

## “Plausible inssofar as it is intelligible”: Quine on underdetermination

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**Abstract** Quine’s thesis of underdetermination is significantly weaker than it has been taken to be in the recent literature, for the following reasons: (i) it does not hold for all theories, but only for some global theories, (ii) it does not require the existence of empirically equivalent yet *logically incompatible* theories, (iii) it does not rule out the possibility that all perceived rivalry between empirically equivalent theories might be merely apparent and eliminable through translation, (iv) it is not a fundamental thesis within Quine’s philosophy, and (v) it does not carry with it the anti-realistic consequences often associated with the thesis in recent debates. The paper analyzes Quine’s views on the matter and the changes they underwent over the years. A conjecture is put forth about why Quine’s thesis has been so widely misrepresented: Quine’s writings up to 1975 tackled primarily the formulation and justification of the thesis, but afterwards were concerned mostly with the question whether empirically equivalent rivals to the theory we hold are to be considered true also. When this latter discussion is read without bearing in mind Quine’s earlier formulation and justification of the thesis, his thesis seems to have stronger epistemic consequences than it actually does. A careful reading of his later writings shows, however, that the formulation of the thesis remained unchanged after 1975, and that his mature and considered views supported only a very mitigated version of the thesis.

**Keywords** Quine · Underdetermination · Empirical equivalence · Theory translation

Quine is widely regarded as one of the main proponents of the thesis that natural science is underdetermined by observations.<sup>1</sup> His views on the matter,

<sup>1</sup> See, for example, Sklar (1975, p. 379), Newton-Smith (1978, p. 71), Horwich (1982, p. 61), Worrall (1982, p. 202), Ariew (1984, p. 313), Bergström (1984, p. 349; 1993, p. 331; 2004, 91 ff.), Ben-Menahem

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however, changed several times over the years. He held one version or another of the thesis throughout his writings, but his formulations were revised at least twice, and at least three times he changed his mind about whether empirically equivalent yet rival systems of the world can be simultaneously thought to be true.<sup>2</sup> Quine's views are thus not easy to learn, and perhaps for that reason have been frequently misrepresented.

A common mistake consists in assigning to Quine belief in a thesis stronger than the one he actually put forth. This is not surprising, given his influence as a proponent of the thesis. One is rather naturally led to believe that it was somehow fundamental in his philosophy. Yet, closer inspection shows that within Quine's philosophy the thesis is not fundamental in any sense. The version of the thesis which Quine eventually settled for is too weak to support any strong metaphysical or epistemological doctrines, and it is neither obviously true nor is it clear what its implications are. It is nonetheless a plausible thesis, especially for an empiricist such as Quine; moreover, it is suggested by other doctrines within his philosophy, holism in particular.

The main goals of this paper are to lay out Quine's views on underdetermination as clearly as possible, track down the changes they underwent, and to try to understand why they have been so systematically misread in the recent literature. These are tasks which have only partially been undertaken in the literature, most notably by Gibson (1988 and 1998).<sup>3</sup> The intention here is both to complement existing expositions and correct some common misunderstandings. In doing so, a few implications of Quine's analysis of the thesis for the ongoing debates on underdetermination are explicitly stated. A secondary goal is to show that despite Quine's influence as a proponent of the thesis, his views contain a rather neglected alternative to the ones which currently prevail.

The paper begins with a brief introduction. Section 2 tracks down changes in Quine's formulation and justification of the thesis up to 1975. Section 3 focuses on later writings and Quine's vacillations on the truth of empirically equivalent rival theories. Section 4 concludes the paper with a summary and brief discussion of some of the ways in which Quine's views have been misrepresented.

## 1 Introduction

As mentioned above, Quine constantly upheld the thesis of underdetermination but changed his mind several times about its consequences and about how to best formulate it. While asserting the thesis, he often qualified it with phrases which indicate that he did not regard it as immediately clear or evident. He wrote, for example, that underdetermination is “slippery when we try to grasp it more firmly” (1975b, p. 80) and “plausible insofar as it is intelligible, but less readily intelligible than it may

Footnote 1 continued

(1990, p. 262), Laudan (1990, 271 ff.), Laudan and Leplin (1991, p. 449), Earman (1993, p. 31), Kitcher (1993, pp. 249–251), Hoefer and Rosenberg (1994, 593 f.), Kukla (1996, p. 139), Leplin (1997, p. 203), Yalçın (2001, *passim*), Stanford (2001, S8 n.), Devitt (2002, 31 ff.), Okasha (2002, p. 304), and Massimi (2004, p. 243). In fact, nearly all recent publications on underdetermination acknowledge Quine's contribution.

<sup>2</sup> See Quine (1955, p. 254; 1960, pp. 22–23; 1969, p. 302; 1970, pp. 178–179; EESW, 1975a, *passim*; 1975b, p. 79; 1981, pp. 21–22, 29–30; 1984, p. 294; 1998b, pp. 156–157; 1987, pp. 9–10; 1990b, pp. 13–15; 1992a, pp. 95–101; 1992b, p. 9).

<sup>3</sup> See also Bergström (1990, 1993, and 2004).

seem” (1975a, p. 313). None of this seems too characteristic of Quine’s work or style. This was a philosopher who throughout his life changed position very little about his core doctrines (holism, naturalism, empiricism), and tended to assert them quite forcefully. It is also unexpected that as proponent the thesis, he should be somewhat hesitant to assert its intelligibility. Thus, it might not be surprising that his vacillations on this matter and on the consequences of the thesis for realism have been interpreted as a symptom of a deeper tension within his philosophy, a tension between Quine’s naturalism (and the realism it inspires) and his empiricism.<sup>4</sup>

Also unexpected for someone regarded as one of the main proponents of the thesis is the fact that Quine’s views differ significantly from almost all others that have been recently published. Debates on underdetermination have been taking place quite independently of Quine’s writings, despite his acknowledged influence on its original formulations. In the recent literature, two major attitudes towards the thesis stand out: (i) Laudan and Leplin (1991), Kitcher (1993), Norton (1994), Massimi (2004), and others, challenge both the idea that rival theories can be empirically equivalent and the perceived entailment of underdetermination from empirical equivalence. According to these authors, currently held arguments for underdetermination rely on an oversimplified account of scientific practice, which is to be overcome by closer attention to the role of auxiliary assumptions and to actual historical cases of alleged empirical equivalence. (ii) On the other hand, van Fraassen (1980), Bergström (1993), Kukla (1996), Stanford (2001), and others, seem confident that underdetermination can be established (either by developing empirically equivalent rivals to any given theory through an appropriate algorithm, or by an inductive reasoning over the history of science). They maintain, accordingly, that because underdetermination is true, we should back down on at least some aspects of scientific realism: either theories (or at least their non-observable consequences) ought not to be thought as attempting to identify and describe reality but rather as tools for reliable predictions of observations, or they are to be thought as attempting to identify and describe reality but we are to remain always agnostic about whether that goal has been achieved. Attitude (i) is the one prevailing among epistemic *realists*. Attitude (ii) prevails among epistemic *anti-realists* and *agnostics*.<sup>5</sup>

All the above mentioned authors have in common the fact that they perceive underdetermination as somehow incompatible with scientific realism. Broadly stated, the thesis of underdetermination says that rival theories of the world can be equally warranted by observations. Quine says that those theories are “empirically equivalent”, meaning that they imply the same observation categoricals (1992a, pp. 16–17 and 95–96). All observations corroborating (or disconfirming) one of the theories likewise corroborates (or disconfirms) its rivals. Presumably they are irreducible to one another, otherwise there would be no sense of describing them as *rivals*. Accordingly, the set of entities and principles posited by each must differ somehow. Thus the

<sup>4</sup> Gibson (1986), Bergström (1993), and Yalçın (2001), for example, maintain that it is a symptom not merely of a tension in his philosophy but of an outright inconsistency—although Gibson is less critical in a later work (1988, pp. 113–124).

