without the realism, in the following way. Natural science, we again assume, is underdetermined by all possible observation. However, suppose that we have settled for one of the many over-all theories of nature that fit all possible observation. Translation remains indeterminate, even relative to the chosen theory of nature. Thus the indeterminacy of translation is an indeterminacy additional to the underdetermination of nature.[1]

Translation remains indeterminate even under the supposition that the underdetermination of theory does not appear, i.e., even if we fix the theory of nature. Thus indeterminacy of translation is additional to underdetermination of theory. This remains true even under the supposition that the overall theory of nature is not physics. It suffices to suppose that the overall theory of nature is fixed, i.e., that we do not face underdetermination, to see that we still need to conclude to indeterminacy of translation, that indeterminacy is additional to underdetermination.

Thus, if the indeterminacy thesis derives from the fact that Quine is a realist about the theory of physics, and because physics (and hence reality) is irrelevant for deciding between two behaviorally equivalent manuals, indeterminacy is additional to underdetermination. But, even without identifying which theory of nature he adopts, even without saying that our theory of nature is physicalistic, Quine can maintain that indeterminacy is additional to underdetermination. For under the supposition that this as yet unidentified and not necessarily physicalistic theory of nature is fixed, under the supposition that we have chosen one of the overall theories that fit all possible observation, translation remains indeterminate. Translation remains indeterminate even in a situation in which we do not face underdetermin-

ation, for *in*determinacy is due to an enduring trait of language, to the nature of linguistic meaning. It is owing, explicitly, to the fact that there are no meanings as entities. It is not owing to the degree to which observation *under*determines our theories. Even if we had made all *possible* observations, and even if we had adopted a single theory that fitted them all, translation would still be indeterminate. For the nature of linguistic meaning remains the same. There would still be no meanings as entities.

Underdetermination of theory is a thesis about the relation between our theories and the evidence we have for them. It says that many logically incompatible theories can be built on a same observational basis. Here is Gibson's formulation:

> The doctrine of underdetermination of theory claims that theories about the world transcend all possible observations of the world, and, further, that different, competing theories can be developed on the same observational basis. In a word, theories can be shown to be logically incompatible with one another, yet empirically equivalent. "This is a point on which I expect wide agreement," says Quine, "if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary" (RIT, 179).[2]

Here is one of Quine's formulations:

> Here, evidently, is the nature of under-determination. There is some infinite lot of observation conditionals that we want to capture in a finite formulation. Because of the complexity of the assortment, we cannot produce a finite formulation that would be equivalent merely to their infinite conjunction. Any finite formulation that will imply them is going to have to imply also some trumped-up matter, or stuffing, whose only service is to round out the formulation. There is some freedom of choice of stuffing, and such is the under-determination.[3]

Underdetermination is thus a practical claim about what we humans can achieve in building theories. It is an epistemological claim, a claim about how theories are linked to observation:[4] the links between the theoretical talk and the observational talk are flexible and fragmentary; that is to say the observational talk *under*determines

the theoretical talk. In other words, there is freedom in the establishment of those relations, hence underdetermination: many competing theories can be built on the same observational basis.

Indeterminacy of translation is a different thesis. It is a thesis about the nature of language: we do not have identity criteria for meanings, thus we cannot reify meanings. There are no meanings as entities. This thesis has nothing to do with the evidential link between observation and theories, hence it has nothing to do with underdetermination of theories either. The claim that we have no identity criteria for meanings is not linked to the relation between evidence and theory. It concerns language generally, whether evidenced or not.

Indeterminacy of translation is a claim about the nature of language beyond and above underdetermination of theory. It is not an epistemological claim but an ontological claim. It does not intend to dismiss our capacity to translate, or to dismiss our meaning talk. It does not tell us that we cannot translate, but rather that when we do translate we do not do what many philosophers have thought we are doing, that is, we do not base our translations on the finding of entities—meanings—that are what the translated sentences have in common. In other words, when we do translate, we do not refer to entities—meanings. Thus, we see, indeterminacy of translation is a claim about what there is, or, rather what there is not: there are no meanings as entities.

Another way to show that indeterminacy of translation is an ontological claim is to note that translation manuals do not have an ontology. They do not make any suppositions about the furniture of the world. They just establish correlations between sentences. Thus, the furniture of the world (ontology), whatever it is, cannot determine the rightness or wrongness of translation manuals. That, of course, does not imply that the rightness of a single translation manual is not factual (Quine, as we saw, says that it is only the choice among two rival manuals that lacks factuality, and that the rightness of a single translation manual is a matter of fact[5]). However, it does imply that the rightness of a single manual is not a matter of ontology.[6]

In other words, we have data for translation but no entities. Translations are based on observation of verbal behavior in observable circumstances, and not on the reference to a denominator—an entity, the proposition or the meaning—that the sentences of the translator's language and the sentences of the native's language would

have in common. Translations are not based on the identification of entities (meanings) about which translations would be right or wrong. In other words, there is meaning but no meanings: "The point of my conjecture [about indeterminacy] is the unseating not of meaning but of the reification of meanings, primarily propositions. This I challenge by challenging the concept of sameness of meaning."[7]

On the other hand, scientific theories do have ontologies in addition to the data on which they are built. Let's compare the different kind of relations manuals and theories have to ontology. In the case of underdetermination—i.e., where we face two empirically equivalent incompatible theories—we can suppose that one theory is true and one theory is false even if both theories are equally warranted, for there are entities to be right or wrong about, so we suppose. In the case of *under*determination, then, the lack of determination comes from an epistemological lack, not from a lack of reality (entities). Indeed, underdetermination of theories derives from the fact that "the observational criteria of theoretical terms are commonly so flexible and fragmentary."[8] In other words, the lack of determination of theories does not result from a lack of entities about which theories could be right or wrong, but from the fact that the links between observational talk and theoretical talk are flexible and fragmentary.

In the case of indeterminacy of translation, however, the lack *or rather the absence of determination* is a lack of entities about which manuals could be right or wrong. Thus, even if there were no epistemological lacunae affecting the building of theories, even if the links between theoretical and observational talk were strict and complete, even if the theoretical talk was uniquely determined by the observational talk, there would still be indeterminacy of translation. There would still be no entities for translation manuals to be right or wrong about. When we evaluate scientific theories of the world, we can rest assured that there are entities underlying the data, but when it comes to the evaluation of translation manuals, we cannot thus transcend the data, for there are no entities (meanings) underlying the data. When it comes to translation, the data are all we have to go on. If we transcend them and decide that one of two manuals equally in conformity to the data is better than the other, our decision will be based on mere intuition, for want of meanings as entities determining translation uniquely. On the other hand, if we claim that one of two

incompatible empirically equivalent theories is true and the other false, we do so on an ontological basis, on the basis of what we think the furniture of the world is, on the basis of what objects we think there are. Indeed we choose the theory that best reflects what we think the world is.

NOTES

1. Quine, W.V., "Indeterminacy of Translation Again," *Journal of Philosophy*, 84, 1987, pp. 9–10.
2. Gibson, R., *Enlightened Empiricism: An Examination of W.V. Quine's Theory of Knowledge*. Tampa: University of South Florida Press, 1988, pp. 11–12. Quotation from Quine from "On the Reasons for Indeterminacy of Translation," *Journal of Philosophy*, 67, 1970.
3. Quine, W.V., "On Empirically Equivalent Systems of the World," *Erkenntnis* 9, 1975, p. 324.
4. We saw in Chapter 3 that Quine replaces the question of the link between observation and theory by the question of the link between our theoretical talk and our observation talk.
5. "[T]he conformity of a translation manual to speech dispositions is decidedly a matter of fact. It is only the choice between certain rival manuals that lacks factuality" (Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, pp. 459–60).
6. An idiom can be factual without having an ontology. It is the case of ordinary language, as we have seen in Chapter 3. Translation manuals are in the same situation. Their rightness is a matter of fact, but they do not have an ontology.
7. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 728.
8. Quine, "On the Reasons for the Indeterminacy of Translation," p. 179.

CHAPTER 7

CHOMSKY'S MISUNDERSTANDING

In the present chapter, my aim is to recall and criticize Noam Chomsky's denial of Quine's claim that there is an asymmetry between underdetermination of theory and indeterminacy of translation. Chomsky reads indeterminacy of translation as an epistemological claim. He sees indeterminacy of translation as a mere case of underdetermination. In short, he finds indeterminacy uninteresting because he thinks that it says only that the translator, like any scientist, goes beyond the data. But Chomsky fails to see that Quine's ontological point is the denial of meanings as entities. Moreover, he reads the notion of fact of the matter as an epistemological notion. He argues that there is no fact of the matter either to physics or to translation, for, he says, skeptical doubts hold in both cases. He says that both manuals and theories are underdetermined, and so both lack a fact of the matter. Let's address Chomsky's position and its problems in more detail.

In "Quine's Empirical Assumptions" (a chapter of *Words and Objections*) Chomsky takes part in the debate on whether or not there is an asymmetry between physics and translation in regard to underdetermination and indeterminacy. He denies that indeterminacy of translation is additional to underdetermination of theory. What I want to show here is that Chomsky is mistaken in rejecting the idea that there is an asymmetry between the two cases, and that the mistake is rooted in his understanding the notion of fact of the matter as an

epistemological notion and his reading of indeterminacy of translation as an epistemological thesis.

Chomsky does not argue that the indeterminacy thesis is false; he simply questions its importance. He defends the idea that it is uninteresting in itself because it is no different from underdetermination of theories:

> There can be surely be no doubt that Quine's statement about analytical hypotheses is true, though the question arises why it is important. It is, to be sure, undeniable that if a system of "analytical hypotheses" *goes beyond evidence* then it is possible to conceive alternatives compatible with the evidence, just as in the case of Quine's "genuine hypotheses" about stimulus-meanings and truth-functional connectives. Thus the situation in the case of language, or "common sense knowledge", is, in this respect, no different from the case of physics.[1] (Emphasis added)

Thus, for Chomsky, physics and translation are on a par. He concludes that neither translation nor physics has a fact of the matter.

> Interpreted in an epistemological context, as a claim about the possibility of developing linguistic theory, Quine's thesis is simply a version of familiar sceptical arguments which can be applied as well to physics, to the problem of veridical perception or, for that matter, to his "genuine hypotheses". It is quite certain that serious hypotheses concerning a native speaker's knowledge of English, or concerning the essential properties of human language—the innate schematism that determines what counts as linguistic data and what intellectual structures are developed on the basis of these data—will "go beyond the evidence". If they did not, they would be without interest. Since they go beyond mere summary of data, it will be the case that there are competing assumptions consistent with the data. But why should all of this occasion any surprise or concern?[2]

It thus seems that Chomsky concludes there is no fact of the matter either to physics or to translation simply on the grounds that manuals and theories are both underdetermined by evidence, that both go beyond the evidence. This, as we shall see, is a mistake. Gibson can lead us to the correct analysis:

> Føllesdal shares with Rorty (and Chomsky) the mistaken notion that Quine is using the expression "fact of the matter" in some methodological (i.e., epistemological) sense. But this is erroneous; Quine's understanding of this term is decidedly *naturalistic* and *physicalistic*. When Quine says that there is a fact of the matter to physics and no fact of the matter to translation, he is talking about physical facts, and he is talking from within an already accepted naturalistic-physicalistic theory.[3]

In order to discuss Gibson's rebuttal of Chomsky's reading, let me review how Gibson describes Quine's naturalism in *Enlightened Empiricism*. "Naturalism" has two usages, a negative one, and an affirmative one. In its negative usage, "naturalism" means the denial of a first philosophy, i.e., of traditional epistemology. This means the denial of the possibility of finding outside science the norms or truths that justify science. Quine's naturalism in its negative sense thus amounts to the claim that the project of founding science on a class of non-scientific truths is doomed to failure. Quine's argument for this claim is based on his holism, that is, the idea that many of the sentences of our scientific theories do not have their own unique empirical content.

If holism is true, then traditional rationalist philosophers who want to *deduce* all scientific truths from non-scientific a priori truths and norms must abandon their project. Indeed if holism is true, all we have access to is the empirical content of theories taken as wholes: it is impossible to differentiate between analytic (or other kinds of a priori) sentences and synthetic sentences. But empiricists who have defended a traditional epistemology must also abandon their project of a deduction or *rational reconstruction* of scientific discourse from certain a posteriori non-scientific truths and norms. On the doctrinal (justificatory) side of epistemology, the project of deducing scientific knowledge from immediate experience has been abandoned since Hume—even if we could couch scientific discourse in terms of observation alone, the problem of induction would remain entirely: "[t]he most modest of generalizations about observable traits will cover more cases than its utterer can have had occasion actually to observe."[4] On the conceptual side of epistemology, however, empiricists like Carnap still wanted to attempt a rational reconstruction of scientific discourse in terms of observation, logic, and set theory, for

at least, so they thought, scientific discourse would thereby gain in clarity. It would become as clear as the language of observation, logic, and set theory. Now, it is here that holism affects the empiricists' traditional epistemology. Indeed, if we add to holism a verificationist theory of meaning, we have to conclude that not only do many sentences fail to find their own unique empirical evidence, they do not even have their own unique meaning. Thus, theories must be taken as wholes. They cannot be translated sentence by sentence into the language of observation, logic, and set theory. Holism, we then see, informs Quine's argument for naturalism understood in its negative sense, i.e., as the denial of traditional epistemology.

But Quine has also used "naturalism" in an affirmative sense—to mean scientism. Scientism, for Quine, amounts to the claim that it is acceptable (not viciously circular) for epistemologists to use the findings of science to justify science. This time, Quine's argument is rooted in his realism, which he understands thus: "the robust state of mind of the natural scientist who has never felt any qualms beyond the negotiable uncertainties internal to science."[5] Whether we want to justify *or* criticize science, we are working within the framework of a scientific theory that we are taking seriously, that we are using to identify and describe reality. Thus, if Quine's realism is adopted, the new epistemologist's use of scientific findings to justify science is legitimate, for Quine's realism involves the acknowledgment that even skeptical challenges presuppose science itself. Skeptical doubts are themselves scientific doubts. Quine's point here is not to say that the skeptic's appeal to science is illegitimate, but rather that both the skeptic and the epistemologist need to make use of science.

Thus, Quine's holism serves to defend the negative usage of "naturalism" by helping to show that we must abandon the project of trying to find the foundations of science from a position of exile outside of scientific discourse. We always work within the framework of a scientific theory. Quine's realism, on the other hand, serves to establish the second, affirmative, usage of "naturalism," that is, the confidence in science (scientism). In addition to the acknowledgement that we always work within the framework of a particular theory, there is the acknowledgement that we take this theory seriously: it is within this theory that *reality* is identified and described.[6]

Now, in criticizing Chomsky's epistemological reading of Quine, Gibson cannot mean that Chomsky ignores Quine's naturalism alto-

gether. Indeed, Chomsky sees well the first and negative aspect of naturalism: he acknowledges the denial of a cosmic exile, he acknowledges that we always work within the framework of a particular theory. In fact, as the next quotation shows, Chomsky insists on this negative aspect of naturalism in his denial of the asymmetry between physics and translation. Indeed, Chomsky thinks that translation and physics are on a par because both the translator and the physicist work within a tentative theory of the world:

> What about the assumption that although in physics we may work within the framework of a tentative theory, in studying language (or learning language, or translating, or interpreting what we hear), this is not possible,[7] since it is impermissible to make general statements about language, or, more generally, about our "common sense theories", and since innate properties of the mind can impose no conditions on language and theories? ... It is difficult to see why this dogma should be taken more seriously than any other.[8]

We see that Chomsky does not deny Quine's naturalism altogether. He puts translation and physics on a par precisely because he wants to acknowledge the first aspect of naturalism: he insists on the fact that the translator is working within a theory of the world, just as the physicist is.

However, when he denies to physics a fact of the matter Chomsky shows that he misses the second, positive, aspect of Quine's naturalism. He misses the fact that Quine's naturalism is a species of realism, that it involves the idea that our experiences of nature are experiences of the real. In other words, Chomsky misses the fact that Quine's naturalism involves not only the assumption that we are always working within the framework of a particular theory, but also that it is within this theory that *reality* is identified and described (scientism). We consider the ontology of this theory as the actual furniture of the world. Thus what Chomsky has failed to acknowledge in denying a fact of the matter to physics is that Quine's naturalism involves the idea that the theory within which one is working fixes the fact of the matter, fixes the ontology of the moment. This mistake on Chomsky's part is a major one for, according to Quine, it is precisely in regard to the fixed ontology that translation is left indeterminate, whereas physics is not.