<sup>5</sup> Yet another view found in the recent literature is that of Sklar (2000), who focuses his attention not on philosophical theories or abstract methodological accounts of science but on actual scientific practices instead. His claim is that some of the concerns motivating underdetermination can already be found in actual scientific reasoning, and not just in philosophical reflection about that reasoning. He also claims that scientists typically refrain from drawing some of the broader philosophical consequences that one often finds in more abstract reflections about the methodology of science. In this regard, as we shall see below, Sklar’s views are similar to Quine’s.

world according to one theory would not be the same as the world according to its empirically equivalent rivals. Scientific realism enjoins us to interpret scientific theories literally, and to assume that the entities and principles posited by such theories are real, insofar as the theories are thoroughly confirmed by observations. Hence the conflict: empirically equivalent theories seem to stand on the same footing in regards to warrantedness; if we are to assign truth to one, there seems to be no reason to refrain from assigning truth to the others as well. But if taken at face value, as scientific realism enjoins us, it seems that at most one of any two conflicting theories can be true. It is thus thought that if underdetermination holds, either we must suspend judgment as to the truth of empirically equivalent but conflicting theories, or maintain that they do not literally depict reality but are merely tools for predicting sensory stimuli. Reactions to the thesis, accordingly, tend to fall into the two groups mentioned above: either (i) the thesis of underdetermination is false or confused, or (ii) it entails anti-realism or agnosticism.

Quine's views on underdetermination differ from both (i) and (ii). His views are unique in that he tends to downplay the theoretical significance of the thesis: "the more closely we examine (...) the less we seem to be able to claim for it a theoretical basis" (1975a, p. 326). He also seems quite reluctant to assign to the thesis any strong metaphysical or epistemological consequences. In fact, Quine did maintain the thesis while holding fast to realism, which by itself already indicates a divergence with the two attitudes described above. As we shall see below, there was quite a lot of vacillation in Quine's own views. But over time they led to a progressive mitigation of the philosophical significance of the thesis. In one of his last published treatments of the matter, he came to suggest that some of the considerations which motivate attitudes (i) and (ii) above would "simmer down, bathetically, to a question of words": "The fantasy of irresolvably rival systems of the world is a thought experiment out beyond where linguistic usage has been crystallized by use." (1992a, pp. 100–101).

## 2 “The doctrine is plausible insofar as it is intelligible”

Quine's most detailed and thorough analysis of underdetermination is his essay "On Empirically Equivalent Systems of the World" (1975a). Prior to that, he discussed the thesis only very briefly in various passages—for example, in "Posits and Reality" (1955, p. 254), and *Word and Object* (1960, p. 23). In those earlier texts, he neither argues for the thesis extensively nor goes into much detail about what it amounts to or how it can be spelled out. Rather, he seems to rely merely on its overall plausibility, which he takes to follow from the way the statements of natural science relate to observations: In a fully developed and theoretically complex scientific theory such as the ones currently held in physics, there is always a gap between scattered even if methodically collected observation reports, on the one hand, and the theoretical principles and entities which systematize those reports into a theory, on the other. In a typical modern physical theory, correct predictions of a large number of observable events can only be obtained when theoretical principles and entities are posited alongside observable entities. Evidence for those theoretical principles and entities can only be indirect, since it comes from the role they play in observable predictions. The connections that thus obtain between observations and the more theoretical portions of a theory remain always "less-than-rigid" (1955, p. 254), which makes it plausible to

think that alternative sets of theoretical principles and entities can be devised that allow for exactly the same predictions of observations.<sup>6</sup>

Quine's reasoning in "Posits and Reality" and *Word and Object* roughly follows the lines just sketched. The plausibility of underdetermination in Quine's thought is further strengthened by his description of scientific practice as generally conforming to the so-called 'hypothetico-deductive method', at least in regards to the testing of scientific statements.<sup>7</sup> Clusters of statements imply observation categoricals, which are then compared to observations. Alternative clusters of statements may in principle imply the same observation categoricals, and indeed do so with equal theoretical simplicity or economy.<sup>8</sup> Contrary to much that has been said of Quine in the recent literature, his reasoning on these matters is only conjectural, and his conclusion is not a categorical assertion of a positive thesis, but almost a concession in the absence of good reasons to think otherwise

... we have no reason to suppose that man's surface irritations even unto eternity admit of any one systematization that is scientifically better or simpler than all possible others. It seems likelier, if only on account of symmetries or dualities, that countless alternative theories would be tied for first place. Scientific method is the way to truth, but it affords even in principle no unique definition of truth. (1960, p. 23)

Rival, alternative theories could then be legitimately described as "empirically equivalent": Any evidence for one such theory would likewise corroborate the others.<sup>9</sup> They would agree on all observation categoricals and thus make exactly the

<sup>6</sup> See also Quine (1960, Sect. 6; 1981, *passim*; 1975b, pp. 74–75, 79; and 1992a, chapter 1).

<sup>7</sup> Norton (1994), Massimi (2004) and others have argued that the hypothetico-deductive method provides at best an impoverished representation of actual scientific practice, leaving unexplained the certainty with which currently accepted theories are upheld by scientists against relevant rival alternatives. They also argue that part of the plausibility of the thesis of underdetermination comes precisely from such an impoverished representation. In part, the point is well made against Quine. But we should note that Quine's main concern was with formulating a *general* description of the relations between theories and observations. It is only at that level of abstraction that Quine claims that the hypothetico-deductive method captures the essence of scientific practice. That remains true even if the construction of new theories and the choice between rival theories is constrained, as Norton and Massimi forcefully point out, by currently held hypotheses and practices in neighboring areas of science. Given those constraints, a choice may even seem to be *determined* by the available evidence. This is explicitly acknowledged by Quine in various passages (for example: 1992a, p. 18).

<sup>8</sup> This kind of reasoning is of course not original in Quine. Pierre Duhem was already arguing in a similar vein several decades earlier: "Shall we ever dare to assert that no other hypothesis is imaginable? Light may be a swarm of projectiles, or it may be a vibratory motion whose waves are propagated in a medium. Is it forbidden to be anything else at all?" (1954, p. 190) See also Poincaré (1905, chapter 10, especially pp. 167 ff.).

<sup>9</sup> For Quine, two theories are empirically equivalent if they entail the same observation categoricals (1975a, p. 319; 1981, p. 28; 1990b, p. 13). He seems to adopt here a view that has been very much criticized in the recent literature, suggesting that all evidence in favor of or against a theory has to fall within the scope of the observation categoricals that are entailed by the theory. Against that view, Laudan and Lepkin (1991), Massimi (2004), and others, have argued that evidence for a theory can also come from confirmed observation categoricals that are not directly entailed by the theory in question but by a more general theory, which in turn entails the theory in question. In such cases, direct evidence for the more general theory is also indirect evidence for the local theories that it entails. The point, though well taken, is only relevant where local theories are concerned. Quine's discussions of underdetermination, however, focus primarily if not exclusively on global theories, where indirect evidence coming from more general theories simply cannot obtain. His discussions are therefore

same observable predictions.<sup>10</sup> Yet they would diverge on the unobservable entities each posits or on the theoretical principles each asserts.

Room for divergences between empirically equivalent theories increases as science becomes theoretically more complex and sophisticated. Science has become an increasingly theoretical affair: currently held views on electrons and quarks, the principle of general relativity, and models for quantum theory, can provide some examples. For Quine, underdetermination can *only* obtain in such cases, where the internal structure of a theory is unavoidably complex and does not directly hinge on observations. Theories which posit no theoretical principles and entities cannot be underdetermined, unless by “rival theory” one has trivial linguistic variations in mind. Theory formulations indeed do have linguistic alternatives, but surely this is not what proponents of the thesis of underdetermination intend assert. If it were, then any theory formulation in, say, Portuguese and its English translation would count as alternatives, even if the translations were straightforward and unambiguous.

In his essay on empirically equivalent systems of the world (1975a, p. 323), Quine argues that theories which entail only a finite number of observation conditionals should not be regarded as underdetermined either. He maintains that rival alternatives can in such cases be altogether avoided by retaining in the original theory only the observation conditionals. The revised theory would be preferable on account of its simplicity. Such a revised theory would then only admit empirically equivalent rivals in a trivial and uninteresting sense. All non-trivial alternatives would posit theoretical entities and principles, and would be less preferable because of the unneeded complexity. The scientist can always, insofar as theory construction goes, opt for simplicity. Likewise, underdetermination would fail for theories that entail an infinite number of observation conditionals without thereby also entailing theoretical entities and principles.

#### Footnote 9 continued

conjectural, since we have never had an actual global theory of the world. The best candidate so far is physics, but even physics is yet to be unified. Quantum mechanics and general relativity are yet to be reconciled. Nevertheless, it is such a unified global theory that Quine has primarily in mind when discussing underdetermination. This is reasonably clear in several passages; for example: “Let us limit [the case] further to global systems of the world, so that there is no question of fitting the rival theories into a broader context.” (1992a, 98) Also, his use of the phrase “systems of the world” (1975a, p. 313; 1979, p. 66; 1998b, p. 155; 1990b, p. 14; and 1992a, pp. 95–102) throughout his discussions indicates that he has global theories in mind.

<sup>10</sup> In texts written in the 1970s and before, Quine uses the notion of ‘observation conditional’ rather than ‘observation categorical’. An observation conditional is a conditional sentence in which both the antecedent and the consequent are conjunctions of pegged observation sentences (1975a, p. 318). A pegged observation sentence is “a non-observation sentence obtained by pegging an observation sentence” (p. 328, n. 2) with space-time coordinates. Observation conditionals are thus standing sentences, “meant to be true or false independently of the occasion of utterance” (p. 316). In this respect, they differ from observation sentences, which command “assent on some occasions and not others, depending on what is happening where and when the sentence is queried” (*ibid.*). From the early 1980s on, Quine stopped using the notion of ‘observational conditional’, favoring instead that of ‘observation categorical’, which does not presuppose a space-time coordinate system, and is thus less theory-laden: “We can withdraw to what I may call *observation categoricals*—sentences like ‘Where there is smoke there is fire’ or ‘When it rains it pours’ or ‘When night falls the lamps are lit’. These enjoy generality over places and times, but they do not need to be read as assuming a prior ontology of places and times or any implicit universal quantification over them.” (1981, p. 27) The change in vocabulary, here, is not a significant change in doctrine, but an attempt to focus more narrowly on the pertinent issues and avoid unnecessary complications.

Quine thus explicitly allows for exceptions to the thesis of underdetermination, contrary to what some commentators have claimed.<sup>11</sup> On Quine's view, underdetermination can only obtain for theories that cannot do away with theoretical entities and principles

Here, evidently, is the nature of underdetermination. 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 underdetermination. (1975a, p. 324)

Quine makes this same point also in a slightly different context: In a response to Newton-Smith, he says that only “physical theory, the global theory of the world, is underdetermined, but not (...) every subordinate system” (1979, p. 66). Subordinate systems are partly determined from above, so to speak; they are selected depending how neatly they fit whichever global theory is adopted at the moment. Moreover—and this is where his response to Newton-Smith connects to the passage from 1975a just quoted—unmanageably large formulations might be needed to avoid positing theoretical entities and principles. Although a global theory of the world may be expected to entail an infinite number of observation conditionals only by positing theoretical principles and entities, the subsystems that make up the global theory may be simple enough to avoid them altogether.<sup>12</sup>

Quine's initial writings on underdetermination claim that the thesis is rendered plausible by the fact that “there is a slack” (1955, p. 254) between theories and observations. One way to understand that slack is by analogy to points on a plane and lines connecting them: Imagine a number of points spread out arbitrarily on a plane. Then, various (in fact, infinite) lines can connect them. Analogously, if we think of those points as observable events, and the lines connecting them as theories or theoretical hypotheses, it seems plausible to think that several alternative theories can be

<sup>11</sup> Newton-Smith (1978, p. 71), for example, spoke of “Quine's notorious claim that (...) all theories are underdetermined”; Laudan (1990, p. 271) wrote that Quine holds that “*for any theory, T, and any body of evidence supporting T, there is at least one rival (i.e. contrary) to T that is as well supported as T*”; and Devitt (2002, p. 45) suggests that for Quine *any* theory that posits unobservables would have rivals that are empirically equivalent to it.

<sup>12</sup> In the 1980s, Quine replaced the notion of ‘observation conditional’ with that of ‘observation categorical’ (see footnote 11 above). Unlike the former, which are “pegged” to individual space-time points and therefore infinite in number, the latter express generalities and are thus finite in number, since only a finite number of generalities can count as observational. It is not clear whether Quine came to allow for the exceptions we have been discussing once the notion of ‘observation conditional’ was dropped. Nevertheless, it seems to remain true that some theories imply (a finite number of) observational *categoricals* without thereby positing theoretical entities and principles, whereas more complex theories imply observation *categoricals* which are (also finite in number but) so varied in kind and so numerous that those theories become unmanageably complicated if some theoretical entities and principles are not also posited. It seems reasonable to assume that Quine's view in the 1980s and 1990s was that only theories of this latter kind can be underdetermined. The difference is then not between the finite or infinite number of observation *conditionals* that a theory implies, but rather between a manageable and an unmanageable assortment of implied observation *categoricals*. However, even in Quine's later view, we would still need a theory containing theoretical principles and entities in order to find the out the finite list of observation *categoricals*. This is an issue which Quine discusses in connection with Craig's theorem. See Quine (1975a, pp. 324–326) and Craig (1956).

designed to conform to all observations, even all possible observations.<sup>13</sup> Points on a plane do not determine a unique line connecting them; by analogy, observations do not determine a unique theory that predicts them.

As we shall see, however, when we move from points on a plane to actual scientific theories, the analogy becomes rather tenuous. Quine is nonetheless confident enough to assert the thesis, and in part that confidence stems from the support which holism (or, the “Duhem–Quine thesis”) lends to underdetermination. Unlike underdetermination, holism is a very basic doctrine in Quine’s thought. It connects systematically with almost all aspects of his philosophy, in particular, with his qualms about the analytic/synthetic distinction, with the theses of indeterminacy of translation and ontological relativity, and with his take on epistemology, empiricism, and naturalism. An indication of how fundamental he took holism to be is betrayed by the fact that he repeatedly speaks of holism as obviously and trivially true. Answering a criticism by Grünbaum (see his 1960 and 1962) for example, this is what he had to say:

Your claim that the Duhem–Quine thesis, as you call it, is untenable if taken nontrivially, strikes me as persuasive. (...) For my own part I would say that the thesis as I have used it is probably trivial. (1976, p. 132)<sup>14</sup>

Holism can of course be understood in various ways, not all of which are trivial. Quine’s version says merely that scientific sentences are not each endowed with their own separate empirical content, and therefore that in revising a theory one may choose from several different alternatives, all of which bring about the same desired overall conformation to given observations reports. Hence, if a theory implies a false prediction, an individual sentence in the theory can only be deemed responsible if various background assumptions are taken for granted. Strictly speaking, various adjustments in distinct parts of the theory and background assumptions may each suffice to avoid the false implication.