So, interpreting Gibson's critique of Chomsky, we can conclude that Chomsky misses one aspect of Quine's naturalism. He misses its ontological commitment, and thus wrongly takes the issue before him to be epistemological. This mistake takes different forms in Chomsky's commentary. We have just seen that he denies a fact of the matter to both physics and translation on the grounds that manuals and theories are both underdetermined by evidence. This is surely an illustration of Chomsky's epistemological take on the issue. There are other important illustrations of it, however, such as in the passage cited earlier where Chomsky attributes to Quine "the assumption that although in physics we may work within the framework of a tentative theory, in studying language (or learning language, or translating, or interpreting what we hear), this is not possible."[9] Here Chomsky accuses Quine of relying on the assumption that the physicist is working within the framework of a theory whereas this is not the case for the translator. In other words, Chomsky thinks that in order to defend the asymmetry Quine relies on the idea that the physicist and the translator do not have the same epistemological resources. In so saying Chomsky is not only mistakenly placing the debate in the epistemological arena, he is also saying something plainly false: Quine does not deny that the translator also works within the framework of a theory of nature. The following extract from Quine's reply to Chomsky demonstrates this:

> Though linguistics is of course a part of the theory of nature, the indeterminacy of translation is not just inherited as a special case of the under-determination of our theory of nature. It is parallel but additional. Thus, adopt for now my fully realistic attitude toward electrons and muons and curved space-time, thus falling in with the current theory of the world despite knowing that it is in principle methodologically under-determined. Consider, from this realistic point of view, the totality of truths of nature, known and unknown, observable and unobservable, past and future. The point about indeterminacy of translation is that it withstands even all this truth, the whole truth about nature.[10]

So Quine does not want to deny that the translator works within a theory of nature; indeed he says: "linguistics is of course a part of the theory of nature." He says that the translator *does* work within our

theory of nature, but that this theory is not an ultimate parameter for his translation enterprise. Indeed, he stresses that if the translator faces an indeterminacy additional to underdetermination of theory, it is because theory is an ultimate parameter in physics, whereas it is not an ultimate parameter in translation: "Where then does the parallel fail? Essentially in this: theory in physics is an ultimate parameter."[11] Why is the theory of nature—ontology—an ultimate parameter for the physicist but not for the translator? As we saw in Chapter 6, there are no objects behind the translator's data whereas there are objects behind the physicist's data. Ontology is indifferent to the choice between behaviorally equivalent translation manuals, but not to the choice between empirically equivalent theories. Thus *even if*, just as the physicist does, the translator works within the framework of a theory of nature, the ontology of this theory cannot decide between translation manuals that are equivalent in regard to the data.

So Chomsky is wrong in thinking that Quine does not provide the translator with the framework of a theory of nature, and he is wrong in thus situating the debate in the epistemological arena, in thus claiming that Quine does not provide both the translator and the physicist with the same epistemological resources. In the same spirit, Chomsky supposes that Quine needs to assume that the translator's *mind* is not endowed with the same properties as that of the physicist: "The particular assumptions that Quine makes about the *mental processes and structure* that provide the basis for human language learning are quite unwarranted, and have no special status among the many assumptions that can be imagined"[12] (emphasis added). Once again Chomsky seems to think that Quine has to suppose that the translator suffers from a lack of epistemological resources, and, in the present case, of mental resources. We saw that it is rather an ontological lack (no meanings as entities) that Quine points out in the translator's situation. Moreover, as we have seen in Chapters 4 and 5, Quine relies on behavioristic assumptions, not mentalistic ones.

Still in the same spirit, here is another illustration of Chomsky's epistemological reading of the issue:

> To understand the thesis [of indeterminacy] clearly it is necessary to bear in mind that Quine distinguishes sharply between the construction of analytical hypotheses on the basis of data and the postulation of "stimulus meanings of observation sentences" on the

> basis of data. The latter, he states, involves only uncertainty of the "normal inductive" kind. . . . The same is true, apparently, about the inductive inference involved in translation (similarly, "learning" and understanding) of sentences containing truth-functional connectives. In these cases, induction leads us to "genuine hypotheses", which are to be sharply distinguished from the "analytical hypotheses" to which reference is made in the discussion of indeterminacy of translation. Hence Quine has in mind a distinction between "normal induction", which involves no serious epistemological problem, and "hypothesis formation" or "theory construction", which does involve such a problem. Such a distinction can no doubt be made; its point, however, is less than obvious. It is not clear what Quine is presupposing when he passes over the "normal uncertainty of induction" as within the range of radical translation. . . . *It would then be necessary for him to justify the empirical assumption that the mind is natively endowed with the properties that permit "normal induction" to "genuine hypotheses", but not "theory construction" with some perhaps narrowly constrained class of "analytical hypotheses".*[13] (Emphasis added)

According to Chomsky, Quine's indeterminacy thesis relies on a distinction between two radically different kinds of hypotheses, a distinction for which he provides no empirical evidence. It is true that Quine provides no empirical evidence for the distinction; he does, however, provide arguments. More precisely, he provides his behavioristic view of linguistic meaning.[14] We saw in Chapters 4 and 5 that this approach to meaning leads Quine to the conclusion that there are no meanings as entities. It is because of this ontological vacuum that Quine thinks the translator frames a radically different kind of hypothesis: analytical hypotheses are not genuine hypotheses, not because of an epistemological lack in the translator's mind, but because there are no entities for those hypotheses to be right or wrong about, contrary to what is the case with the physicist's hypotheses. Thus, in contrast to what Chomsky claims, Quine is justified when he distinguishes two kinds of hypotheses and thus sees in translation an indeterminacy additional to underdetermination of theory.

A final illustration of Chomsky's epistemological reading of the issue may be found in the following passage:

> Interpreted in a psychological context, then, Quine's thesis of indeterminacy of translation amounts to an implausible and quite unsubstantiated empirical claim about *what the mind brings to the problem of acquisition of language* (or of knowledge in general) as an innate property. This claim seems to me of only historical interest. Interpreted in an epistemological context, as a claim about the possibility of developing linguistic theory, Quine's thesis is simply *a version of familiar sceptical arguments* which can be applied as well to physics, to the problem of veridical perception, or, for that matter, to his "genuine hypotheses".[15] (Emphasis added)

If this is what Chomsky thinks the indeterminacy of translation amounts to, we must remind him of Quine's arguments for the asymmetry. There are entities for the physicist to suppose lie behind his data whereas there are no entities for the translator to suppose lie behind his data. Such is the translator's situation, for there is no reality without entities (objects), no entities without identity, and the fact is that we do not have identity criteria for meanings. This is to be understood, as we saw, in the context of Quine's behavioristic approach to meaning. Acknowledging that context, we can respond to Chomsky by pointing out that Quine's claim is not "an implausible and quite unsubstantiated empirical claim about what the mind brings to the problem of acquisition of language" since it is backed up by arguments, and anyway is not even about the mind's contribution to the problem of acquisition of language. It is not even an epistemological claim. Neither is the claim "a version of familiar sceptical arguments," for, again, Quine's point is not to express doubts about our cognitive power, but about what there is: There are no meanings as entities.

It is ironic that Chomsky reproaches Quine for providing no empirical evidence for an indeterminacy additional to underdetermination of theory. For it is precisely because Quine sticks to a strictly empiricist approach to language that he adopts a behavioristic view of meaning, and thus concludes to his thesis of the indeterminacy of translation. Indeed, Quine's point is precisely that if we approach language and meaning from a purely empirical point of view, we will never be justified in reifying meanings, or to supposing meanings as entities.

To sum up: Chomsky supposes that indeterminacy of translation

derives from such epistemic facts as that analytical hypotheses go beyond the data, or that the translator does not work within the framework of a theory of nature. He suggests that indeterminacy of translation is either a claim about the mind's contribution to the problem of language learning or a version of familiar skeptical arguments. But he fails to see that indeterminacy of translation comes from a lack of reality. It is this failure that leads him to miss the asymmetry between indeterminacy of translation and underdetermination of theory. Gibson is thus right in tracing Chomsky's error to his epistemological conception of the issue.

NOTES

1. Davidson, D. and Hintikka, J. (eds), *Words and Objections: Essays on the Work of W.V. Quine*. Boston: D. Reidel Publishing Company, 1961, p. 61.
2. Ibid., pp. 66–7.
3. Hahn, L. E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 143. We saw in Chapter 4 that Quine has admitted that he does not need physicalism to argue for indeterminacy of translation.
4. Quine, W.V., *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969, p. 74.
5. Quine, W.V., *Theories and Things*. Cambridge, MA: The Belknap Press of Harvard University Press, 1981, p. 72.
6. Here I am paraphrasing Quine's own description of his naturalism: "naturalism: the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described" (Quine, *Theories and Things*, p. 21).
7. Chomsky writes: "Thus what distinguishes the case of physics from the case of language [for Quine] is that we are, for some reason, not permitted to have a 'tentative theory' in the case of language (except for the 'normal inductive case' mentioned above). There can be no fixed set of analytical hypotheses concerning language in general. We need a new set for each language (to be more precise, for each speaker of each language), there being nothing universal about the form of language. This problem, then, is one that faces the linguist, the child learning a language (or acquiring 'common sense knowledge', given the interconnection between these processes), and the person who hears or reads in his own language" (Davidson and Hintikka, *Words and Objections*, p. 62).
8. Ibid., pp. 65–6.

9. Ibid.
10. Ibid., p. 303.
11. Ibid.
12. Ibid., pp. 65–6.
13. Ibid., p. 61.
14. I have argued in Chapter 4 that the *source* of the indeterminacy of translation thesis (which *is* an ontological claim) lies in Quine's behavioristic approach to language.
15. Davidson and Hintikka, *Words and Objections*, p. 66.

CHAPTER 8

RORTY'S MISUNDERSTANDING

Quine defends the idea that anyone who thinks indeterminacy of translation is just an inherited special case of the underdetermination of theory—and thereby fails to recognize it as additional—must still be in the grip of the old notions of meanings, propositional attitudes, and the like. Rorty says that this claim is neither clear nor backed by arguments. I think that Rorty is wrong in denying the asymmetry between physics and translation, and that his major mistakes are those that Chomsky makes too; namely, reading indeterminacy of translation as an epistemological thesis and reading the notion of fact of the matter as an epistemological notion. In the present chapter, I provide several illustrations of Rorty's epistemological misreading.

The first and most obvious illustration may be found in Rorty's explicit exposition of what he takes to be the notion of fact of the matter: "What more does it take for there to be a 'fact of the matter' than a rational procedure for reaching agreement about what to assert?"[1] In the same vein, a second illustration lies in Rorty's explicit reference to methodology when he talks of fixing translation or synonymy: "[I]f we narrow down the sense of 'the methodology of analytical hypotheses' 'to those "canons" by which we in fact choose between otherwise "tied" manuals', then it is not at all clear that 'synonymous' remains indefinable."[2] Thus, according to Rorty, Quine provides the translator with enough tools to choose between rival translation manuals and to define synonymy. In other words, for

Rorty, people *do* establish synonymy relations, and when asked to justify them they will appeal to the method they have employed to arrive at their result. Rorty says: "[w]hen pressed for reasons, they will cite the usual behavioral facts and the sort of things which Quine calls the 'canons' of linguistics."[3] Thus, Rorty thinks that synonymy can be defined by reference to people's practices (presumably including those of the linguist), and thus that "the methodology of analytical hypotheses" *does* endow " 'same meaning with a sense.'[4]

The problem here is that indeterminacy of translation cannot be reduced or eliminated, synonymy cannot be established, by any reinterpretation of the method of analytical hypotheses, for, as we saw in the previous chapters, the absence of determination of the choice between behaviorally equivalent translation manuals is the absence not of a rational procedure to reach agreement but of relevant entities. The choice between behaviorally equivalent translation manuals is indeterminate, for there is no underlying reality to determine it.

A third illustration of Rorty's epistemological reading is the claim that, of course, the aforementioned rehabilitation of "same meaning" implies that we talk of an ordinary meaning of "meanings" and of "same meanings" and not of "meaning as conceived by certain linguistic philosophers". Thus, according to Rorty, Quine can make sense of "meaning the same" by reference to the methodology of analytical hypotheses if he grants the distinction between an ordinary meaning of "meanings" and a philosophical meaning of "meanings". And Rorty thinks that Quine does grant that distinction:

> [T]he only sort of meaning which Quine wants to say does not exist is "meaning as conceived by certain linguistic philosophers". In the "vegetarian" sense in which talking about "same meanings" is just shorthand for talking about the currently accepted translations, Quine does not want to deny that there are meaning*s* any more than he wants to deny that there are translations.[5] (Emphasis added)

> Meaning*s* (in the non-explanatory, vegetarian, [ordinary] sense) are as primordial a "posit" (to use another Quinean term) as are physical objects. The meaning of the foreign locution is what the bilingual knows and we don't, and doubtless men have talked about meanings as long as there have been bilinguals. The thought that *we* know what

> we mean, even if the witless barbarian does not, is doubtless equally old.[6] (Emphasis added)

Here Rorty betrays that he has missed the ontological point of Quine's indeterminacy thesis when he says that Quine does not want to deny meaning*s*. What Quine does not want to deny, as we saw in the previous chapters, is the possibility of meaning talk and of translation. However, for sure, Quine wants to deny that there are meaning*s*—*of any sort, ordinary or philosophical*—for he denies the individuation of meanings, he denies that we have identity criteria for meanings. This ontological point is the whole point of the thesis, as we saw in Chapters 4 and 5. To quote the relevant passage once again:

> [T]he indeterminacy of translation was always a conjecture, albeit a plausible one. It is a dismissal neither of translation nor of meaning. I have questioned the reification of meanings, plural, as abstract entities, and this not on the score of their abstractness, but of their individuation; for there is no entity without identity. Seeing meaning as vested primarily in the sentence and only derivatively in the word, I sought in vain an operational line on sameness of sentential meaning by reflecting on the radical translation of sentences.[7]

Fourth, Rorty thinks that Quine's defense of the asymmetry must be referring to the fact that what we know intuitively, those ordinary meanings, does not seem the kind of thing about which there could be alternative theories. Whence Rorty's interpretation of Quine's claim:

> *The "parallel but additional" indeterminacy which according to thesis (3) afflicts translation is not an extra, second, indeterminacy, piled on top of the usual "underdetermination of our theory of nature", but is simply a matter of this latter underdetermination turning up where we did not expect to find it.*[8] (Emphasis Rorty's)

Of course this reading of the asymmetry is wrong. Let's recall quickly the distinction between the two theses developed in Chapter 6.

*Under*determination of theory is the following doctrine: even if we had made all possible observations, logically incompatible theories could be built on the basis of those observations, for the links

between theoretical talk and observational talk are flexible and fragmentary. We are active and creative in the production of our theories; theories are not mere mirrors of the world. We need to do a certain amount of "stuffing" in building theories on the basis of observation. Underdetermination expresses the epistemological lack or slack between observation and theory that necessitates this stuffing. In the case of *in*determinacy of translation, however, the lack of determination is the lack of a kind of entity—meanings. We saw that the indeterminacy of translation thesis is a point against the reification of meanings. Thus even if there were no underdetermination of theory, even if the links between the theoretical and the observational were strict and complete, there would still be indeterminacy of translation. That is to say, there would still be no entities about which translation manuals could be right or wrong. We see then that indeterminacy of translation is not simply underdetermination of theory turning up where we did not expect it, as Rorty claims. It is additional.

Rorty's premises are, first, that Quine does not deny meanings in an ordinary sense of meanings, and second, that he can define "mean the same" by reference to the methodology of analytical hypotheses. These premises already indicate Rorty's epistemological reading, and they are wrong. They thus offer no support for Rorty's conclusion about the asymmetry between physics and translation—his view that indeterminacy of translation is in fact underdetermination of theory turning up where we did not expect it. Moreover, as we just saw, this conclusion taken on its own is radically incompatible with what Quine has written on both theses, namely that underdetermination of theory is an epistemological claim whereas indeterminacy of translation is an ontological claim.