The doctrine, Quine wrote, “must command assent”, but “with reservations” (1975a, p. 313).<sup>15</sup> One reservation is that observation sentences do have an empirical content of their own, even when considered individually. However, even these sentences are not independent from the theories in which they are embedded, since the terms which they contain also occur in more theoretical statements. Observation sentences may be taken “analytically”, word by word, in which case they function just like the more theoretical statements of a theory; and they may also be taken “holophrastically” (1992a, p. 7), in which case they are thought fit for immediate testing, without the aid of further assumptions or investigation. Taken holophrastically, observation sentences stand out as an exception to an unqualified assertion of holism; taken analytically they do not.

<sup>13</sup> Quine speaks of rival theories conforming to “all possible” data or observations in various places (1969, p. 302; 1970, p. 179; 1975a, p. 313; 1975b, pp. 79–80; 1984, p. 294; 1986, p. 156; 1987, pp. 9–10; 1992a, p. 97, and SN (1992b, p. 9). But when pressed on the issue by Bergström (1990), he replied that “[i]n treating of the underdetermination of theories it is a poor idea to assume compatibility with all possible data, though in an earlier paper I put it that way; for it is both unrealistic and irrelevant. What matters is that the theories be empirically equivalent, that is, that they imply all the same possible data, or, more precisely, all the same observation categoricals.” (1990a, p. 53) In later writings, however—perhaps by force of habit—we still find Quine speaking of theories conforming to all possible observations: (1992a, p. 97) and (1992b, p. 9).

<sup>14</sup> See also (1975a, pp. 313–314; 1990b, pp. 11–12; and 1992a, pp. 15–16).

<sup>15</sup> See also Quine (1991, pp. 268–270).

A second reservation to an unqualified assertion of holism has to do with its scope. In ‘Two Dogmas’, Quine wrote that the “unit of empirical significance is the whole of science” (1953, p. 42). In later writings, he regrets such a “needlessly strong statement of holism” (1991, p. 268). That strong statement may remain true, he wrote, but only in a “legalistic sort of way” (*ibid.*). It remains true that even observation sentences may be revised in light of theoretical adjustments, and it remains true that statements which are much removed from observations, such as the statements of logic and pure mathematics, are not immune to revision: “Revision even of the law of excluded middle has been proposed as a means of simplifying quantum mechanics.” (1953, p. 43) He later stresses, however, that the minimal unit of empirical significance need not be taken to be the whole of science, but “chunks of it” (1991, p. 268); that is, “a cluster sufficient to imply an observable effect of an observable experimental condition” (*ibid.*) suffices.<sup>16</sup> Each branch, or clusters of neighboring branches, can thus be thought to imply, together with logic and mathematics, its own set of observation categoricals, and hence to have some autonomy relative to other branches

Science is neither discontinuous nor monolithic. It is variously jointed, and loose in the joints in varying degrees. In the face of a recalcitrant observation we are free to choose what statements to revise and what ones to hold fast, and these alternatives will disrupt various stretches of scientific theory in various ways, varying in severity. Little is gained by saying that the unit is in principle the whole of science, however defensible this claim may be in a legalistic way. (1975a, pp. 314–315)

Granted those two qualifications to the thesis of holism—namely, the separable empirical content of observation sentences taken holophrastically and the relative autonomy of each branch of science—the doctrine remains obviously and even trivially true for Quine: In the face of “adverse observations” a scientist can always elect one among various distinct possible revisions to his overall theory, or to his particular branch of science. No individual statement can be thought to be, on its own, responsible for a theory’s false implications, unless, of course, that statement is itself an observation sentence or the long conjunctive sentence that states the whole theory. But even in those cases, various distinct revisions of the theory as a whole can be devised that inactivate the false implication, by either preventing the implication from obtaining or by changing the extensions of some of the terms that occur in it.

Holism thus suggests underdetermination

If in the face of adverse observations we are free always to choose among various adequate modifications of our theory, then presumably all possible observations are insufficient to determine theory uniquely. (1975a, p. 313)

The conclusion of the reasoning is here offered as something plausible, yet nonetheless conjectural: Holism “lends credence” without actually establishing underdetermination in any way. There are two reasons why it does not. The first is that although “we are always free to choose among various adequate modifications of our theory”, we cannot assume that those modifications will all yield theories that are empirically equivalent. Holism says merely that various modifications may “inactivate” a false

<sup>16</sup> Norton (1994), however, still describes the Duhem–Quine thesis as saying that “theories can only confront evidence as whole”.

implication, but does not say whether the theories that result from those modifications are empirically equivalent. Indeed, it is plausible to think that in most of the relevant cases, alternative modifications will yield theories that are not empirically equivalent. Those modifications would each “inactivate” the false implication in a different way, and in doing so each may “inactivate” (or “activate”) some further implications which remain untouched in alternative modifications.

The second reason is that even if all possible revisions to a given theory yielded only theories which are empirically equivalent, those revised theories might not be sufficiently different to count as rivals, or even alternatives to one another in any nontrivial sense. This seems especially likely if the theories in question are global, and not merely local theories. In the case of global theories, it may just happen that all possible revisions that deactivate a false implication yield theories which are not only empirically equivalent but also theoretically identical, or logically equivalent. They would have the same theoretical structure and would posit the same principles and entities. Whatever differences they might have would then be merely terminological; all sentences of one theory would be reducible via translation to sentences of the other. But theories that are thus intertranslatable are best seen as versions of the same theory, rather than rivals or alternatives. A Chinese physics manual and its Japanese translation are not what we would typically call “alternative” or “rival” theories.

Additional reasons why holism cannot entail underdetermination can also be offered. Holism can be accounted evident not just on the basis of an abstract reflection on the relation between theories and observations in general, but also because of sufficiently clear historical cases of theories that were reformed in various ways so as to avoid known counter-examples.<sup>17</sup> No such direct evidence, however, can be mustered in favor of underdetermination. Although there are some clear enough cases of alternative hypotheses posited as explanations for the same observations, no clear historical cases of empirically equivalent yet rival theories are available. Known examples of rival theories have turned out to be either cases where there is no empirical equivalence (various versions of competing Ptolemaic and Copernican astronomies, for example), or found to be theoretically identical theories couched in different languages (Heisenberg’s and Schrödinger’s formulations of quantum mechanics, for example).<sup>18</sup>

Furthermore, there are no clear historical cases of global theories. Hence, there is no clear evidence that global theories, if we ever devise any, will admit empirically equivalent rivals in a nontrivial sense. Some have argued that there is evidence for the underdetermination of local theories, which is a stronger thesis than the one defended by Quine. Understood as a universal and unqualified claim, it is clearly false: Not all local theories have the theoretical complexity that it takes for underdetermination to obtain; and most rival local theories may be inconsistent with the overall global theory adopted at the moment. This, of course, does not mean that local underdetermination cannot obtain for some theories. But it does show that we can only be sure that it has obtained by simultaneously establishing that global underdetermination holds.

<sup>17</sup> For a couple of historical cases, see Duhem (1954, pp. 184–188). Lakatos (1976) discusses examples from the history of mathematics.