Rorty acknowledges that some passages in Quine's work resist his interpretation. He cites Quine's reply to Chomsky, which we looked at in the previous chapter:

> Thus, adopt for now my fully realistic attitude toward electrons and muons and curved space-time, thus falling in with the current theory of the world despite knowing that it is in principle methodologically under-determined. Consider, from this realistic point of view, the totality of truths of nature, known and unknown, observable and unobservable, past and future. The point about indeterminacy of translation is that it withstands even this truth, the

> whole truth about nature. This is what I mean by saying that, where indeterminacy of translation applies, there is no real question of right choice; there is no fact of the matter even to *within* the acknowledged under-determination of a theory of nature.[9]

Rorty makes two attempts at interpreting this passage, and manages to conclude that, after all, there is no way of reading it that is actually problematic for his own position. The first attempt is the following:

> At first reading, this may seem to say simply that if you knew all about the elementary particles you would still have a free choice between "tied" translation manuals. So you would, but you would have the same choice between "tied" chemical and biological theories. There is nothing special about the case of linguistics. All that falling in with the current theory of physics does is to let you turn your back on "tied" theories of the ultimate constituents of matter.[10]

Here Rorty overestimates the scope of Quine's physicalism. Physicalism must be taken as a mere example. Quine says: "adopt *for now* my fully realistic attitude" (emphasis added). His point is not that "if you knew all about the elementary particles you would still have a free choice between 'tied' translation manuals," but rather that no ontology that you could adopt would help in choosing between "tied" translation manuals, for ontology is simply *irrelevant* to the choice between them. Nature does not favor one manual over the other. In other words, Quine's defense of the idea that indeterminacy of translation is additional to underdetermination of theory does not rest on his physicalism, for his point is not that there are no physical objects determining the choice between behaviorally equivalent translation manuals, but that there are no objects at all determining that choice. Quine says: "Where then does the parallel fail? Essentially in this: theory [ontology] in physics is an ultimate parameter."[11] The parallel fails because theory—ontology—is an ultimate parameter for physics but not for translation.

Rorty's second attempt at interpretation of Quine's reply to Chomsky is as follows:

> As a last attempt at interpreting this passage, we might take the following line: there are no truths about meaning in the "totality of

> truths of nature" because there is no such thing as meaning (just as there are no truths about witches because there are no witches). This will not help, for the only sort of meaning which Quine wants to say does not exist is "meaning as conceived by certain linguistic philosophers". In the "vegetarian" sense in which talking about "same meanings" is just shorthand for talking about the currently accepted translations, Quine does not want to deny that there are meanings any more than he wants to deny that there are translations.[12] (Emphasis added)

But this second interpretation will not do either. Here we can reply to Rorty as we did above: pointing out that Quine wants precisely to deny that there are meanings of any sort, philosophical or vegetarian, for what he denies is the individuation of meanings. To thus recall that Quine does not deny meaning but the individuation of meanings indeed helps in reading Quine's reply to Chomsky. For the reply comes to this: if there are no truths about meanings in the "totality of truths of nature," it is because there are no such things as meanings, in the first place. Thus Rorty's dismissal of the passage that is problematic for his interpretation is not warranted.

After having defended his own reading of the issue, Rorty argues against Quine's. As does Chomsky, Rorty says that Quine's defense of the asymmetry between physics and translation relies on a canon–law distinction:

> In the case of the linguists' canons Quine seems to treat unverifiability (unimaginability of revision) as signalling absence of truth-value. But if he is going to do this, he should do it across the board. If he does, plenty of physics, chemistry, and philosophy will turn out to be devoid of truth-value.[13]

In other words, Rorty asks (as does Chomsky) how Quine can distinguish substantive laws from canons given that he denies the analytic/synthetic distinction. As we know, Quine rejects the idea that it is because we fail to imagine how we could come to give up a widely shared belief that we must conclude to a new kind of truth (truth by virtue of meaning). The same sort of argument that Quine uses against the analytic/synthetic distinction can be used, Rorty says, against a canon–law distinction. For Rorty, unimaginability of revi-

sion is not a ground for such a distinction, just as it is not a ground for the distinction between different kinds of truth (analytic/synthetic).

Rorty is right in supposing that unimaginability of revision cannot serve as a basis for a canon–law distinction, but he is mistaken in thinking that Quine relies on unimaginability of revision to defend such a distinction. Rather, Quine relies on the fact that there is no object for the analytical hypotheses to be right or wrong about: "The point is not that we cannot be sure whether the analytical hypothesis is right, but that there is not even, as there was in the case of 'Gavagai', an objective matter to be right or wrong about."[14] Thus while Rorty is right in saying that unimaginability of revision does not ground a canon–law distinction, he is wrong in saying that Quine asserts the contrary. Quine does not rely on such an epistemological lacuna to distinguish analytical hypotheses from genuine hypotheses; he does not base his distinction on the fact that we cannot imagine how to revise our analytical hypotheses, but on the fact that there is nothing for them to be right or wrong about.

Finally, Rorty concludes with the following remark:

> If my argument is sound, the dilemma facing Quine is this: he should either give up the notion of "objective matter of fact" all along the line, or reinstate it in linguistics. On the first alternative, he can say that the notion of "being about the world", which the positivists used to explicate both "analytic" and "meaningless", was as empty as these latter notions themselves, and cannot survive in their absence. On the second alternative, he can say that the linguists discover "substantive laws" just as the chemists do, remarking merely that these discoveries are likely to hold few surprises.[15]

This dilemma shows that Rorty misses the fact that Quine's notion of fact of the matter is ontological, and must be understood in light of his robust realism. Quine cannot do as the first branch of Rorty's dilemma suggests. He cannot give up the notion of fact of the matter all along the line, for he is a realist. As a realist, Quine takes our experiences of nature as experiences of the real. He thinks that facts of the matter are constitutive of reality. It is also because of this realism that Quine cannot do as the second branch of Rorty's dilemma suggests, i.e., reinstate the notion of fact of the matter for the trans-

lator, for there is simply no reality of meanings: there are no meanings as entities that the translator can take as constitutive of reality, no meanings about which he can be a realist.

CONCLUSION

In conclusion, let me compare Rorty's and Chomsky's positions. They are both, as has been said, examples of an epistemological reading of the issue. But even if both authors reject the asymmetry, they arrive at different conclusions: Chomsky thinks there is no fact of the matter to either physics or translation, while Rorty thinks there is to both.[16]

Both Rorty and Chomsky read indeterminacy of translation as an epistemological claim, seeing it as a mere case of underdetermination. As we saw in the previous chapter, Chomsky finds indeterminacy uninteresting because he thinks that it says only that the translator, as does any scientist, goes beyond the data. Rorty thinks that indeterminacy of translation is only surprising. He thinks that the only asymmetry between underdetermination and indeterminacy of translation is that the latter turns up where we did not expect it. Moreover, both Chomsky and Rorty fail to see that Quine's ontological point is the denial of meanings as entities.

Both read the notion of fact of the matter as an epistemological notion. Rorty argues that there is a fact of the matter to both physics and translation on the grounds that there is a rational procedure to reach agreement in both cases. Chomsky argues that there is no fact of the matter to either one, for, he believes, skeptical doubts hold in both cases. On the grounds that both manuals and theories are underdetermined, he believes that both lack of a fact of the matter.

Rorty and Chomsky dwell on Quine's empiricism and are thus led to take an epistemological stance in addressing the asymmetry between underdetermination of theory and indeterminacy of translation. For his part, Quine relies rather on his naturalistic physicalism, which is ontological. A naturalist is a species of robust realist, one who accepts our experiences of nature as experiences of the real. A physicalist accepts the natural world as physicists characterize it. An empiricist believes that the only evidence there can be for anything, hence the only basis for holding any belief, lies in experience. For one who, like Quine, is an empiricist, a naturalist, and a physicalist all at once, nature is the realm of facts, physics determines what nature

actually consists in, and the empirical sets the limits to what reasons we can legitimately have for accepting the physicists' account of nature. The important lacuna in Rorty's and Chomsky's views of Quine is thus naturalism—Quine's robust realism. Without it, Quine's physicalism would be no more than a conjecture as to why our experience is as it is, and ontology would be little more than a corollary to epistemology, whence Rorty's and Chomsky's epistemological readings of Quine. But when the naturalism is taken into account, facts of the matter cease to be conjectural, are constitutive of reality, and stand as that-by-dint-of-the-existence-of-which true statements are true.

NOTES

1. Rorty, R., "Indeterminacy of Translation and of Truth," *Synthèse*, 23, 1972, p. 453.
2. Ibid., p. 447.
3. Ibid.
4. Ibid.
5. Ibid., p. 452.
6. Ibid., p. 449.
7. Orenstein, A. and Kotatko, P. (eds), *Knowledge, Language, and Logic: Questions for Quine*. The Netherlands: Kluwer Academic Publishers, 2000, p. 409.
8. Rorty, "Indeterminacy of Translation and of Truth," p. 450.
9. Davidson, D. and Hintikka, J. (eds), *Words and Objections: Essays on the Work of W.V. Quine*. Boston: D. Reidel Publishing Company, 1969, p. 303.
10. Rorty, "Indeterminacy of Translation and of Truth," p. 452.
11. Davidson and Hintikka, *Words and Objections*, p. 303.
12. Rorty, "Indeterminacy of Translation and of Truth," p. 452.
13. Ibid., p. 459.
14. Quine, W.V. (1960), *Word and Object*. Cambridge, MA: MIT Press, p. 73.
15. Rorty, "Indeterminacy of Translation and of Truth," p. 459.
16. More precisely, Rorty thinks that both positions make sense, but he has been arguing for the latter in his article.

CHAPTER 9

FRIEDMAN'S MISUNDERSTANDING

Friedman believes there are problems with Quine's claim that there is an asymmetry between underdetermination of theory of physics and indeterminacy of translation manuals:

> I . . . argue that Quine has not provided us with a reason for thinking that translation theory is undetermined (in the relevant sense) by the totality of physical facts. Quine has not provided us with a reason for thinking that linguistic theory is different from any other higher-level theory—like chemistry or biology—in this respect. In short, I try to show that one can accept Quine's physicalism without accepting the indeterminacy thesis.[1]

Friedman believes that Quine has not provided arguments for the asymmetry. What I want to show in the present chapter is that Friedman is wrong in this belief, and wrong, as were Chomsky and Rorty, in denying the asymmetry. However, Friedman's mistake is not the same as Rorty's and Chomsky's, i.e., is not rooted in an epistemological reading of the notion of fact of the matter. Friedman understands that the issue is ontological,[2] but still he misses the fact that Quine's point in proposing the indeterminacy of translation thesis is to argue against the reification of meanings. This mistake takes two forms in Friedman's article. First, Friedman claims that Quine's point is to propose the irreducibility of linguistics and

translation theory to physics. Second, he claims that, for Quine, indeterminacy of translation amounts to an inexplicability of linguistic facts by physical facts. Let's take the two cases in turn.

First, the idea that indeterminacy of translation means the irreducibility of translation to physical facts. Friedman writes:

> So from now on I will interpret "physical determination" as "weak or strong reduction" and interpret physicalism as the doctrine that all "respectable" predicates and theories must be weakly or strongly reducible to physics. Quine's complaint against linguistics and translation theory, then, is that they—unlike other higher-level theories—fail to be (strongly or weakly) reducible to physics.[3]

Friedman rejects the asymmetry between theories of physics and translation manuals because he thinks that translation might well be physically determined by (reduced to) physics, or, at least, that we have no evidence to the contrary:

> If Quine is to make a convincing case for indeterminacy, he must give us some reason to think that such non-behavioral facts [facts relating our uses of words to our internal (physiological) states, facts relating our uses of words to external physical objects] are not relevant to reducing translation theory and that, consequently, since behavioral facts do not suffice to determine translation, nothing does.[4]

Friedman thinks that non-behavioral facts might determine translation. Given his definition of physical determination, this means that translation could be reduced to physics. Friedman is right in thinking that the issue before him is ontological, but he is wrong in reading indeterminacy of translation as entailing irreducibility to physics. We have seen that Quine's ontological point with indeterminacy of translation is that there are no meanings as entities, that there is no individuation or reification of meanings; it is not that linguistic facts are irreducible to physical facts. Moreover, it is a mistake on Friedman's part to focus on Quine's physicalism in discussing indeterminacy of translation, for physicalism is in fact irrelevant to Quine's argumentation for indeterminacy of translation, as we saw in Chapter 4. Worse than that, the physicalism that Friedman invokes is not even

Quine's. Indeed, we saw in Chapter 2 that Quine's physicalism does not involve any form of reductionism.

Second, Friedman also says that Quine's point in the indeterminacy of translation thesis is to claim the inexplicability of linguistic facts by physical facts:[5]

> And even if non-behavioral factors do vary arbitrarily,[6] it doesn't follow that they can't be relevant to *explaining* linguistic behavior and physically determining linguistic theory.

> Thus, it seems to me that the central issue underlying the debate over whether there is a fact of the matter about translation is an empirical one—it is not something that can be settled by philosophical argument. The issue is whether there are non-behavioral facts which are sufficient to (weakly or strongly) reduce linguistics and translation theory to physical science. At best, therefore, what Quine is doing is betting on the future course of science. Quine is betting that science will not uncover such facts and that consequently only behavioral facts will prove *explanatorily* relevant. Quine's opponents are betting that science will uncover such facts.[7] (Emphasis added)

There is, in this passage, another mistake on Friedman's part. Indeterminacy of translation does not claim the inexplicability of linguistic behavior by physical factuality (or by any other non-behavioral factuality). Indeterminacy of translation is, as we have seen, a claim against the reification of meanings.

I think that the source of Friedman's mistakes lies in his misreading of the role played by behaviorism in Quine's treatment of indeterminacy of translation. Friedman rightly identifies behaviorism as the core of Quine's argumentation for indeterminacy of translation, but he wrongly thinks that Quine's behaviorism is intended to defend the irreducibility of linguistic facts to physical facts or the inexplicability of linguistic facts in terms of physical facts. Again, let's take both cases in turn.

First, Friedman seems to think that Quine's behaviorism claims that linguistic facts are *reduced* to or determined by behavioral facts. Friedman draws no distinction between reduction to and determination by such facts. "[T]he fact that we learn language on the *basis* of observable behavior gives us no reason to think that all linguistic

facts must be epistemically (or ontologically) *determined* by facts about behavior."[8] We saw in Chapter 5 how Quine's behaviorism does not assert that linguistic facts are determined by or reduced to behavioral facts. It simply asserts that linguistic facts *are* behavioral facts. Moreover Quine's behaviorism does not *deny* that linguistic (behavioral) facts might be determined by or reduced to physical facts. In fact it remains silent about both reductionism and determinism. Behaviorism provides the data for semantics. It tells the linguist who studies meaning where to *start*. It tells him where meaning is.

Friedman also seems to think that Quine's behaviorism claims that linguistic facts are *explained* by behavioral facts: "Quine is betting that science will not uncover such [non-behavioral] facts and that consequently only behavioral facts will prove *explanatorily* relevant"[9] (emphasis added). But, as we saw in Chapter 5, Quine's behaviorism does not intend to provide an explanation. On the explanatory side it welcomes genetics and neurophysiology. As we saw, it simply tells what the data for semantics are.

CONCLUSION

Thus, while Friedman rightly takes the issue before him to be ontological,[10] he wrongly thinks that the indeterminacy of translation thesis claims an irreducibility of linguistic facts to physical facts or an inexplicability of linguistic facts by physical facts. Indeterminacy is, rather, a claim against the reification of meanings. Friedman rightly thinks that in the attempt to *explain* linguistic behavior, nonbehavioral facts are relevant, but he is wrong in thinking that Quine's behaviorism serves to deny that. When it comes to *explanation* Quine welcomes genetics and neurophysiology. Quine's behaviorism does not say that the *causes* of linguistic behaviors are not to be found in the physical. It says that the only *data* for *semantics* are observable behaviors in observable circumstances.

With indeterminacy of translation, Quine addresses neither determinism, nor reductionism, nor explanation. On the other hand, Friedman is interested in those topics, and he associates them with the issue of indeterminacy of translation. I think this is why he cannot see that there is an asymmetry between underdetermination of theory and indeterminacy of translation. Indeed, Friedman reads "indeterminacy" as indicating a lack of cause or a lack of causal

explanation. Being guided by this take on the term "indeterminacy," Friedman rightly remarks that there are no reasons to think that linguistic facts could not have physical causes and hence a causal explanation. However, Quine's indeterminacy thesis denies none of that. Quine's use of "indeterminacy" indicates that we lack identity criteria and so lack individuation of meanings, but not that we lack causes or lack causal explanations. Friedman's mistaken take on the term "indeterminacy" is probably due to his overestimation of the import of Quine's physicalism for his position on indeterminacy of translation. As we saw in Chapter 4, Quine is partly responsible for this. For, at first, he himself argued for indeterminacy of translation by invoking his physicalism. On the other hand, Friedman's version of physicalism is so different from Quine's that Quine should not in the last analysis take the blame for Friedman's denial of the asymmetry.

NOTES

1. Friedman, M., "Physicalism and the Indeterminacy of Translation," *NOÛS*, 9, 1975, p. 356.
2. "Thus, it is clear that Quine intends to be making something more than an epistemological claim in his thesis of the indeterminacy of translation. He wants to say that not only is translation not determined by all our *evidence*, it is not even determined by all the facts there are" (ibid., p. 355).
3. Ibid., p. 359.
4. Ibid., p. 365.
5. This version of indeterminacy of translation shows that Friedman also sometimes falls into the trap into which Rorty and Chomsky fell: an epistemological reading of indeterminacy of translation. If indeterminacy of translation was an inexplicability of some sort, the thesis would be epistemological.
6. For Quine, Friedman recalls, "non-behavioral factors (internal physiological states, 'causal' relations to external objects, etc.) can vary arbitrarily as long as these conditioned relations are preserved" (ibid., 9, p. 367).
7. Ibid., p. 369.
8. Ibid., p. 366.
9. Ibid., p. 369.
10. Although he explicitly says we should read indeterminacy of translation

as an ontological thesis, Friedman sometimes seems to oscillate between an epistemological and an ontological reading. Interpreting, as he sometimes does, indeterminacy as claiming the *inexplicability* of linguistic facts by physical facts constitutes an epistemological reading of the thesis.

CHAPTER 10

FØLLESDAL AND GIBSON GET IT RIGHT

In the present chapter I will present and discuss Dagfinn Føllesdal's and Roger Gibson's views of the debate over the asymmetry between underdetermination of theory and indeterminacy of translation. I present both authors in the same chapter because I think that their positions, duly combined and mutually corrected, entail the right position. I will use Føllesdal's "Indeterminacy of Translation and Under-Determination of the Theory of Nature"[1] (1973) and his "Indeterminacy and Mental States" (1990)[2] and Gibson's "Translation, Physics, and Facts of the Matter" (1985).[3] As we shall see, unlike Rorty and Chomsky, Føllesdal agrees with Quine on the fact that there is an asymmetry between underdetermination and indeterminacy. However, in his article of 1973 he shares Chomsky's and Rorty's misreading of the notion of fact of the matter. In that article, he takes the notion to be epistemological. Gibson has pointed out that mistake on Føllesdal's part. However, Gibson seems to have gone wrong on another point where Føllesdal has got it right, that is, regarding the role of physicalism in the argumentation for indeterminacy of translation. Let me try to summarize their discussion.

In "Indeterminacy of Translation and Under-Determination of the Theory of Nature," Føllesdal describes the asymmetry between underdetermination and indeterminacy thus:

If, now, in translation, simplicity were a guide to truth, then trans-

> lation would be on a par with empirical theory. Translation would be underdetermined: several alternative translations would yield the required correlations of observation sentences etc. But translation would not be indeterminate, since one of the translations would be the true one.[4]

This passage makes of the difference between underdetermination and indeterminacy of translation a methodological difference, for it says that if it were not for the difference in the role played by simplicity in physics and translation, indeterminacy and underdetermination would be on a par. Gibson, in "Translation, Physics, and Facts of the Matter," quotes this passage from Føllesdal's article and criticizes it relative to its focus on simplicity, a methodological canon:

> So, on Føllesdal's reading of Quine, the difference between underdetermination and indeterminacy can be traced back to the different roles played by simplicity considerations in the two domains, physics and translation. This analysis of the difference leads Føllesdal to conclude that "indeterminacy of translation seems to follow from empiricism alone without the need for any extra dogma [of physicalism]" (Føllesdal, p. 296), and that "the indeterminacy of translation seems to be with us to stay" (Føllesdal, p. 300).[5]
>
> Føllesdal's account of the difference between physics and translation with respect to facts of the matter, given in terms of simplicity considerations, is certainly clever, but it is just as certainly not Quine's view. Føllesdal shares with Rorty (and Chomsky) the mistaken notion that Quine is using the expression "fact of the matter" in some methodological (i.e., epistemological) sense. But this is erroneous; Quine's understanding of this term is decidedly *naturalistic* and *physicalistic*. When Quine says that there is a fact of the matter to physics and no fact of the matter to translation, he is talking about physical facts, and he is talking from within an already accepted naturalistic-physicalistic theory.[6]

Gibson is right: the passage quoted from Føllesdal's article indicates that he makes a mistake in reading Quine, in interpreting "fact of the matter" in a methodological sense. However, Føllesdal's interpretation is less naïve than the passage suggests, for he *also* says:

> In the case of empirical theories, this failure of a pragmatic definition of truth did not deter us from defining truth (e.g. à la Tarski) in terms of our talk about the world. Why can we not do the same for translation? The answer is, I think—and here we are at the crucial point of the whole argument—that the only entities we are justified in assuming are those that are appealed to in the simplest theory that accounts for all the evidence. These entities and their properties and interrelations are all there is to the world, and all there is to be right or wrong about. All truths about these are included in our theory of nature. In translation we are not describing a further realm of reality, we are just correlating two comprehensive language/theories concerning all there is. This is, I think, the reason for the difference between theory of nature and translation and thereby for indeterminacy of translation.[7]

In this passage, Føllesdal shows that he understands that, for Quine, it is a lack of entity, and thus, an ontological lack, which explains why indeterminacy of translation is additional to underdetermination of theories. What, then, is Føllesdal's reading of Quine? Does he understand Quine's ontological point or does he read "no fact of the matter" as an epistemological notion, as Gibson suggests? Maybe Føllesdal is in fact proposing to Quine a different way to argue for indeterminacy of translation, for he acknowledges that his own reading departs from Quine's texts:

> The argument is not stated in this way in any of Quine's writings, but it seems to fit in well with what Quine says on this topic. When Quine says that in translation there is nothing to be right or wrong about, he is, it seems, not just stating an ontological dogma to the effect that there are no propositions or other intensional entities; although he sometimes says that he is only expressing a bias toward physicalism. It seems to me that Quine's position is more interesting if his ontological bias toward physicalism is regarded as a consequence of a more fundamental epistemological bias toward empiricism. As has been argued above, indeterminacy of translation seems to follow from empiricism alone without the need for any extra dogma.
>
> This completes the second *argument* for indeterminacy of translation. *The crucial part of it is . . . where I try to explain why in translation there is nothing to be right or wrong about.*[8] (Emphasis added)

In light of the analysis of the content of and the arguments for Quine's indeterminacy thesis offered in Chapter 4, we can make the following remarks on this passage from Føllesdal's 1973 article.

We can note a hesitation on Føllesdal's part. On the one hand, as the last paragraph of the quotation shows, he takes the claim that in translation there is nothing to be right or wrong about to be the crucial part of the argument for indeterminacy of translation. But this is not the argument; this is the (ontological) thesis of indeterminacy itself. On the other hand, in the first paragraph of the quotation, Føllesdal notes that the claim of indeterminacy *is* an ontological claim to the effect that there are no propositions or other intensional entities, and that it is a *consequence* of a more fundamental epistemological bias toward empiricism. In other words, physicalism does not need to be part of the argumentation. Empiricism suffices to entail indeterminacy of translation.

As long as we retain the first paragraph of the quotation, our hypothesis that Føllesdal is in fact proposing a different way of arguing for indeterminacy of translation appears to be right. In fact Føllesdal has proposed explicitly to Quine that he change his mind about the role of physicalism in the argumentation for indeterminacy of translation. Thus, Gibson was right in 1985 to say that, at first, Quine had argued that indeterminacy is a consequence of his physicalism, but the fact is that, later, Føllesdal brought Quine to acknowledge that physicalism is irrelevant to the argumentation for indeterminacy and that empiricism, or more precisely behavioral semantics, suffices to entail it. This subsequent part of the debate appears in *Perspectives on Quine* where we find Føllesdal's "Indeterminacy and Mental States" and Quine's reply to it, in which he acknowledges that physicalism is irrelevant for indeterminacy.

Before taking a look at the text, let me point out that the confusion between the content of and the arguments for the indeterminacy thesis that appeared in "Indeterminacy of Translation and Under-Determination of the Theory of Nature" disappears in "Indeterminacy and Mental States." Føllesdal's insight over the asymmetry between physics and translation gets better elaborated and more precise in the later article. Føllesdal expresses clearly his belief that the *content* of the indeterminacy thesis is an ontological claim to the effect that there are no entities to determine translation. He also expresses clearly what he takes to be the epistemological source of, or the main *argument* for,

indeterminacy of translation, and for the asymmetry between underdetermination of theory and indeterminacy of translation in Quine's philosophy—behaviorism applied to language and what he calls the MMM thesis, or, the thesis of man-made meaning:

> [T]he thesis of *man-made meaning* (the MMM thesis):
>
> MMM thesis: *The meaning of a linguistic expression is the joint product of all the evidence that helps learners and users of the language determine that meaning.* The crucial point here is that the meaning is a product of the evidence. Exactly what the evidence is, is left open in this formulation of the thesis, it is circular on that point. Let us therefore first look at that issue. By "evidence" I mean, as in science in general, all that helps to eliminate hypotheses. Given that we are empiricists, the evidence has to be sensory, it will not comprise telepathy, thought reading, etc. Even those who appeal to extrasensory sources of evidence would admit that it does not play any significant role in the establishment, learning and use of language. These three processes are all based on publicly accessible evidence, i.e., observation of behavior. Quine, Davidson and many others have discussed just what evidence we can make use of. However, we do not have to go into the details of this discussion.
>
> The thesis that meaning is a product of all this evidence follows ... from our taking the social, public, nature of language seriously.[9]

Føllesdal appeals to this MMM thesis in his discussion of the asymmetry between physics and translation. He explains that the difference between physics (science) and translation is as follows:

> The difference between translation and science, however—and here we come to the crux of the MMM thesis—is that while in science we are exploring a realm that is not of our own making, or at least not totally of our own making, this is not so with translation. In science, unless we are idealists, we think of the world as something that lies there waiting to be explored, even before we start our theorizing. With language, however, the situation is different. The various sounds and written marks in a language do not have a meaning before they become part of a language. And what makes them part of a language and gives them meaning is the use we make of them. Not the use I as an individual make of them, nor the use that I and

> my contemporary fellow men and women make of them, but the use that we and generations before us have been making of these expressions, to the extent that this use is public, accessible to fellow users of the language through ordinary everyday observation. The meaning of linguistic expressions is the sedimentation of all this linguistic behavior, all this publicly accessible evidence. And it is nothing more. Where the evidence leaves off, meaning leaves off. Hence the MMM thesis.[10]

An important element to note here is that Føllesdal shows that physicalism is not needed for the argumentation for indeterminacy of translation.

> The argument we have just been through makes indeterminacy a consequence of just two basic assumptions: the public nature of language, which we could also call behaviorism applied to language, and the MMM thesis. In the argument we did not have to make any assumptions for or against mental states, nor did we have to assume physicalism ... So, physicalism and these other considerations are not needed for the argument. There are of course other ways to approach indeterminacy, where physicalism plays a role ...[11]

And Quine approves: physicalism is not needed for the argumentation for indeterminacy of translation. It is in fact irrelevant:

> Dagfinn has illuminated the indeterminacy thesis by clearing away what does not pertain. What matters is just that linguistic meaning is a function of observable behavior in observable circumstances. Dagfinn divides this condition into two: that meaning is the product of the evidence by which it is learned, and that that evidence is public.
>
> Broader behaviorism is irrelevant; physicalism is irrelevant; monism is irrelevant.[12]

Thus Føllesdal makes it clear that indeterminacy of translation derives from Quine's epistemological bias toward empiricism, or more precisely, behaviorism in semantics. Quine has epistemological reasons to adopt the ontological position that the indeterminacy of translation constitutes. It becomes clear also that there is only an

ontological asymmetry between physics and translation and that there is no epistemological asymmetry between the two. Physics and translation are on a par epistemologically, as Gibson had already pointed out in his article of 1985: "physics and translation are on a par methodologically (i.e., epistemologically), but they are not on a par ontologically."[13] Indeed, Quine is an empiricist toward meanings just as he is an empiricist towards the rest of the world, in physics for instance. In fact, it is his empiricism toward language and meaning (i.e., his behaviorism in semantics) that leads him to the realization that there is an ontological lack (no individuation of meanings) affecting linguistic meaning and thus that there is an asymmetry between physics and translation.

So, it seems that Gibson was right about the (ontological) *nature* of the asymmetry although he was mistaken in arguing that physicalism is crucial to the argumentation for the asymmetry. As we have seen, Føllesdal in his 1973 article also made mistakes about the argumentation for indeterminacy. Among other mistakes, he took the idea that in translation there is nothing to be right or wrong about as an *argument* for the thesis, rather than recognizing it as the thesis itself. As we have seen, Føllesdal adjusted his position in "Indeterminacy and Mental States" by separating appropriately the ontological content of the thesis from its epistemological source. This has also enlightened Quine. Quine has acknowledged that the source of indeterminacy of translation and of the asymmetry between physics and translation is epistemological, and that it is not physicalism. Thus, it is to Føllesdal that the credit must go for having accomplished the last advance in the discussion over the asymmetry between underdetermination of theory and indeterminacy of translation.

NOTES

1. As Roger Gibson has claimed, this paper is Føllesdal's "direct response to Chomsky and Rorty. In particular, Føllesdal may be read as responding to Rorty's claim that Quine cannot consistently subscribe to a 'canon/law' ('heuristic'/'substantive') dichotomy. Føllesdal's answer is that a consistent Quine can do so, *if* he is willing to assign simplicity a role in physics different from the role assigned to it in translation, a notion Føllesdal seems to countenance even while admitting that '[t]he

argument is not stated in this way in any of Quine's writings, but it seems to fit in well with what Quine says on this topic' (Føllesdal, p. 296). As we have seen, according to Føllesdal, simplicity's role in physics is that of 'a guide to truth,' overriding almost every other consideration in our choice between specific scientific theories, while simplicity's role in translation pales by contrast, being itself overridden by such things as mere agreement. Hence, in Rorty's terms, physics discovers 'laws' while translation constructs 'canons', such is the work wrought by simplicity, according to Føllesdal" (Hahn, L.E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded, edn). La Salle, IL: Open Court, 1998, p. 143).

2. In Barrett, R. and Gibson, R., *Perspectives on Quine*. Oxford: Basil Blackwell, 1990.
3. In Hahn and Schilpp, *The Philosophy of W.V. Quine*.
4. Føllesdal, D. (1973), "Indeterminacy of Translation and Under-Determination of the Theory of Nature," *Dialectica*, 27, 1973, p. 295. In the same spirit: "[T]he view that translation manuals are just a species of empirical theories is deeply rooted, and to loosen its hold it may be helpful to note also the following difference between translation manuals and empirical theories: while simplicity overrides almost every other consideration in our choice between scientific theories, this is not so for translation" (ibid., p. 296).
5. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 143.
6. Ibid.
7. Føllesdal, "Indeterminacy of Translation and Under-Determination of the Theory of Nature," p. 295.
8. Ibid., p. 296.
9. Barrett and Gibson, *Perspectives on Quine*, p. 103.
10. Ibid., p. 104.
11. Ibid.
12. Ibid., p. 110.
13. Hahn and Schilpp, *The Philosophy of W.V. Quine*, p. 144.