<sup>18</sup> See Laudan and Leplin (1991), Kitcher (1993, chapter 7), Hoefer and Rosenberg (1994), Sklar (2000), and especially Massimi (2004) for further examples and discussion. The common perception that early formulations of quantum mechanics by Heisenberg and Schrödinger were equivalent—see, for example, Hughes (1989, pp. 44–45)—has been contested by Muller (1997a and 1997b), however.

To establish the underdetermination of global theories, we cannot rely on the kind of evidence from the history of science often drawn in favor of holism. Unlike holism, this is not a thesis for which there is direct evidence.<sup>19</sup> In fact, the majority of the known historical cases of competing theories are not cases of empirical equivalence.<sup>20</sup> Moreover, there are no cases of global theories in the history of science: Physics is the best candidate so far, since it is the only discipline that aims at “full coverage” (1981, p. 98). But even physics is yet to attain full coverage, and significant portions and research fields are yet to be unified. So, in this sense, underdetermination must remain somewhat of a conjecture.

Some authors, however, speak of algorithms for creating new theories out of existing ones: see Kukla (1998, chapter 5) and Van Fraassen (1980, chapter 3, Sects. 2 and 3). Most of these are just cases of what Quine would regard as intertranslatable theories. In some cases, it remains unclear whether the artificially constructed “theories” are actually alternatives to be seriously reckoned with in science (see Stanford, 2001). In other cases, the algorithms for creating new theories *are* just the manuals of intertranslation. The resulting theory formulations thus do not qualify as rivals in a non-trivial sense. So one question that remains is how cases of underdetermination should look like. The thesis of underdetermination needs to be examined not only in regards to its truth or plausibility, but also meaningfulness. It might, after all, be an empty thesis; there might not be empirically equivalent *rivals* in any relevant sense of the word. The very idea of rivalry might not make sense when applied to empirically equivalent theories.

Quine took up this issue in 1975a. We have seen that up until then his discussions of the thesis just say that it is plausible given the holistic nature of scientific statements and the “less-than-rigid” connections that obtain between theory and observations. He then insisted that empirically equivalent theories may conflict with each other, and that we should expect a conflict of this sort to come up eventually. In a passage from 1970, he characterizes such conflicts in terms of ‘logical incompatibility’

Theory can still vary though all possible observations be fixed. Physical theories can be at odds with each other and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. 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. (1970, p. 179)<sup>21</sup>

In earlier works Quine had not used the notion of ‘logical incompatibility’; in *Word and Object* (1960, pp. 22, 23), for example, he speaks merely of *alternative* theories.

<sup>19</sup> Stanford (2001) has an inductive argument over the history of science which seeks to establish ‘transient underdetermination’, that is, the underdetermination of theories by *currently* available evidence. His argument, however, can at best establish a conjecture, for it relies on the assumption that at any given moment in the history of science there were “unconsidered alternatives at least roughly equally well-established by the available evidence” (p. S9 n.). Even if this is true, there is no guarantee that such alternatives, if they can truly be devised, are alternatives in a nontrivial sense. Moreover, transient underdetermination is weaker than Quine’s thesis of underdetermination. Even if theories have always been underdetermined by the evidence available at the time they were adopted, it may be the case that global theories do not admit empirically equivalent rivals in a non-trivial sense.

<sup>20</sup> Massimi (2004) offers an interesting and rather detailed analysis of one such case, and further references.

<sup>21</sup> See also Quine (1969, p. 302): “The totality of possible observations of nature, made and unmade, is compatible with physical theories that are incompatible with one another.”

This shift in terminology was not motivated by a substantive change in his views, however, although it was not inconsequential either. The move was an attempt to be more precise in stating what kind of rivalry empirically equivalent theories can be thought to have.

A more substantive review came with “On Empirically Equivalent Systems of the World” (1975a), where Quine added a clause to his characterization of underdetermination stipulating that theories are to be regarded as underdetermined only when empirically equivalent but logically incompatible rivals *cannot be rendered logically equivalent by reconstrual of predicates*. ‘Reconstrual of predicates’, in this context, is nothing but a case of translation, which Quine uses instead because (in 1975a) he narrows the discussion down to empirically equivalent theories already regimented into his canonical notation. Since his canonical notation has no singular terms (see 1960), differences in the vocabulary of rival theories can only occur among predicates. The remainder of the vocabulary (variables, quantifiers, and logical constants) is shared. In a later work, he speaks more generally of translatability (see the second and subsequent printings of *Theories and Things* (1981, p. 29).

The characterization of rival but empirically equivalent theories as non-intertranslatable aims at overcoming the following objection: the usual way of finding two theories empirically equivalent is precisely through translation. Very seldom can the empirical consequences of two theories be checked one by one to ensure equivalence. This indeed is only possible when the number of observation categoricals implied by each theory is small, which is typically not the case. Short of that, empirical equivalence needs to be established by reducing one theory to the other through translation, therefore establishing not only empirical equivalence but also the *theoretical or logical equivalence* of the theories, or at least of the translated versions of the theories. This procedure rules out all rivalry and incompatibilities between theories; it thus brings up the suspicion that all cases of empirical equivalence might also be cases of logical equivalence.

This last concern seems to have been initially raised by Humphries (1970), who pointed out that cases of logical incompatibility (or theoretical conflict in general) between empirically equivalent theories must be distinguished from cases of mere terminological variations. A theory formulation in which all occurrences of “proton” and “electron” are interchanged is trivially distinct from the original theory formulation in which the terms are not interchanged. The observable predictions that each implies are the same, since neither protons nor electrons are directly observable, and the theoretical structure of both theories is also the same. The sole difference lies in that whatever one theory refers to using “proton”, the other does so using “electron”, and vice-versa. There is an uninteresting and trivial sense in which they may be taken to be alternative theories; they are, to be sure, empirically equivalent yet logically incompatible. But there is hardly any reason or advantage to construe theories so stringently. Scientific practice and the ordinary man’s view agree on this regard, treating such variations as merely terminological, and irrelevant for theory.<sup>22</sup>

Following Humphries, a more general objection can be raised against the thesis of underdetermination: it could very well be that all alleged logical incompatibilities

<sup>22</sup> There is of course an issue here of how one ought to define or construe “theory”. Quine does not think this has important consequences for the thesis of underdetermination (see his 1992a, p. 96). Some authors, however, have challenged his views: see Bechtel (1980), Bar-On (1986), Gemes (1991), and Yalçın (2001).

among empirically equivalent theories are due not to substantial theoretical conflict but to mistranslation. This objection was voiced by Dummett

... there could be nothing to prevent our attributing the apparent incompatibility [of two empirically equivalent theories] to equivocation. Indeed, if we can establish the empirical equivalence of the two theories, we must be able to find translations from each to the other. (1981, p. 617 n.)

The objection says that *in principle* any two empirically equivalent theories are intertranslatable, even when we do not know how to translate them. Quine addressed this kind of criticism in (1975a) and (1975b). In the former essay he distinguishes between theories and theory formulations, and concedes that the same theory may be formulated in terminologically inconsistent ways. The proton/electron example mentioned above is an example. Logical incompatibility in such cases would indeed be a trivial matter of equivocation, for surely what is meant by “electron” in one theory formulation is just what is meant by “proton” in the other. The incompatibility can be avoided by translating both theories into the vernacular English of technical physics. Quine’s formulation of the thesis of underdetermination from 1975 onwards rules out such cases by stipulating that it can only hold for empirically equivalent theories that cannot be rendered logically equivalent through translation.