CHAPTER 11

COGNITIVE SCIENCE'S DENIAL OF THE ASYMMETRY

INTRODUCTION

My aim in this chapter is to discuss and evaluate the denial of Quine's asymmetry on the basis of the promises of cognitive science. I place this discussion at the end of the book because I take it to be a substantial aspect of the present debate over Quine's asymmetry (although it is not always described as such). Leading advocates of this more recent denial of the asymmetry are defenders of the following position: since cognitive science is a successful science and since it posits mental states, there seems to be no more reason to refuse to accept those mental entities than to eschew any other theoretical entities, such as numbers or magnetic fields. Evidently, if this position were right, we would have reason to revise Quine's indeterminacy thesis and with it the asymmetry claim, for there would be good reason to believe that there are meanings as entities about which translation manuals could be right or wrong.

Here I will present this version of the denial of the asymmetry, and discuss two reasons to be skeptical towards it. Although both reasons deserve more attention than they can receive in this chapter, my objective here is not to advocate a definitive conclusion but rather to identify, articulate, and critically discuss two fundamental issues that I take to be crucial in any dialogue with the most recent opponents of the asymmetry. Note that what I am saying in the present chapter,

though quite congenial with Quine's point of view, extends well beyond any of Quine's arguments.

The first reason to be skeptical that I will focus on is the following: in its introduction of mental states cognitive science seems to encounter a major epistemological problem. For, it is not clear that such a science can obtain the kind of data it would need in order to be autonomous relative to neuroscience and to behavioral accounts. And in the absence of such data, it would seem to lack an identity criterion for mental states and processes. If I am right and this problem is real, we have a reason to be extremely cautious in introducing mental entities into our ontology based merely on the fact that cognitive science makes use of them.

Of course, the lack of data about mental states and processes is not an argument against the introduction of mental states and processes as theoretical entities, for theoretical entities are unobservables that we posit on the basis of their utility. However, the lack of data about mental states and processes is an argument against a science that purports to account for mental states as cognitive mentalistic *phenomena*. It is clearly legitimate to ask whether there are data about the mentalistic phenomena cognitive science claims to account for, and, if it is not evident that there are, we will have good reason to withhold judgment about cognitive science and its ontological commitments. Thus, a first reason to be skeptical about the legitimacy of the introduction of mental states and processes by cognitive science is the epistemological problem that the necessary data may be unavailable.

One could object that the lack of data for mental states is not a reason to bar them from our ontology, for we also have no data for numbers, for instance. However, this would be to misunderstand my claim. For my first suggestion is not that mental states can never be introduced into our ontology, nor that they cannot be introduced because we have no data about them. My proposition is simply that introducing mental states on the basis of their use by cognitive science is not ipso facto legitimate, and is sorely in need of further support beyond what the cognitive science community has thus far provided. Thus, the important epistemological problem encountered by cognitive science here is just that the availability of the kind of data it needs remains an unjustified assumption.

There is a second reason to be skeptical towards the introduction of mental states by cognitive science; one suggested, I think, by

Michael Friedman's *Theoretical Explanation*. Friedman, if I read him correctly, argues along the following lines: The simple slogan "to be real is to be admitted by an accepted theory" is too liberal to justify the introduction of theoretical entities unless supplemented by other considerations. In addition to that slogan, a specifiable further condition must be satisfied. Given an appropriately holistic view of science, an ontological commitment made by an admitted theory in a given field might well be rejected if it leads us in the direction of a *less unified* global conceptual scheme. I will argue that this is precisely the situation with mental states, namely that their postulation seems to be a step towards a less unified conceptual scheme. Importantly, to assign a high value to the development of a global, holistic view of science in addition to the slogan that "to be is to be posited in an admitted theory" allows us more critically to evaluate the merits of different kinds of theoretical entities. As Friedman claims, the degree of legitimacy of a theoretical construction increases with the number of different usages we make of it through science. If he is right, it becomes possible to differentiate the case of mental states from the case of numbers, to take just one example. If we grant that the legitimacy of a kind of theoretical entity must be measured from a relatively global point of view, the value of numbers appears enormous compared to that of mental entities.

Before I address these two issues in detail, let me recall the crucial elements of the previous discussion that will be needed in the present chapter, thus encapsulating Quine's case for indeterminacy/underdetermination asymmetry. Quine is a linguistic behaviorist and a scientific realist. His linguistic behaviorism is the view that a complete characterization of a language (a complete manual for translating sentences of that language into sentences of ours) is admitted simply on the basis of the linguistic behavior that is its empirical basis. So such a translation manual posits no extra-behavioral entities, and is not a theory of whatever mental activity may underlie the behavior in question. Nor indeed is it a theory of anything at all. Quine's scientific realism, on the other hand, is the view that scientific theories typically do posit entities beyond their empirical bases and that such entities exist once the theories that posit them are admitted as true. Now, the indeterminacy of translation is the fact that each of a pair of distinct but equally adequate translation manuals may be fully compatible with the totality of linguistic behavior that

constitutes a given language; whence the behavior does not uniquely determine the manual. And the underdetermination of scientific theory is the fact that two or more distinct and in fact incompatible theories are possible that square with all the same empirical data; whence the data do not uniquely determine the theory. There is thus a superficial symmetry between indeterminacy of translation and underdetermination of theory. For just as alternative translation manuals are possible for the same totality of (actual or potential) linguistic behavior, so alternative theories are possible accounting for the same totality of (actual or possible) empirical data. Behavior cannot uniquely determine manual, nor can empirical data uniquely determine theory. But the superficial symmetry here gives way to an ultimate asymmetry when this key difference is recognized: beyond the empirical behavioral basis, there are no further relevant facts, nothing to break a tie among alternative manuals of translation, nothing to make at most one of them right; by contrast, the tie between two incompatible scientific theories is simply a matter of our ignorance, for external facts of the matter do obtain which nominate one of the incompatible theories as true. It is thus, in brief, that Quine's argument for the asymmetry is structured. And its structure reveals why Quine's behaviorism renders indeterminacy ultimate and intransigent, while his realism insures underdetermination against a like fate.

THE DENIAL OF THE ASYMMETRY BASED ON THE USE OF MENTAL STATES BY COGNITIVE SCIENCE

One of the questions raised by many of Quine's opponents in regard to the asymmetry is this: why should we renounce the introduction of mental entities when we acknowledge the introduction of other kinds of theoretical entities? If to be real is to be admitted as such by the best theory of the moment, why need we avoid the introduction of mental entities, now that cognitive science makes use of them? William Bechtel argues along those lines. He claims that now that cognitive science proposes powerful theories that posit mental states, we should admit mental states into our ontology. The case of mental states, he thus supposes, is not different from the case of numbers or of magnetic fields.

In *Philosophy of Mind: An Overview for Cognitive Science*, Bechtel

directs our attention to the big picture with the following historical reminders and more recent cognitive-scientific arguments.[1] Following Brentano, some philosophers have argued that intentionality, or aboutness (the capacity to be about something else), is a feature that distinguishes mental phenomena from physical phenomena and which impedes a real science of mental phenomena. Mental states or events have the capacity to be about other states or events, whereas physical states or events do not have that capacity. Quine also rejects the idea of a scientific study of intentionality:

> Evidently, then, the relativity to non-unique systems of analytical hypotheses invests not only translational synonymy but intentional notions generally. Brentano's thesis of the irreducibility of intentional idioms is of a piece with the thesis of indeterminacy of translation.
>
> One may accept the Brentano thesis either as showing the indispensability of intentional idioms and the importance of an autonomous science of intention, or as showing the baselessness of intentional idioms and the emptiness of a science of intention. My attitude, unlike Brentano's, is the second. To accept intentional usage at face value is, we saw, to postulate translation relations as somehow objectively valid though indeterminate in principle relative to the totality of speech dispositions. Such postulation promises little gain in scientific insight if there is no better ground for it than that the supposed translation relations are presupposed by the vernacular of semantics and intentions.[2]

Bechtel tells us that most cognitive scientists reject Brentano's and Quine's positions, for they claim, contra Quine, to study intentional mental states, and to do so, contra Brentano, within a science that is continuous with the physical sciences.

> Although the final verdict is not yet in, the success of mentalistic theories that have been developed in cognitive science and the corresponding limitations of behaviorist approaches . . . would seem to be evidence that these theories have explanatory power in the same manner as other scientific theories and so should be treated in the same light . . . It is then incumbent upon us to explain how the intentionality of these mental states arises.[3]

For an old-fashioned empiricist, the question would arise immediately: how can we justify the introduction of mental states? The introduction of mental states within a naturalistic framework seems to require an argument, since mental states are unobservable. Indeed, the old-fashioned empiricist insists on strong evidence for a theory before adopting, embracing, or otherwise using it, and on regarding a theory as a good one to just the extent that its past credentials are strong. On the other hand, the pragmatist approach features tentative theory-adoption consisting in putting a theory into service to see how well things work out if one does, and counting those theories as good which are successful in terms of their consequences. Thus, roughly speaking, the older empiricist tends to put his money on the inductive method, the pragmatist on the hypothetico-deductive method. Quine—and most of the rest of us these days—are of course exponents of the latter a-theory-is-good-to-the-extent-to-which-it-works methodology.

Thus, a faithful Quinean could only conclude that it is what we have in the way of current theory that gives us no reason to posit mental states. If there were, one day, a theory that employed them and did a better job than other theories in the field, that would be reason to embrace them. It is an empirical question whether or not such entities are admissible. A Quinean cannot, in good conscience, rule them out a priori.

So, the old-fashioned empiricist seems to be reversing the order of things when he claims that we have to justify the introduction of mental states into our ontology. What we have to do is show that a theory employing them is an improvement on those that don't. The justification does not come first, but comes afterwards. The position is sensitive to empirical findings, not a priori.

It is in this pragmatist spirit that Bechtel claims that there are mental states, for, he believes, such states serve our scientific purposes well. As he notes, this supposes the denial of the asymmetry.

> Some have challenged Quine's account of the significance of the indeterminacy thesis itself by arguing that a decision to adopt a determinate translation manual and develop a theory of meaning for language within it is no different than the decision to accept a particular theory in a scientific discipline and work within it. Even though, as Quine maintains, there will be other theories empirically

> equivalent to the one we use, he allows that we are entitled in physics to accept one theory and work within it. If we treat the activities of translation and interpretation in a similar manner to theorizing in physics, then we should view postulating mental states to account for intentional phenomena as on a par with developing a theory in physics. The measure of adequacy of a mentalistic theory will be whether it serves our scientific purposes (e.g., explaining behavior). If it turns out that treating humans as having intentional states facilitates these ends, then countenancing such states will accord well with adopting a scientific attitude. . . .
>
> Quine, however, has steadfastly resisted this approach, arguing that the indeterminacy thesis establishes more than that mentalistic theories manifest the usual underdetermination true of all scientific theories . . . He claims that such theories are simply vacuous. Whether these theories are in fact vacuous, however, would seem to depend on their explanatory power. . . .
>
> Cognitive science seems to be in the process of developing powerful explanatory theories that postulate intentional mental states. So we seem to be faced with the challenge to see if we cannot explain the intentionality of mental states.[4]

According to Bechtel, there is no reason to treat translation manuals (and cognitive science) and theories in physics differently. Intentional mental states are useful theoretical entities for cognitive science, and, Bechtel thinks, in close analogy to Quine's physical realism, we must include them in our ontology.

Now, a careful look at the methodology of cognitive science suggests that, contrary to what Bechtel claims, it does not *postulate* mental states or processes to make better predictions of behaviors. As we will see, cognitive science rather uses behaviors (among other data) to try to *infer* what mental processes are used by the mind, i.e. what mental processes intervene between the input–output behaviors. In the remainder of the present section, I will try to show that cognitive scientists claim that their objects of study, the *phenomena* in which they are interested, are mental processes. Second, I will argue that behavior is not what cognitive scientists want to predict or study, but rather what they use to *infer* what they are interested in: i.e. mental phenomena. Let's take each point in turn.

The following characterizations of cognitive science by important

authors in the field show that it is commonplace in cognitive science to claim to study the mental phenomena, i.e., *what is going on in the mind between the input–output behaviors*. The following passages also make it clear that mental processes are not considered by cognitive science as mere useful theoretical posits allowing better predictions of observable behaviors. Rather, mental processes are considered as natural phenomena that deserve to be studied for themselves, scientifically.

Bechtel's and George Graham's characterization of cognitive science is as follows:

> The expression *cognitive science* is used to describe a broadly integrated class of approaches to *the study of mental activities and processes* and of cognition in particular. Cognitive science is broad not just in the sense of encompassing disciplines as varied as neurosciences, cognitive psychology, philosophy, linguistics, computer science, and anthropology, but also in the sense that cognitive scientists tend to adopt certain basic, general assumptions that the mind is (1) an information processing system, (2) a representational device, and (3) (in some sense) a computer.[5] (Emphasis added)

> A number of different phenomena comprise *mental activities and processes*. These provide areas of study for investigators within cognitive science—to name just a few: *attention, consciousness, imagery, language, memory, perception, and reasoning.* It is largely as a result of focusing on these common *phenomena* that cognitive scientists, though coming from different disciplines and using different research methods, interact with each other.[6] (Emphasis added)

Now, here is Joao Branquinho's characterization of cognitive science:

> Thus conceived, the field of cognitive science covers not only cognition proper, the usual paradigms of which are *mental events and processes such as conscious thoughts and inferences and intentional mental states like beliefs and desires*, but also any other *mental phenomena* in which information processing happens to play a central role. Hence, in so far as they can be subsumed as instances of information-processing activity (which in many cases is clearly the case), a variety of *mental states and events* traditionally grouped under the heading of "experience" and often contrasted *in limine* with cognitive states and events

> also fall within the scope of cognitive science. Among those states and events are notoriously sensations and perceptions, for instance visual experiences such as the experience of seeing a red object moving around in one's visual field; and auditory experiences such as the experience of hearing a piano sonata. In other words, both propositional attitudes—as philosophers call psychological states like belief and desire—and sensory experiences are part of the subject matter of cognitive science.[7] (Emphasis added)

The objects of study of cognitive science are clearly identified in Ernest Lepore and Zenon Pylyshyn's description of their goal at the Rutgers University's Center for Cognitive Science:

> The general goal, therefore, is to understand such cognitive capacities as perception, language acquisition and processing, planning, problem solving, reasoning, learning, and the acquisition, representation, and use of knowledge, in terms of the computational processes that underwrite these capacities, as well as their instantiation in silicon hardware or biological tissue.[8]

In *Cognitive Science: An Introduction* by Neil A. Stillings *et al.*, we can read:

> One of the most important intellectual developments of the past few decades has been the birth of an exciting new interdisciplinary field called *cognitive science*. Researchers in psychology, linguistics, computer science, philosophy, and neuroscience realized that they were asking many of the same questions about the nature of the human mind and that they had developed complementary and potentially synergistic methods of investigation. The word *cognitive* refers to perceiving and knowing. Thus, *cognitive science is the science of the mind.* Cognitive scientists seek to understand perceiving, thinking, remembering, understanding language, learning, and other *mental phenomena*.[9] (Emphasis added)
>
> Cognitive scientists seek to discover highly general and explanatory fundamental principles of information processing.[10]
>
> The cognitive scientist must cultivate a capacity to be puzzled by *mental phenomena* that occur without notice in everyday life. Often it is

> the most effortless cognitive activities, like understanding a simple word, recognizing the family cat, or planning a trip to the grocery store, that are the most complex and contain the most profound clues to the nature of cognition.[11] (Emphasis added)

In another introductory book on cognitive science, David W. Greens writes:

> We define the scope of cognitive science as the interdisciplinary scientific study of mind. . . . *It seeks to understand how the mind works in terms of processes operating on representations.* Mind, and hence the basis of intelligent action in the world, is viewed in terms of computations or information-processes. For example, in order to read this page you must be able to go from a printed word to its meaning. Retrieving the meaning of a word is a mental process. This process must make the meaning of the word or, rather, your mental representation of its meaning, available to a further process that constructs the meaning of the sentence and so on.[12] (Emphasis added)

In *Foundations of Cognitive Science*, cognitive science is characterized thus:

> Cognitive science, defined as the study of intelligence and its computational processes, can be approached in several ways. We can undertake to construct an abstract theory of intelligent processes, divorced from specific physical or biological implementations. We can study human (or animal) intelligence, seeking to abstract a theory of intelligent processes from the behavior of intelligent organisms. Or we can study computer intelligence, trying to learn the computational principles that underlie the organization and behavior of intelligent programs.
>
> In fact, cognitive science follows all of these paths.[13]

These characterizations suggest that the difference between cognitive science and behaviorism is not merely that the former postulates mental states in order to predict human behaviors, whereas the latter refuses such postulation. Cognitive scientists want to know how it works inside the black box. Mental entities and processes are not considered as mere theoretical entities (that are useful to predict

observable events such as behaviors) but as natural *phenomena* that are cognitive science's objects of study. Pylyshyn expresses that point clearly:

> Where do these observations leave a *science* of mind (e.g. cognitive science)? If you can't rely on introspection of your conscious experience to tell you what's going on in your mind, and if you can't rely on looking inside the skull using biological techniques to tell you what psychological processes are taking place, then how in the world *can* you tell? Of course, you can observe the organism in various ways in the laboratory. But if you are observing only the visible behavior—the input–output behavior—then can you distinguish among theories that produce the same input–output behavior? If the answer is no, then we are in trouble, because science is not interested in merely predicting input–output behavior. It is interested in the question: *how does it work*? And to say how it works is to do so much more than predict what output it will produce, given a particular input. At the very least, it is to specify the form in which representations are encoded and to give the algorithm by which the input–output function is computed in detail.[14]

From this point of view, shared by many cognitive scientists, behavior is interesting, but—and this is what I want to insist on now—not because it is what needs to be predicted, but rather because it is part of what is used to *infer* what goes on in the mind, between the input–output behaviors. Behavior, along with reaction times, number of errors, brain waves, and other data, is part of what cognitive scientists call *indirect evidence* of mental processes. Pylyshyn calls this intermediate-state evidence. Let's take a look at this kind of evidence.