Dummett’s objection says that *in principle* we should expect intertranslation. His reasoning thus rules underdetermination out by definition. Quine’s response to that kind of objection offers instead a more open-minded and empirically oriented approach to the question, one that avoids ruling the thesis out by definition even if we cannot quite comprehend what it implies. This is in keeping with Quine’s empiricism, and his qualms with a priori reasoning in general

Terminology aside, what wants recognizing is that a physical theory radically different from ours, with nothing even recognizably similar to our quantification or objective reference, might still be empirically equivalent to ours, in the sense of predicting the same episodes of sensory bombardment on the strength of the same past episodes. (1975b, p. 80)

In a different context, he makes the same point

Might another culture, another species, take a radically different line of scientific development, guided by norms that differ sharply from ours but that are justified by their scientific findings as ours are by ours? And might these people predict as successfully and thrive as well as we? Yes, I think that we must admit this as a possibility in principle; that we must admit it even from the point of view of our own science, which is the only point of view I can offer. I should be surprised to see this possibility realized, but I cannot picture a disproof. (1981, p. 181)

Interestingly, Quine’s response to the objection says that *in principle* we cannot rule out the possibility of empirically equivalent theories that are not intertranslatable. The objection itself was that *in principle* underdetermination cannot obtain because the conjecture is unintelligible. Quine’s response is that *in principle* we cannot rule it out, even if we cannot quite understand what it entails; that is the sense of a sentence already quoted at the beginning of this paper: “The doctrine [of underdetermination] is plausible insofar as it is intelligible, but it is less readily intelligible than it may seem.” (1975a, p. 313) Although we cannot quite know—for now, at least—what kind of theories, if any, would satisfy the requirements posed by the thesis, the thesis itself remains

plausible. We have no reason to rule out the chances of eventually finding theories which we somehow come to think of as empirically equivalent while systematically failing to find a translation manual which would render both logically equivalent.

Some philosophers have pointed out that unintelligible possibilities should not count as possibilities to begin with. It is indeed true that we can hardly imagine what it would be like to find a square circle in a distant corner of the universe, unless by “square” and “circle” one means something other than square and circle. The conjecture that Quine entertains, however, is not of that sort. Although he concedes that it is not quite clear what it entails or what it would take to find empirically equivalent but untranslatable theories or what they should look like, the conjecture itself is not incoherent. In this regard it is unlike the “square circle” conjecture, which is just false, if not meaningless. The question whether there can exist theories that are empirically equivalent but not intertranslatable thus remains an open question, one to be answered as any other in the natural sciences. Perhaps empirically equivalent rival theories can be devised and we will eventually find some. Perhaps we will never find any due to our lack of persistence or sheer ignorance. Perhaps there are further facts about theories and translations, currently unknown—perhaps even forever unknown—which render the conjecture empty. In this last case, the thesis of underdetermination would be false. Right now, however, it remains a plausible thesis, given the known slack between theoretical hypotheses and observations in modern scientific theories and the likelihood of cultural developments divergent to our current one.

The conjecture that supports the thesis of underdetermination is an empirical conjecture like any other. Later we may find out that it is false, even if nothing suggests it today. We may even think later that it never made much sense, as we do today of some scientific theories of the past. At the fringes of scientific research, there are no definite or sharp boundaries between questions of truth and questions of meaning. This is part of what Quine's writings on the analytic/synthetic distinction intend to bring out, and it can be easily seen in some recent empirical conjectures in theoretical physics. Despite the strong evidence supporting quantum mechanics, for example, the persistent debates on its interpretation indicate that it is far from clear what it amounts to or entails. The lack of understanding about what the world should be like, given the truth of that theory, does not prevent science to advance as usual; likewise with some recent conjectures in string theory. Similarly with underdetermination: it remains unclear what global theories that are empirically equivalent but non-intertranslatable should look like, but as far we can tell the thesis remains plausible. It is not only supported by a coherent conjecture as it is suggested by the lack of a rigid connection between theoretical hypotheses and observations.

If this is true, however, Quine's thesis of underdetermination is not as strong as one might have been inclined to believe. What in Quine's earlier writings may have sounded like a categorical statement, in (1975a) it is not much more than a conjecture. Whether our system of the world is bound to have rivals that are empirically equivalent but not translatable into our own, he wrote, is an “open question” (1975a, p. 327). What is not a conjecture, but persists for Quine as the *thesis* of underdetermination—a “last ditch version”—is “merely that our system of the world is bound to have empirically equivalent alternatives which, *if we were to discover them, we would see no way of reconciling by reconstrual of predicates*” (1975a, p. 328—emphasis added). The thesis is thus “vague and modest”, and says merely that empirically equivalent theories may be so radically different that, even if intertranslation is possible, it may

forever elude us. This mitigated version of the thesis does not rule out the possibility that there might be ways of rendering equivalent through translation any two empirically equivalent theories. It does not say that this will happen, however. Rather, it just says that there are empirically equivalent global theories which, if we ever find them, will remain non-intertranslatable, either by force of our ignorance or because such translations cannot obtain to begin with.

This formulation of the thesis significantly demotes its theoretical significance, as Quine himself acknowledges.<sup>23</sup> Quine claims that the thesis remains nonetheless “vitally important to one’s attitude toward science”. It adds to the generally accepted claim that theories cannot be deduced from observations. Not only can theories not be deduced from observations, but observations alone warrant more than one theory. Alternative, rival theories can be equally warranted by observations. Given this mitigation of the thesis, its theoretical significance cannot be strong. Quine merely says that it has a practical significance, in that it enjoins us to refrain from categorically or dogmatically asserting a theory even if it manages to account for all observations (see 1975a, p. 326).

### 3 Vacillation

We have seen that Quine’s formulations of the thesis of underdetermination changed a few times over the years. Some of those changes were motivated by shifts in reasoning and perspective, while others were merely attempts at a more precise formulation. In papers published in the 1980s Quine completely abandons the distinction he held in (1975a), between theories and theory formulations

It has been urged by many, including me, that scientific theory is underdetermined by all possible data; in other words, that different theories can be empirically equivalent. But this depends on what verbal formulations to count as formulations of different theories and what ones to count rather as different formulations of one and the same theory; and surely this question is philosophically uninteresting. What we are given to compare are the verbal formulations, variously unlike but empirically equivalent. Whether and when to rate them as formulations of the same theory is an inconsequential question of words; we can take the theory formulations and let the theories go. (1984, p. 294—footnote omitted)<sup>24</sup>

In papers of this period, Quine also avoids speaking of ‘logical incompatibility’ among empirically equivalent theories. Characterizing the differences between empirically equivalent rival theories in terms of lack of intertranslation rendered the notion of logical incompatibility inconsequential for the thesis. Since theories that are empirically equivalent must agree on all observation categoricals, logical incompatibilities among them must occur only among sentences which hinge on observation only indirectly. Suppose that one such sentence is  $Fx$ , and that one theory affirms  $(\forall x)Fx$  whereas its rival affirms  $\sim(\forall x)Fx$ . Systematically replace all occurrences of  $F$  in one

<sup>23</sup> Quine claims that the theoretical importance of the thesis is further undermined by Craig’s theorem (1975a, pp. 324–326). Although issue is interesting, we shall leave it aside here; but see Craig (1956), English (1973), and List (1999).

<sup>24</sup> See also Quine (1992a, p. 96).

theory with occurrences of, say,  $F'$ . One theory will then affirm  $(\forall x)Fx$  while the other will affirm  $\sim(\forall x)F'x$ , thus bypassing the logical incompatibility. Repeat the procedure for all sentences which are contradicted by the rival theory and the result will be a new theory which is *compatible* with its empirically equivalent rival. This move of course does not eliminate the incompatibility of the two original theories, nor is it meant to, but it does show that the notion of incompatibility is here inconsequential (see 1990a, p. 53). The new formulation can indeed be regarded as a translation of the one from which it originated into a new vocabulary. Thus, the rival theories are no more incompatible than a physics manual written in Chinese and its Japanese translation.