One kind of indirect evidence is "protocle analysis": "[s]o here is one possible way to do better than merely trying to reproduce the input–output behavior that is observed in a laboratory. Ask the subject to tell you what he is trying to do, or what he is doing, or what he knows at various times during the process."[15] This method, Pylyshyn explains, can be used only to study relatively deliberate and slow processes (e.g. playing chess). We need to ask the subjects to "think out loud" while they perform the task. Pylyshyn acknowledges that this kind of information from the subject is not available when the processes under study are "rapid and highly fluent processes such as

visual perception." Moreover, he agrees that there is always the possibility that the subjects make errors in their report: they can be wrong about what processes they are actually using.

Another source of evidence in cognitive science is "relative complexity evidence." The use of reaction times is a good example of it: time is used as a measure of the number of steps of a mentalistic process. The underlying assumption is that the number of steps increases with the reaction time. Another example of relative complexity evidence is the number of errors. Then, the underlying assumption is that the more error-prone condition requires more steps.[16]

Cognitive scientists can also get evidence for "which of several possible processes are actually being used by finding certain measurable properties that we have independent evidence to believe are correlated with different operations, or stages."[17] An example of this is the correlation of brain waves of a certain type with the recognition of a stimulus, as opposed to the preparation to respond to the stimulus.

These are only examples of the sources of evidence cognitive scientists can appeal to. Other kinds can be found, but all have one thing in common, as Pylyshyn explains:

> There is no limit to the kinds of evidence that can be brought to bear on deciding what process is being used to derive a response. *In each case the method depends on an assumption about what the measurement is actually related to in the information processing*, just as we assumed that reaction time was related to number of operations. But the step from observation to theory always depends on such methodological assumptions, which have to be justified independently—and this is true in every science. In other words, there is nothing special about finding out how the process inside the black box works.[18] (Emphasis added)

The same conclusion about cognitive science's methodology is advocated by Paul Thagard:

> Much of *our inference from behavior to the underlying program* has to be indirect.
>
> But that is no cause for dismay. In this regard, cognitive psychology is not different from the other sciences, which are *always inferring*

underlying theoretical processes from gross observed events.[19] (Emphasis added)

Let's comment on these passages. First, recall on what basis the talk about mental entities and processes has been justified in the first place: despite their inaccessible character, we can make use of such entities and processes as *theoretical entities* insofar as they are useful, insofar as they help us to make better predictions of future observable events and account for past observable events. It is in that context that mental states and processes are legitimately *posited* (hypothetical). I insist on the fact that they are posited: they are theoretical entities, posits, that we use *in order to* account for the observable events. Now, it seems that some cognitive scientists reverse the order of things. What were supposed to be useful theoretical constructs have become phenomena. What is observable (behaviors, reaction times, number of errors, etc.) is what is used by cognitive scientists to account for mental processes which are no longer considered as mere useful entities but as *phenomena*. Instead of making hypotheses and using unobservable posits in order to account for observable events, observable events are used as evidence for what is unobservable. The observable events are used to *infer* what mental processes are used by the mind. The observable events are considered as *indirect evidence* of *mental phenomena*. I take it that good science uses theoretical unobservable posits to account for observable events, not the contrary, i.e. science does not use the observable events to *infer* unobservable phenomena such as mental phenomena.

Second, there is a problem with this notion of indirect evidence when it comes to mental states and processes. The claims that the observable events (behaviors, reaction times, number of errors, etc.) constitute evidences of mental phenomena are mere assumptions. This is considered unproblematic, as we just saw, insofar as these assumptions are justified *independently*. The problem is that it is impossible to justify this kind of assumption if *all* the evidences we have for mental states and processes are indirect, i.e., merely based on the *assumption* of a link between observable events and mental states and processes. Let's take an example. If we want to justify the assumption that reaction time is related to the number of mental operations, we need another kind of evidence of the number of mental operations and need to correlate it with reaction time. Let's

say that this other kind of evidence is the number of errors made by the subjects while they accomplish a task. This is, again, *indirect evidence* of the number of mental operations which relies on a mere assumption (according to which mental operations are linked to the number of errors). We can pursue and find as many indirect evidences of number of the mental operations as we want, but there will be no justification of any of their underlying assumptions if all that can be used as justifications are other assumptions. In order to justify any one of the assumed links between observable events (e.g. reaction time) and mentalistic phenomena (e.g. number of mental operations), we need a real correlation with mental processes at some point, which supposes, at some point, *direct* evidence of mental processes. In other words, if our assumptions about the links between certain observable events and certain mentalistic processes are justified by other assumptions of the same kind, they are not justified at all. All we can do with the observable events such as reaction time, number of errors, verbalizations, etc. is to correlate them among themselves. Claiming that they are correlated with mental processes is not justified until we have direct evidence of those mental processes. Let's put it otherwise. In order to grant a value to indirect evidence of mentalistic processes, we need to believe the following general claim to be true: "mentalistic processes manifest themselves in observable events (reaction time, number of errors, etc.)." This general claim remains a mere assumption until we find *direct* evidence for mental processes, until we establish a correlation between mental processes and observable events, *not merely between observable events that are merely assumed to be linked with mental processes.*

The indirect evidences cognitive science relies upon are not data that can justify the claim that it is a science of *mentalistic phenomena. Strictly speaking, cognitive science correlates observable events that are merely assumed to be linked to mental processes.* I will now try to explain why the data cognitive science needs in order to be an empirical science of the mind and of mentalistic processes, as it claims to be, are not available. My position takes the form of a dilemma: either cognitive science fails to provide an identity criterion for mental states and processes, or it is redundant relatively to other kinds of accounts.

FIRST REASON TO BE SKEPTICAL: AN EPISTEMOLOGICAL PROBLEM WITH COGNITIVE SCIENCE

If there were, one day, a theory that employed mental states and did a better job of explaining human behavior than other theories in the field, that would militate in favor of embracing such states.[20] But, as we saw, cognitive science purports to explain not human behavior but human mental phenomena such as human cognition, representation, etc. Thus, its success must be judged relative to its ability to predict those mental phenomena. I want to suggest that it is not obvious that this success can be evaluated since the availability of the kind of data cognitive science needs is not yet well established.

Within an empiricist framework, there is one kind of data that is rejected at the outset: mentalistic data. Since the mental *as such* is unobservable, there is no way we can claim to access it directly. The mind or mental properties or states cannot be directly observed. At best, they could be indirectly studied. This is what Bechtel suggests:

> One of the epistemic challenges confronting studies of both mind and brain is the indirectness of inquiry. The concern with the inaccessible character of mental processes was one of the factors leading the behaviorist B.F. Skinner to attempt to explain behavior totally in terms of observable stimuli and responses. . . . [W]e have no awareness of many of the mental processes psychologists are interested in (e.g. *how* we remember things or recognize objects we see as opposed to *what* we remember or see), and so any knowledge of these processes must be arrived at indirectly. Accordingly, over the last 150 years psychologists have developed a variety of indirect measures of the processes occurring in us.[21]

To suppose that we have privileged access to our internal states, or to make use of introspection, would not help in providing data for a science of the mental, for there is no prospect of comparing our subjective experiences.[22] In other words, in the scientific spirit, introspection must be rejected as a privileged means of obtaining mentalistic data. Introspection is no scientific method, for nothing can qualify as scientific data unless it is at least in principle available to all observers.

One way to get data about mental phenomena might be to reinvent materialism. If there is a way to conceive the mental as being somehow identical or at least linked to the physical, we could end up gathering data about the mind, in the course of gathering data about the brain. Bechtel tells us that "probably most cognitive scientists endorse materialism. Since the 1950s, however, philosophers have tried to state the thesis of materialism more precisely. As a result, they have developed a variety of different versions of materialism."[23]

To determine whether any of these versions of materialism makes mentalistic data available, let's see what data are made use of within their frameworks. Neurological or cerebral data are appealed to. We can observe the activity of parts of the brain and thus gather data about the brain. Cognitive scientists acknowledge, however, that in order to study cognitive phenomena, studying the brain *as such* will not suffice: "[m]erely looking at the brain is not informative; one must determine what processes occurring in brains are related to cognition."[24] We must bridge brain activity to cognition, and, to do so, we need data about cognition *per se*. Neurological/cerebral data are just about the brain, and so not yet about anything mental or cognitive. Thus, information about the brain as such is insufficient. It does not provide cognitive science with its own data.

According to Bechtel, the main challenge in providing data for cognitive science is the fact that we use instruments (built out of theory, as Hacking has explained): "[t]he attempt to link studies of the brain to psychological processes is critically dependent on a variety of instruments and research techniques."[25] Here Bechtel would seem to be mistaken. For, although the problem about theory-ladenness is real, it appears throughout science. It is not the lot of cognitive science and the neurosciences exclusively. Whether we use instruments or not, observation is theory-laden for everybody. The problem that is peculiar to cognitive science in regard to the availability of the data does not lie, then, in the need for instruments but rather in the fact that the proper kind of data is not available, whether or not we grant the use of theory-laden instruments.

The challenge is to show that cognitive science's data are available, but also to show that cognitive science is autonomous in regard to its data. If its data are totally identical with those of neurology, the need for cognitive science disappears, for it does not have its own realm of study. If its data are behavioral and neurological, it will not account

for anything mental but, at best, merely correlate behaviors with neurological activities. This is not, of course, what it intends to do, as this kind of correlation would not provide information about mentalistic processes. Thus, cognitive scientists must suppose that in observing the brain, they somehow get data about mentalistic/cognitive phenomena. They are thus looking for something that we could call "*physical-as-mental data*." In order for cognitive science to be a science, it must have data, and to differentiate itself from neuroscience, it must have data different from those of neuroscience. In order to be a materialistic theory *and* a mentalistic theory it cannot merely correlate brain activity and behavior, and it must provide at least a supervenient relation between the mental and the physical; for, otherwise, there will be no data about the mental.

Let me dwell on the second point for a moment. Cognitive science can claim to have access to physical-as-mental data only if there is such a relation. Kim Sterelny expresses this idea very clearly:

> Psychological theory had better not posit spooky causal processes, or devices with impossible causal powers. . . . Psychological properties ought to reduce to, or at least supervene on, the properties that are immediately causally involved in reactions to perceptual stimulation and in the production of behavior. Otherwise psychological explanation will invoke spooky causation. . . . [P]sychological properties do not reduce to neurophysical properties. But on this view, they should at least supervene on them: no psychological difference without difference in the intrinsic properties of brain states.[26]

This relation between the mental and the physical has been developed within the new versions of materialism mentioned above. The new versions of materialism in question are Mind–Brain Type Identity Theory, Eliminative Materialism, and Token Identity Theory.[27] Among them only two are candidates for establishing the relation between the mental and the physical and thus providing the relevant kind of data for cognitive science (physical-as-mental data). For, as its title suggests, Eliminative Materialism does not try to establish a link between the mental and the physical, but rather suggests that we eliminate the mental altogether.

Eliminative Materialists begin by claiming that neuroscience

> research does not demonstrate the correlation of brain processes with mental processes claimed by the Type Identity Theory and argue that this is a reason to replace mental talk with talk about brain states. More pointedly, they contend that there are no mental phenomena and that those who thought there were, were mistaken.[28]

As we are confined, then, to the other two versions of materialism, let us examine what they have to offer.

> The type/token distinction refers to the difference between a class of events (the type) and a specific member of the class (a token). The term *chair* identifies a type of object, whereas my desk chair is a token of that type. The Type Identity Theory holds that all instances of a particular type of mental state (e.g. experiencing a certain kind of pain or seeing a certain color) are identical to instances of a correlated type of neural event (e.g., a certain pattern of neural firings).[29]

This theory is highly controversial. Here is what Bechtel tells us about the controversy over Type Identity Theory, which he himself rejects: Type Identity Theory cannot defend a mere correlation between neural and mental states. There are indeed many problems with the claim that mental states are merely paired with brain states. First, there is no way to identify mental states, if they are not causally related to the physical states. Second, if mental states are merely correlated with brain states, they constitute a mere epiphenomenon with no causal power and are thus no part of the materialistic world of causal relations. But the claim of a strict identity is problematic too. Bechtel tells us that, if it means that we can use mental terms and physical terms interchangeably, then it is obviously false. If it means that mental activity *is* a form of physical activity, then the claim is a mere unverifiable assumption. The claim is still problematic if it means that what is truly predicated of physical events can be truly predicated of mental events, for it might be argued that there are properties that we cannot attribute to both the physical and the mental. For instance, some argue that intentionality is attributed exclusively to mental phenomena and that physical events only have spatial coordinates. Bechtel recalls that other authors have argued that we cannot identify mental events with physical events because of the way we access them: we find out about brain

states only indirectly, whereas we have privileged access to our mental life.[30]

Because of this major controversy, we can legitimately say, I think, that the Type Identity Theory has not yet established the availability of the kind of physical-as-mental data cognitive science needs; for, although the identity involved assures the reality of mental states, there is no way to obtain data about those states other than via this very identity supposition. This simply re-emphasizes the point that cognitive science has only cerebral data, and hence that cognitive science's realm of study does not appear different from that of neuroscience.[31] For both study the brain as brain, and nothing further.

Token Identity Theory is the surviving alternative to the Type Identity Theory. It proposes the following:

> Rather than repudiating mental discourse, Token Identity Theorists sanction its continued use by advocating a weaker version of the Identity Theory. They maintain that every token of a mental event is a token of a neural event, but do not require that types of mental events be equated with types of neural events. Thus, the Token Identity Theory holds that (a) every time I am in a particular mental state, that mental state is identical to a brain state, but (b) on other occasions when I am in the same mental state I may be in a different brain state.[32]

Bechtel is critical here also and acknowledges the importance of the controversy. Token Identity theorists claim to preserve our mentalistic talk (but, as we will see, it is not clear that they can, because of the resulting lack of an identity criterion for mental states). One of their options is to see the token identity as a *necessary condition* of any *materialistic* talk of mental phenomena. This is how Davidson sees things, according to Bechtel. However, because of the lack of laws relating mental and physical *descriptions*, Davidson has concluded that, although we can develop mental descriptions (and cognitive theories), we cannot claim that they are scientific. Both Putnam and Fodor have also adopted the Token Identity Theory but do not see this lack of laws as a problem: since "the classifications useful in psychology may be quite different than those useful for neuroscience,"[33] the absence of laws relating mental and physical descriptions does not mean that the mental cannot be studied within a science. Thanks to this autonomy

of sciences, cognitive science would find its niche. Bechtel writes:

> Token Identity Theory claims that there can be alternative, incompatible accounts of the internal activities of cognitive systems—one neural and one cognitive. Thus, of all the philosophical positions on the mind–body problem, Token Identity Theory is most compatible with the programs of cognitive science.[34]

According to this view, cognitive science is autonomous, for its accounts can even be incompatible with those of neuroscience. However, on this view, we lack an identity criterion for mental states. As Bechtel notes, "the Token Identity does raise the question of how mental events are to be categorized if this categorization is to be different from the categorization applied in brain events."[35] We can paraphrase the last sentence thus: the Token Identity Theory raises the question of how we are to get data about mental/cognitive phenomena if such phenomena are not identified in physicalistic terms. How will cognitive science get its data about the mental, if there is no link established between mentalistic and neurological descriptions? We need a means to identify the mental phenomena, to get data about the mental, but Token Identity Theory leaves us utterly without criteria for identifying mental states. To answer this challenge, "[a]dvocates of the Token Identity Theory have proposed that mental events be advocated functionally."[36]

> Functionalism represents a philosophical attempt to explicate a critical part of the research program of cognitive science—the way in which mental events are recognized and classified. Functionalism maintains that mental events are classified in terms of their causal roles. Thus, a mental event would be described in terms of its *role in the mental system* just as a cam shaft is characterized in terms of its causal role of controlling the opening and closing of valves in a car engine. *An important aspect of this approach is the claim that mental events can be recognized and classified independently of their physical constitution.* . . .
>
> Although all versions of Functionalism agree that mental states are to be identified primarily in terms of their interactions with one another, they differ mainly over how these interactions are to be specified.[37]

Bechtel himself argues for functionalism, but there is still much controversy about this version of the Token Identity Theory. Indeed, it is far from obvious that functionalism can provide the data cognitive science needs. It can be argued that we no more have identity criteria for causal roles in mentalistic systems than we have identity criteria for mental states.[38] Thus, the problem is still that we do not know how to characterize mental states in a way that provides an identity criterion for them and which would give cognitive science its autonomy.

It is not legitimate to go from the claim that mental states are useful theoretical entities to the claim that we have data about mental states. We have granted with Bechtel that we do not have direct access to mental states. In order for mental states to be identifiable, there must be a relation between them and physical/physiological states. We just saw that the theoretical attempts to establish that link are not yet convincing.

Cognitive science needs a specific kind of data. Behavioral data do not suffice, since cognitive science refuses the behaviorists' reduction: it sees mental phenomena as internal phenomena with causal power. Neurological or cerebral data do not suffice either, for, as we saw, cognitive science wants a bridge with cognitive processes. At best, with both kinds of data (cerebral and behavioral), cognitive scientists could correlate cerebral activities with behavior, but this is not what they want to do, for this kind of correlation says nothing about internal mentalistic activities. Contrary to what Bechtel suggests, the problem is not the use of instruments (which are theory-laden). The problem is that there is no way to identify mental states, even with instruments, unless they are identified by way of something that is, in principle, observable. However, when such an identification is made, cognitive science loses its autonomy and becomes no different from neuroscience pure and simple. We have seen that with the Type Identity Theory mental states get their reality from the identity claim, but cognitive science loses its autonomy relative to neuroscience and thus becomes redundant. With the Token Identity Theory cognitive science gets its autonomy back, but loses the crucial identity criterion for mental states. So, both theories fail in their different ways to provide the kind of *data* cognitive science needs. According to Bechtel, however, with functionalism cognitive science gets both autonomy *and* the availability of data. This is still controversial though. For how can it enjoy such a status when we no more have

identity criteria for mentalistic causal roles than we have identity criteria for mental states. Functionalism appears to flounder in just the same way that Token Identity Theory seems to.

Thus, the availability of the kind of data cognitive science needs still appears dubious to say the least. If that is right, if cognitive science is in such an awkward epistemological position, it seems prudent to be skeptical towards the ontological commitments that its introduction of mental states carries with it.

SECOND REASON TO BE SKEPTICAL

If there were to be a theory that employed mental states and did a better job of explaining human behavior than other theories in the field, then that might be a reason for embracing such states. But, as we saw, cognitive science purports to explain not human behavior but human mental phenomena, such as attention, consciousness, imagery, language, memory, perception, reasoning, etc. Thus, its success must be judged relative to its ability to predict those mental phenomena. I have suggested that it is not obvious that this success can be evaluated since the availability of data about mental phenomena is still a controversial issue. If that is true, a healthy skepticism regarding cognitive science's ontological commitments seems entirely warranted.

One could object, as briefly noted earlier, that if the lack of data about mental states militates against introducing them into our ontology, the lack of data about numbers, for instance, should militate against the introduction of numbers into our ontology. This would reduce my position to absurdity. However, I have not presented the lack of data about mental states as an argument against the introduction of mental states *per se*, but only against the legitimacy of the cognitive science on which many rely to advocate the introduction of mental states into our ontology. An important difference between numbers and mental states is that mental states are posited by a relatively recent and epistemologically problematic discipline, whereas numbers are posited by many different theories and more solidly established ones.

The numbers–mental states parallel is interesting, for it raises the question of the conditions under which we admit some entities into our ontology. In order to be taken as real, is it enough for an entity to be posited by a theory currently admitted as the best theory in the

field? It might not be taken to be enough if we hold a holistic view of science. I think that it is not enough, and that is my second reason to be skeptical about the introduction of mental states based on their usage by cognitive science.

Michael Friedman provides the key elements with which to defend this skeptical position. In "Theoretical Explanation" he is interested in how the procedure of theoretical explanation is linked to the postulation of theoretical entities.[39] What follows is my interpretation of those of Friedman's views that I take to be relevant to our present discussion. There are two extreme views towards theoretical entities that we need to avoid. According to the first, the *positivist* view, we should never believe in theoretical entities. Theoretical entities are mere "convenient devices for generating their actual empirical consequences. Indeed, apart from this role, theoretical entities have no 'physical reality' whatsoever."[40] According to this anti-realistic view, the relationship between theory and observation is one of representation and the theoretical entities posited by the theory have only a mathematical "reality."

The other extreme view consists in saying that we should admit as real the theoretical entities of a theoretical explanation "whenever it is the *best available* explanation."[41] This second attitude, Friedman argues, is too liberal and cavalier, for it implies that we must admit the theoretical entities of a theory whenever that theory is the only available explanation of some phenomenon, and thus even if it is a bad explanation.[42]

In order to avoid both extreme attitudes Friedman suggests that we focus on unifying power:

> I believe that the key to this account is a property of theoretical structure which has often been noticed in the philosophical literature, although not, I think, with the proper emphasis: namely, its *unifying power*. A good fruitful theoretical structure does not serve simply to provide a model for the particular phenomenon it was designed to explain; rather, in conjunction with other pieces of theoretical structure, it plays a role in the explanation of many other phenomena as well. . . .
>
> Now it is extremely important to see that the point of this kind of theoretical unification is not merely aesthetic, but also results in our picture of the world being much better *confirmed* than it otherwise

> would be. For a theoretical structure that plays a role in theoretical explanations in many diverse areas picks up confirmation from all these areas. The hypotheses that collectively describe the molecular model of a gas of course receive confirmation *via* their explanation of the behavior of gases. But, in addition, they also receive confirmation from all the other areas in which they are applied: from chemical phenomena, thermal and electrical phenomena, nuclear phenomena, planetary phenomena, and so on and so forth.[43]
>
> Suppose that the process of confirmation works something like this. Each time a theoretical hypothesis figures in the explanation of some observable phenomenon it receives a "boost" in its degree of confirmation. . . .
>
> [W]e should . . . accept a particular piece of theoretical structure when, and only when, it has received sufficient "boosts" in its degree of confirmation *via* its use in many *different* explanations.[44]

Using Friedman's theoretical framework and what has been said in the previous section, we can make the following remarks about the introduction of mental states by cognitive science. It seems hard to believe in the "world" of mental states that cognitive science creates, for several reasons. It is in order to explain mentalistic phenomena that cognitive science postulates mental states and processes. And, as we saw in the last section, the attempts to link those mentalistic phenomena to *other* types of phenomena (e.g. cerebral or behavioral) are still unconvincing and controversial. Indeed, it is not yet established how cognitive science could succeed in relating mental states to other kinds of phenomena. When mental states are different from neurophysiological states, they lack an identity criterion; and when they are identical with those neurophysiological states, cognitive science seems redundant relative to neuroscience. When mental states are described in terms of behavioral dispositions, cognitive science becomes redundant relative to the behaviorist accounts. Thus, the "world" of mental states created by cognitive science has up to now failed to play a unifying role and failed to get a boost of confirmation via its use in many different theoretical areas. Although mental states might appear useful theoretical entities from the point of view of cognitive science, they do not appear as such from a more global perspective. In fact, we can ask, as Friedman suggests, whether there is genuine explana-

tion (whether we really learn something new) when mental states are used to explain mentalistic phenomena, when there is no link established (otherwise than by mere assumption) between *different types of phenomena* that we had otherwise thought to be unrelated.

Friedman suggests that the best available explanation of a phenomenon may nevertheless be a poor account if it has no unifying power, or if it leads us away from an increase in unifying power. If he is right, we can say that even if cognitive science's theories of cognition were taken to be the best available for now, they are not good theories insofar as they lead us away from a unified conceptual scheme. The lack of unifying power of mental states is a point against the theory that posits them. Thus, instead of saying that we should admit mental states because they are posited by the *best available* theory of cognition, we should be skeptical towards the theory that posits them because they lead us away from an increase in unifying power. As we saw in the last section, mental states do not fruitfully interact with other theoretical structures. Nor do they get a boost of confirmation via the explanation of *different* phenomena, since mental states are posited to explain only mental phenomena.

Let's come back to the numbers–mental entities analogy. Numbers have great unifying power, for they fruitfully interact with other theoretical structures and receive a boost of legitimacy via their use in many different explanations. Mental states, on the other hand, have a more limited applicability. Numbers are used in all the disciplines that care to quantify, that is, disciplines in both the natural sciences and the humanities. On the other hand, mental entities are used in accounts of phenomena related only to human beings (though sometimes, depending on the mentalistic processes treated, related also to animals and machines). The contrast between the utility of numbers and mental states is obvious, but other cases can be found. For instance, the number of phenomena potentially explicable by cells and physical parts is much greater than the number of phenomena that mental states could explain. It surely counts heavily against postulating mental entities if doing so leads us towards a *less* unified conceptual scheme.

CONCLUSION

Quine argues that there is an asymmetry between the case of translation manuals and that of theories: whereas there are no entities

(there is no reality) about which translation manuals can be right or wrong, there are entities about which physicalistic theories, for instance, are right or wrong. One of the denials of this asymmetry is nowadays based on the alleged success of cognitive science. Now that cognitive science proposes theories that posit mental states, there might seem to be no reason to deny any fact of the matter, no reason to disallow a reality that would determine uniquely the choice among rival translation manuals. I have proposed two reasons to be skeptical towards the introduction of mental states. If these two reasons are not totally convincing to the advocates of cognitive science, they constitute, at least, an important basis for further discussion.

In one last recapitulation: The first reason to be skeptical towards the introduction of mental states by cognitive science is that it encounters a serious epistemological problem. This problem lies in the puzzle as to where data of the *kind* cognitive science needs could possibly come from. It seems that for mental states to be identifiable, they must be identified either with neurophysiological states or with behavioral dispositions. But either way, cognitive science lacks the autonomy its proponents claim for it. On the other hand, if the autonomy of cognitive science is granted, by distinguishing mental states from the neurophysiological and the behavioral, any identity criterion for mental states is thereby given up. If that is true, and the availability of the data needed by cognitive science is as dubious as I have argued, we are required to be most skeptical towards it and towards its ontological commitments. The second reason for such skepticism is that the introduction of mental states into our ontology seems to lead us away from a unified conceptual scheme. We have seen that mental states do not appear to fit well in our more global materialistic framework. So, if we agree with Friedman that genuine explanation requires the linkage of a variety of different phenomena that seem otherwise unrelated, mental states appear, at least thus far, to fall far short of the viability of the more robust theoretical entities central to science.

Finally, I want to lay stress on the fact that the reasons to be skeptical about mental states presented in this chapter are not *a priori*. They depend on empirical findings. In the first case I stress the unavailability of a proper kind of data for cognitive science. In the second, I focus on the empirical consequences of the postulation of mental states, arguing that these consequences must be relevant

across a large chunk of our conceptual scheme, not simply within those of a local theory or local set of theories such as those of cognitive science.

NOTES

1. Bechtel, W., *Philosophy of Mind: An Overview for Cognitive Science*. Hillsdale, NJ: Lawrence Erlbaum Associates, 1988. See especially Chapter 3.
2. Quine, W.V., *Word and Object*. Cambridge, MA: MIT Press, 1960, p. 221.
3. Bechtel, *Philosophy of Mind*, p. 52.
4. Ibid., pp. 52–3.
5. Bechtel, W. and Graham, G., "Preface," in W. Bechtel and G. Graham (eds), *A Companion to Cognitive Science*. Malden, MA: Blackwell Publishers, 1998, p. xiii.
6. Ibid., p. xiv.
7. Branquinho, J., "Introduction," in J. Branquinho (ed.), *The Foundations of Cognitive Science*. Oxford: Clarendon Press, 2001, p. xiii.
8. Lepore, E. and Pylyshyn, Z., "Preface," in E. Lepore and Z. Pylyshyn (eds), *What is Cognitive Science?* Oxford: Blackwell Publishers, 1999, p. vii.
9. Stillings, N. A., Feinstein, M. H., Garfield, J. L., Rissland, E. L., Rosenbaum, D. A., Weisler, S. E. and Baker-Ward, L. (eds), *Cognitive Science, An Introduction* (second edn). Cambridge, MA: MIT Press, 1987, p. 1.
10. Ibid., p. 5.
11. Ibid., p. 14.
12. Green, D., "Introduction", in D. W. Greens (ed.), *Cognitive Science, An Introduction*. Oxford: Blackwell Publishers, 1996, p. 5.
13. Simon, H. A. and Kaplan, C. A., "Foundations of Cognitive Science," in M. I. Posner (ed.), *Foundations of Cognitive Science*. London: The MIT Press, 1989, p. 2.
14. Pylyshyn, Z. W., "What's in Your Mind?," in Lepore and Pylyshyn, *What is Cognitive Science?*, p. 19.
15. Ibid., p. 20.
16. See ibid., pp. 21–2.
17. Ibid., p. 23.
18. Ibid.
19. Simon, H. A., "What Is an 'Explanation' of Behavior," in P. Thagard (ed.), *Mind Readings*. Cambridge, MA: The MIT Press, 1998, p. 22.
20. I will argue in the next section that even if a theory employing mental states did a better job of explaining behavior than other theories in the field, this might not yet warrant our embracing mental states.
21. Bechtel, W., Mandik, P., and Mundale, J., "Philosophy Meets the Neu-

rosciences," in W. Bechtel, Pete Mandik, J. Mundale, and R. S. Stufflebeam (eds), *Philosophy and the Neurosciences: A reader*. Malden, MA: Blackwell Publishers, pp. 15–16.

22. There is no way to compare our subjective experiences, unless we suppose that the verbal reports are reliable, that is, unless we suppose that there is no gap between what we say and what we mean, think, feel, desire, etc. This is the behaviorist reduction of the mental to behavioral dispositions, a supposition that cognitive science does not want to make since it purports to have its own autonomous realm of study.
23. Bechtel, *Philosophy of Mind*, p. 94.
24. Bechtel, Mandik and Mundale, "Philosophy Meets the Neurosciences", p. 16.
25. Bechtel, W. and Stufflebeam, R. S., "Epistemic Issues in Procuring Evidence about the Brain: The Importance of Research Instruments and Techniques," in Bechtel *et al.*, p. 76.
26. Sterelny, K., *The Representational Theory of Mind: An Introduction*. Malden, MA: Blackwell Publishers, 1990, pp. 81–2.
27. See Chapter 6 in Bechtel, *Philosophy of Mind*.
28. See Bechtel, *Philosophy of Mind*, p. 102.
29. Ibid., pp. 94–5.
30. See Chapter 6 in Bechtel, *Philosophy of Mind*.
31. I am using Bechtel's judgment about the compatibility of Type Identity Theory with cognitive science to address the problem of the availability of the data.
32. Bechtel, *Philosophy of Mind*, p. 107.
33. Ibid., p. 109.
34. Ibid., p. 111.
35. Ibid.
36. Ibid.
37. Ibid., pp. 112–13.
38. See, for instance, Fodor, J. A., "Fodor's Guide to Mental Representation: The Intelligent Auntie's Vade-Mecum," *Mind*, 94, 1985, p. 98.
39. See Friedman, M., "Theoretical Explanation," in Healey, R. (ed.), *Reduction, Time, and Reality: Studies in the Philosophy of the Natural Sciences*. Cambridge: Cambridge University Press, 1981, p. 2.
40. Ibid., p. 3.
41. Ibid., p. 5.
42. Ibid., pp. 5–6.
43. Ibid., p. 7.
44. Ibid., p. 8.