This “trivial expedient”, as Quine calls it (1981, p. 30; 1984, pp. 294–295), splits up all theoretical terms on which the logical incompatibility hinges. The split terms will have different spellings in each formulation. Since the terms are theoretical and their use is only indirectly affected by observations, the procedure will be of no consequence for the empirical content of the theory. Following this strategy throughout, cases of logical incompatibility among empirically equivalent theories are effectively rendered innocuous. However, splitting the terms may yield untranslatable terms and sentences (1992a, Sects. 41–43), aside from the ones that may already be there. The languages of those theories might even be untranslatable regardless of the split terms. Such, then, is the conflict that empirically equivalent theories can be thought to have.

As we have seen, Quine changed his formulation of the thesis of underdetermination at least three times: his first formulations, in (1955) and (1960), speak rather loosely of empirically equivalent theories that are *alternatives* to one another. In (1984), Quine describes them as empirically equivalent but *logically incompatible*. In (1975a), he adds to his definition the clause that those theories cannot be rendered equivalent by reconstrual of predicates. Later (1981), he speaks more generally of untranslatability instead of “irreconcilable by reconstrual of predicates”. In light of these last two formulations, he drops the notion of logical incompatibility as inconsequential, and construes the rivalry between empirically equivalent theories solely in terms of non-intertranslatability.

The analysis of underdetermination that Quine proposed in (1975a) remained mostly untouched in his later writings. Thereafter it was always a “modest and vague” version of the thesis to which he subscribed. However, he did continue to revise his views regarding a specific problem raised by the thesis, namely, that of how to account for the *truth* of empirically equivalent but rival theories. His first few writings on underdetermination say nothing about it. But as soon as he introduced the notion of logical incompatibility into his characterization of underdetermination (1969, 1970), he was forced to take a stand. If empirically equivalent alternatives to our own theory of the world are logically incompatible with it, then they must be conceived as false, or at least false inasmuch as they contradict our own theory. The reasoning is straightforward: If two theories are logically incompatible, then at most one can be true.

Accounting true our own theory and rejecting the alternatives that conflict with it is what Quine would later call the “sectarian” attitude. It is the only coherent option when rival theories are logically incompatible with our own. As we have seen, however, logical incompatibility can be avoided, and in its absence the opposing “ecumenical” attitude becomes attractive. It views non-conflicting empirically equivalent theories as equally true, and allows for each to be used and adopted consecutively, thus contributing to an “added perspective on nature”. The ecumenical attitude is recommended by empiricism and its “reluctance to discriminate invidiously between

empirically equivalent and equally economical theories” (1992a, p. 99). The sectarian attitude, on the other hand, is recommended by naturalism

Whatever we affirm, after all, we affirm as a statement within our aggregate theory of nature as we now see it; and to call a statement true is just to reaffirm it. Perhaps it is not true, and perhaps we shall find that out; but in any event there is no extra-theoretic truth, no higher truth than the truth we are claiming or aspiring to as we continue to tinker with our system of the world from within. If ours were one of those two rival best theories that we imagined a moment ago, it would be our place to insist on the truth of our laws and falsity of the other theory where it conflicts. (1975a, p. 327)

Oddly enough, Quine explicitly defended both attitudes in two successive essays published in *Theories and Things* (1981). In the first essay, “Things and Their Place in Theories”, he adopts the sectarian line

[I]t 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. (1981, pp. 21–22)

Quine takes the idea of a neutral standpoint or of a first philosophy from which theories of the world can be assessed to be hardly intelligible at all. One cannot judge the truth of a theory without looking at the evidence that supports it, and to do so one must have criteria for assessing the evidence. But deciding what to count as good evidence and criteria already requires a theory of the world, however rudimentary. Hence, although hypothesizing and even doubting about the truth of our own theories may be common scientific practice, those hypotheses and doubts must still be put forth from within whichever evolving theory one holds at the moment.

Just a few pages down, in the essay on “Empirical Content” (first printing only), Quine defends the opposing ecumenical view

Still let us suppose that the two formulations are in fact empirically equivalent even though not known to be; and let us suppose further that all of the implied observation categoricals are in fact true, although, again, not known to be. Nothing more, surely, can be required for the truth of either theory formulation. Are they both true? I say yes. (1981, p. 29—1st printing)

The ecumenical view faces a challenge whenever underdetermination is characterized in terms of logically incompatible theories, since incompatible theories cannot be simultaneously true. What allowed Quine to maintain the ecumenical stance in “Empirical Content” was his revised thesis of underdetermination, which does not make use of the notion of logical incompatibility

Being incompatible, the two theory formulations that we are imagining must evaluate some sentence oppositely. Since they are nevertheless empirically equivalent, that sentence must contain terms that are short on observational criteria. But then we can just as well pick out one of those terms and treat it as if it were two independent words, one in the one theory formulation and another in the other. We can mark this by changing the spelling of the word in one of the two theory formulations. (1981, pp. 29–30—1st printing only)

In (1984, pp. 294–295), again this same “trivial expedient” is mentioned, and the logical incompatibility of empirically equivalent theories described as a “red herring” (p. 294). As in the first printing of “Empirical Content”, the trivial expedient is used to justify adoption of ecumenical attitude

We are thus left only with empirically equivalent theory formulations that are logically reconcilable. If we subscribe to one of them as true, we can call them all true and view them as different descriptions of one and the same world. (RA, p. 295)

The difficulty that remains for the ecumenical attitude is the naturalistic considerations mentioned above: empirically equivalent theories of the world that rival our own presumably posit alternative sets of theoretical principles and entities. Hence, to say that those theories can be equally accounted true amounts to either admitting various realities or to restricting the truth of a theory to its observable consequences. Quine accepts neither consequence. He sees himself as a realist, and maintains both that reality is unique and that we can know what it is, insofar as science is correct. At one point, he tried reconciling the ecumenical attitude with naturalism and realism by stressing that underdetermination is an epistemological thesis; that is, a thesis about warrantedness and evidence, not truth

The truth of physical theory and the reality of microphysical particles, gross bodies, numbers, sets, are not impugned by what I have said of proxy functions and of wildly deviant but empirically equivalent theory formulations. Those remarks had to do not with what there is and what is true about the world, but only with the evidence for what there is and what is true about the world. I was showing that scientific discourse radically unlike our own, structurally and ontologically, could claim equal evidence and that we are free to switch. Still we can treat of the world and its objects only within some scientific idiom, this or another; there are others, but none higher. (1984, p. 295)

Here we see Quine laying side by side the thesis of underdetermination and the thesis of ontological relativity. The former, however, is potentially more hazardous to the realist than ontological variations allowed by proxy functions. In the latter, theories vary in ontology but may be rendered equivalent through translation. In fact, a manual for such translations may just be a list of the proxy functions. In such cases, then, theories would not only be empirically equivalent, but also structurally and theoretically identical. They can be rendered logically equivalent through translation. They would differ in what each counts as objects, but not in how they are interrelated. But then there is very little difference between them, other than terminological. The objects that there are, Quine insists, are the ones the theories we adopt say there are, insofar as they are correct, and that remains as is regardless of how the objects are named.