CHAPTER 12

CONCLUSION

CONTRIBUTION

With this book I hope to have contributed to the discussion about the asymmetry between indeterminacy of translation and underdetermination of theory in the following ways.

The analysis proposed in Chapters 2 and 3 was intended to help clarify Quine's notion of fact of the matter, which is not all that clear in Quine's texts. Apart from some explicit claims to the effect that the notion is ontological, physicalistic, and naturalistic, Quine does not provide his readers with a direct account of the notion. In order to grasp his notion of fact of the matter, readers have been obliged to make for themselves multiple links with the particularities of Quine's positions on physicalism, ontology, and naturalism. In addition to coping with that difficulty, readers trying to grasp Quine's notion of fact of the matter might wonder why Quine insists on talking of factuality and fact of the matter whereas he also claims to prefer talking about extralinguistic reality in terms of objects. Moreover, since Quine has said that factuality *and* truth are relative to a theory, his readers may wonder what difference Quine sees between ascriptions of truth and ascriptions of factuality, and what relation there is between the two. In Chapters 2 and 3 I addressed those difficulties and put together what I take to be the relevant information for a precise understanding of Quine's notion of fact of the matter. I have organized all of those elements to produce what I hope is a clear account of Quine's notion of fact of the matter and of its relations with some others of Quine's concepts.

In order to judge the contribution of Chapters 4 through 6, let me recall that, according to my reading of the debate, it is Føllesdal who has accomplished the last and most interesting progress in the discussion of the asymmetry between underdetermination of theory and indeterminacy of translation. So if someone wants to enter the debate he or she should pick it up where Føllesdal has left it. This, then, is what I attempted to do in Chapters 4 through 6. The account that I proposed in those chapters is in line with Føllesdal's. My *conclusions* are his. What my account adds is, first, a review of Quine's many variations in his formulations of indeterminacy of translation. I concluded that these variations are not of substantive importance, for the core of the thesis remains constant. However, the reader might judge that those variations are important enough to explain the confusion that has been cleared away by Føllesdal. Thus, my review of the variations within the formulations of the indeterminacy thesis is of value insofar as it contributes to an understanding of the confusion surrounding indeterminacy of translation and its asymmetry with underdetermination of theory. In Chapter 4, I also review many passages where Quine *argues for* indeterminacy of translation. In doing so, I add to Føllesdal's account the following elements. First, I provide textual evidence that Quine's argumentation moved on from an insistence on physicalism to an insistence on behaviorism exclusively, and I retrace the textual evolution of the argumentation. Second, and more importantly, I show that, although Quine's argumentation for indeterminacy changed in regard to the importance it assigned to physicalism, his behaviorism, the crucial element, was there from the start. Thus, I have concluded that this change in argumentation is not a drastic change, for despite the fact that behaviorism was not what Quine at first emphasized, it was part of the argumentation from the beginning, and was what really entailed indeterminacy. Even if he did not fully acknowledge it as such, the behavioristic approach to language already adopted by Quine was the source of the indeterminacy. Moreover, I point out that the change is not a drastic one because it is a change in the arguments for the thesis, not in the content of the thesis. Third, what I have tried to add to Føllesdal's clarification of the fact that only behaviorism (not physicalism) pertains to the issue, is an evaluation of what this acknowledgement means for the relations between neurology and semantics, and between mentalism and semantics. Finally, I also show

that, even if we must acknowledge that Quine changed his *argumentation* for the indeterminacy of translation thesis and its asymmetry with underdetermination of theory, he has defended the asymmetry itself from the start. Thus, clarifying Quine's arguments does not entail a change in the positions we must attribute to him. But Føllesdal's contribution is very important anyway, for greater clarity about the issues might provoke more fruitful discussions.

In reviewing critics' and commentators' views on the asymmetry in Chapters 7 through 10, I have sought, of course, to provide an overview of the major contributions to the discussion, but also to defend Quine against common misreadings of his texts. At the same time, I hope to have treated the texts of Rorty, Chomsky, Friedman, Føllesdal, and Gibson with due respect and attention, for I think that these texts, despite the criticisms I have made of them, all have the merit of indicating some weaknesses in Quine's texts. In other words, I think that, although it is possible to offer a solid defense of Quine against the critiques made by the previously mentioned authors, these authors, I suppose, did not make the mistakes they have made without good reason (in the next section, I will try to see where this supposition leads us). In Chapter 11 I discussed the denial of the asymmetry based on the promises of cognitive science, and I suggested some reasons to be skeptical about this denial.

CONCESSIONS

Perhaps, then, the commentators' readings of the notion of fact of the matter and of the indeterminacy thesis indicate points where Quine could have done better. Not that he could have been more precise on *the point* of his indeterminacy thesis, for, as we saw in Chapter 5, Quine has repeatedly and explicitly expressed his point. However, his treatment of the notion of fact of the matter itself could have been clearer. As I have argued, although Quine explicitly says that his notion is physicalistic, ontological, and naturalistic, one must have a deep understanding of his positions on physicalism, ontology, and naturalism in order to grasp what that means. For instance, as we have seen, Gibson has shown that, with their reading of the notion of fact of the matter, Chomsky and Rorty have overlooked Quine's naturalism. But one must appreciate that Quine's naturalism involves a robust realism, if one is to benefit fully from Gibson's rebuttal of Rorty's and

Chomsky's epistemological reading of the issue. Thus, in his treatment of the notion of fact of the matter, Quine has left to the reader the task of making for himself the links with physicalism, ontology, naturalism, and realism. I think that this is a huge task. I have asked his commentators for charity for Quine throughout the book. I take a few lines here to ask charity for those commentators themselves.

Another of Quine's moves that has contributed to the confusion surrounding the indeterminacy of translation thesis and its asymmetry with underdetermination of theory is the fact that, at first, Quine invokes physicalism in his defense of indeterminacy whereas, in the end, he acknowledges that physicalism is in fact irrelevant to this argumentation and that linguistic behaviorism suffices to entail indeterminacy of translation. The fact that Quine at first put the emphasis on physicalism (see Chapter 4) might have led such readers as Friedman and Gibson to overestimate the importance of Quine's physicalism in the debate over asymmetry.

Another element—which is not to be imputed to Quine but which has doubtless contributed to the confusion over asymmetry—is the fact that Quine has *epistemological* reasons for adopting the *ontological* position that the indeterminacy of translation thesis constitutes. Thus, even if it is a mistake to take the *content* of the indeterminacy thesis as being epistemological, philosophers such as Chomsky and Rorty, and maybe Føllesdal in his 1973 article, are not wrong in thinking that epistemological considerations are relevant to the *issue*. Indeed, Quine's arguments for indeterminacy are of an epistemological nature: he invokes the data available for semantics. Moreover, a close look at *some* passages where Quine defends indeterminacy of translation shows that the text can easily lead readers to think that the issue is epistemological. In "Epistemology Naturalized,"[1] for instance, Quine's defense of indeterminacy of translation does not mention ontology or fact of the matter. Quine rather uses expressions such as those italicized in the following passages (italics added):

> There will be no *justification* for pairing off the component English sentences with component Arunta sentences, except as these correlations make the translation of the theory as a whole come out right.[2]

> Insofar, there can be no *ground* for saying which of two glaringly unlike translations of individual sentences is right.[3]

> If we recognize with Peirce that the meaning of a sentence turns purely on what would count as *evidence* for its truth, and if we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of theoretical sentences is the natural conclusion.[4]

With expressions such as "justification," "ground," "evidence," the reader may easily be led to think that indeterminacy of translation expresses an epistemological lack rather than an ontological one. Thus, a reader who read only "Epistemology Naturalized" for an understanding of indeterminacy of translation would have little clue that what Quine has in mind is an ontological claim. That article rather emphasizes the verification theory of meaning. If the reader already realizes that the indeterminacy claim is ontological when he reads "Epistemology Naturalized," he will read the appropriate claim between the lines, but it must be conceded that not all of Quine's discussions of indeterminacy of translation are clear on the fact that the issue is ontological, much less on what that means. In other words, in the light of some passages of Quine's texts, it is easy to understand why some readers have mistaken the epistemological arguments for indeterminacy of translation for the content of the thesis itself.

INTEREST

Why and for whom does Quine's discussion of indeterminacy of translation and underdetermination of theory matter?

Underdetermination of theory and indeterminacy of translation should matter to scientists and philosophers. The asymmetry between the two theses should matter also, for one who does not see the asymmetry between the two theses simply does not grasp at least one of them. Underdetermination of theory should matter to any scientist or philosopher interested in the link between evidence and theory. As we have seen, underdetermination of theory is a consequence of the fact that the links between observation and theory are flexible and fragmentary. What about indeterminacy of translation? One might be tempted to say that indeterminacy of translation has a more limited interest, and that it matters only to scientists and philosophers who

are particularly interested in meaning. As we have seen, indeterminacy of translation appears in the context of the thought experiment of radical translation, and its point is to argue against the reification of meanings. Moreover, as Quine points out in the following passage, the *field linguist*, or the one with *broad* semantic purposes, is interested not in translation but rather in interpretation:

> Translation is not the field linguist's goal. His goal is to command the native language and perhaps to teach it, whether for reasons of ethnography and philology or simply to implement fluent dialogue and successful negotiation with the natives. His undertaking, broader than translation, is *interpretation*. An untranslatable sentence, such as the one about neutrinos, can still be interpreted, and that indeed is how we have learned it ourselves. For broadly semantic purposes, as Donald Davidson appreciates, interpretation is the thing. Translation is the narrower project, pertinent specifically to my concern over the fancied concepts of proposition and sameness of meaning.[5]

In this spirit, Quine explains, indeterminacy does not appear in practice, for, in practice, we apply the principles of interpretation:

> The indeterminacy of translation is unlikely to obtrude in practice, even in radical translation. There is good reason why it should not. The linguist assumes that the native's attitudes and ways of thinking are like his own, up to the point where there is contrary evidence. He accordingly imposes his own ontology and linguistic patterns on the native wherever compatible with the native's speech and other behavior, unless a contrary course offers striking simplifications. We could not wish otherwise.[6]

Thus, indeterminacy of translation concerns the interlinguistic case, and it does not obtrude in ordinary practice, for, in such practice, we rather interpret. Moreover, as we saw in the Introduction, indeterminacy of translation is interesting primarily in the context of the project of replacing traditional epistemology—based on translational reduction of knowledge to the language of observation and logico-mathematical auxiliaries—by a naturalized epistemology.

Thus, the lesson seems to be that, on the street, we can continue to

talk about meanings but that we must remember that meanings as entities cannot be seriously postulated within a scientific approach to meaning: "The word 'meaning' is indeed bandied as freely in lexicography as in the street, and so be it. But let us be wary when it threatens to figure as a supporting member of a theory."[7] For all these reasons, one can conclude that indeterminacy of translation has an interest that is limited to philosophical and scientific discussions about meaning.

There is, however, a consideration about indeterminacy of translation that should interest anybody who has even only broad semantic interests. It is the fact that indeterminacy of translation happens also at home:

> I have directed my indeterminacy thesis on a radically exotic language for the sake of plausibility, but in principle it applies even to the home language. For given the rival manuals of translation between Jungle and English, we can translate English perversely into English by translating it into Jungle by one manual and then back by the other.[8]

Thus, indeterminacy of translation is not only of interest in relation to the empiricist project of a translational reduction of all knowledge to the language of observation and logico-mathematical auxiliaries, or in relation to narrow scientific and philosophical talk about meaning, synonymy, etc. Indeed, we can not only present the point of the indeterminacy of translation thesis as a critique of translational reduction, synonymy, and the reification of meanings, we can also present it by saying that indeterminacy of translation shows us that we cannot tell what people *mean* independently of a chosen translation manual. For all that is observable is what people actually say, which we always translate; an arbitrary decision is made on our part, in our own ways of talking, whether we share the same language or not.

> Translation is fine and should go on. [My view] occasions no crisis in linguistics such as the antinomies occasioned in set theory. What [my view] does occasion, if grasped, is a change in prevalent attitudes toward meaning, idea, proposition. And in the main the sad fact is, conversely, that [my view] escapes recognition precisely because of the uncritical persistence of old notions of meaning, idea,

> proposition. A conviction persists, often unacknowledged, that our sentences express ideas, and express these ideas rather than those, even when behavioral criteria can never say which. There is the stubborn notion that we can tell intuitively which idea someone's sentence expresses, our sentence anyway, even when the intuition is irreducible to behavioral criteria. This is why one thinks that one's question "What did the native say?" has a right answer independent of choices among mutually incompatible manuals of translation.[9]

These considerations should interest even laypersons, for any layperson should be happy to do away with a misconception linked to communication, that is, "the stubborn notion that we can tell intuitively which idea someone's sentence expresses, our sentence anyway, even when the intuition is irreducible to behavioral criteria." Insofar as communication matters to them, even laypersons should be interested in the fact that we cannot tell intuitively just what idea someone's sentence expresses. The acknowledgment of the relativity of our attributions of what someone's sentence expresses to a choice among manuals of translation that might be many, and even incompatible, should induce in anybody a certain prudence in his or her expectations of communication. Indeterminacy of translation tells us that what people *mean* is not something, that it is not an *object* in our world; all we have access to is what people actually *say*. Thus, communication happens, but it might not be as "deep" as we ordinarily think. For there is no sense to the question of the quality of communication over and above overt behavior. As Quine says, "[w]ho is to say to what degree we talk past one another between checkpoints?"[10] Laypersons might be interested to consider that there is nothing more to communication than uniformity of verbal behavior, and that there is no exchanging of things like thoughts, ideas, or meanings in communication. Quine says:

> The uniformity that unites us in communication and belief is a uniformity of resultant patterns overlying a chaotic subjective diversity of connections between words and experience. Uniformity comes where it matters socially; hence rather in point of intersubjectively conspicuous circumstances of utterance than in point of privately conspicuous ones.[11]

These considerations, it seems to me, affect our ordinary conceptions of not only communication, but also of *understanding* and *expressiveness*. In fact, insofar as we read indeterminacy of translation as claiming the impossibility of individuating not only meanings, but also any other kind of intensional entities or mental contents, the ordinary conception of the *mind* itself is importantly affected.

It might seem an exaggeration to say that laypersons should be interested in indeterminacy of translation, even under the last description I have provided. Some might think that the abandonment of the ordinary conceptions of meaning and of the mind is too much of a sacrifice for laypersons. But saving the ordinary conceptions of meaning and of the mind requires that we postulate translation as determinate; yet the fact is, at least if Quine is right, that indeterminacy of translation is justified and determinacy of translation is not. Thus, one has the choice. Either one prefers to save the ordinary conceptions of meaning and of the mind, supposing wrongly that translation is determinate, or one accepts indeterminacy of translation and supposes that even one's non-philosopher fellows deserve the actual best account of the moment.

NOTES

1. In Quine, W.V., *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969.
2. Ibid., p. 80.
3. Ibid., p. 80.
4. Ibid., pp. 80–1.
5. Quine, W.V., *From Stimulus to Science*. Cambridge, MA: Harvard University Press, 1998, pp. 80–1.
6. Quine, W.V., *Pursuit of Truth*. Cambridge, MA: Harvard University Press, 1990, pp. 48–9.
7. Quine, *From Stimulus to Science*, p. 83.
8. Quine, *Pursuit of Truth*, p. 48.
9. Davidson, D. and Hintikka, J. (eds), *Words and Objections: Essays on the Work of W.V. Quine*. Boston: D. Reidel Publishing Company, 1969, p. 304.
10. Hahn, L.E. and Schilpp, P.A. (eds), *The Philosophy of W.V. Quine* (expanded edn). La Salle, IL: Open Court, 1998, p. 75.
11. Quine, W.V., *Word and Object*. Cambridge, MA: MIT Press, 1960, p. 8.

INDEX