Reference and ontology recede thus to the status of mere auxiliaries. True sentences, observational and theoretical, are the alpha and omega of the scientific enterprise. They are related by structure, and objects figure as mere nodes of the structure. What particular objects there may be is indifferent to the truth of observation sentences, indifferent to the support they lend to the theoretical sentences, indifferent to the success of the theory in its predictions. (1992a, p. 31)

Thus it is unclear in what sense theories that vary only in ontology could count as rivals.<sup>25</sup> Strictly speaking, such theories are empirically indistinguishable. As in the “electron/proton” example mentioned above, one may shift from one formulation to the other without thereby committing oneself to a different picture of the world. The cases suggested by underdetermination, on the other hand, are such that theory formulations cannot be converted one into the other by systematic intertranslation. These cases must differ in a more substantial manner than the ones suggested by ontological relativity. One expects empirically equivalent but non-intertranslatable theories to be structurally dissimilar, that is, to differ not only in ontology but also in the laws governing the interactions of whichever objects it posits. Differences of this sort do seem to present conflicting accounts of what the world is like, even if the theories are not logically incompatible.

Quine seems to have acknowledged the potential difficulty and attempted to resolve it in various ways. One solution he entertained in the 1980s was to conjoin the alternative theories into a single tandem theory. He later dropped that solution (see 1990b, pp. 14–15): The tandem theory would have separate and redundant lobes, each containing terms and sentences which would not make sense in its counterpart. Untranslatable phrases might even have been created by the trivial expedient of splitting theoretical terms to avoid logical incompatibilities cited above. One lobe must thus remain partially untranslatable and completely redundant, given the other. The idea was thus altogether rejected

It is as if some scientifically undigested terms of metaphysics or religion, say ‘essence’ or ‘grace’ or ‘Nirvana’, were admitted into science along with all their pertinent doctrine, and tolerated on the ground merely that they contravened no observations. It would be an abandonment of the scientist’s quest for economy and the empiricist’s standard of meaningfulness. (1998b, p. 157)

In later writings (1990b, p. 14; 1992a, p. 98), Quine also mentions “entelechy” and “phlogiston” as examples of terms that are meaningless from the standpoint of our theory, as well as the notion of “the center of the universe” discussed by Poincaré. Theories that contain such terms, he says, are partially meaningless from the standpoint of our own theory; some sentences will be shared, most notably the observation categoricals, others will be untranslatable.

Prompted by a commentary by Roger Gibson Jr. (1998, pp. 152–153), who pointed out the inconsistency of the sectarian and ecumenical attitudes in *Theories and Things*, Quine immediately switched back to the sectarian view

The sectarian position, then, is my newly recovered stance on these precarious slopes. It is called for in that last case, where no way is evident of annexing the rival system of the world without adding new terms. Our own system is true by our lights, and the other does not even make sense in our terms. (1998, p. 157)

Accordingly, he replaced the ecumenical paragraphs of “Empirical Content” with the following:

Still we might succeed somehow in persuading ourselves of the empirical equivalence of the two formulations despite finding no way of intertranslation. Then

<sup>25</sup> In fact, it is not even clear whether the theories produced by Quine’s proxy-functions vary in ontology at all, since the complement sets are presumably in the original theory already.

we should indeed recognize the two as equally well *warranted*. We might even oscillate between them, for the sake of a richer perspective on nature. But we should still limit the ascription of truth to whichever theory formulation we are entertaining at the time, for there is no wider frame of reference. (1981 [second printing, of 1987, and subsequent printings], p. 29)

This new defense of the sectarian position would again be short lived, however. In (1990b), prompted by Davidson, Quine switched back to the ecumenical position, this time arguing that rival but empirically equivalent theories, purged of inconsistencies by the trivial expedient of splitting theoretical terms, can be regarded as separately true on their own terms

[E]conomy, which counted against the tandem idea, is imperative only as an ideal of theory construction and not of language. Meaningful application of the truth predicate, on the other hand, extends to the whole language and is not limited to any particular theory formulation. Empirically equivalent and logically compatible theories can be accepted as true descriptions of the world even if one of them uses terms irreducibly alien to the other. There is no call to fuse them into a single redundant theory. Our language can embrace the full vocabularies of both theories, and our truth predicate can then apply to each on its separate merits. (1990b, p. 14)

The vacillation, however, persisted, and in that same year of 1990, Quine again switched back to the sectarian attitude (1992a, pp. 99–101). He argued that the ecumenical view defended in (1990b) raises problems concerning the language of science. If two rival theories are to be equally and separately true, regardless of the fact that each contains expressions that are foreign and meaningless in the other, then the language of science would have to contain all the terms of both theories. Furthermore, the variables would have to range over objects posited by both. If pursued systematically, the result would be a third theory containing the two original ones. Accounting those two theories true does not differ in any relevant way, he argues, from holding a third theory that is just the conjunction of the two. But this, again, violates the maxim of economy in theory construction. Since the conjoined theories are empirically equivalent, one must be redundant, given the other

What is to be gained [with the ecumenical view] is not evident, apart from the satisfaction of conferring the cachet of truth evenhandedly. The sectarian is no less capable than the ecumenist of appreciating the equal evidential claims of the two rival theories of the world. He can still be evenhanded with the cachet of warrantedness, if not of truth. Moreover he is as free as the ecumenist to oscillate between the two theories for the sake of added perspective from which to triangulate on problems. In his sectarian way he does deem the one theory true and the alien terms of the other theory meaningless, but only so long as he is entertaining the one theory rather than the other. He can readily shift the shoe to the other foot. (1992a, p. 100)

This was Quine's last published opinion on the matter. It came with a twist, however. After years fluctuating between the ecumenical and the sectarian views, this time he clearly refrains from asserting his position in a categorical manner. Instead, he acknowledges his own vacillation and demotes the problem to a matter of words

The fantasy of irresolubly rival systems of the world is a thought experiment out beyond where linguistic usage has been crystallized by use. No wonder the cosmic question whether to call two such world systems true should simmer down, bathetically, to a question of words. Hence also, meanwhile, my vacillation. (1992a, pp. 100–101)

It is not too clear what to make of this passage. Quine seems to be insisting here that the idea that there are empirically equivalent rivals (i.e. non-intertranslatable) to any global theory is no more than a conjecture. The thesis which he is willing to assert is weaker and says merely that even if that conjecture is false, we may still find empirically equivalent global theories which *we* fail to intertranslate. This is nonetheless a conjecture, since it remains unclear whether we will indeed find a global theory which conforms to all observations, and even less clear what a rival empirically equivalent theory would look like. We can entertain the thought, but only in imagination. The details are vague, and we cannot anticipate how we would react or what we would say about the truth of those theories. The details of the case are crucial here, and in the absence of those, very little can be said.<sup>26</sup> There is no determinate cut off point here, but only a gradient. The “fantasy irresolubly rival systems of the world” invites us to imagine a situation very different from that of current science. Indeed, one in which the terms and criteria currently used would by then surely have been modified, or abandoned.

Nevertheless, using the notions we have now, it would be a question of words whether to *say* that rival theories are true. This of course depends on how we are willing to use the word “true”, but is not limited to that word. The interesting point to notice here is that in Quine’s own philosophical trajectory, the question *became* a matter of words. In the beginning it was not, since underdetermination was thought by him as comprising logically incompatible theories. Once the notion of logical incompatibility is dropped from the formulation of underdetermination, the ecumenical and sectarian views cease to diverge on whether to account alternative theories false. Instead, the problem becomes that of whether alternative theories can be regarded *meaningful* from the standpoint of the theory we happen to hold and use at that moment. Meaningfulness, however, *is* a matter of words. It is a matter of finding a place and a role for certain words in our description of the world.

“Phlogiston”, to cite a notorious example, has no place in our current physical theory. We can understand how it was used in the past, even if we now reject those uses and the false theories in which they were embedded. But where empirically equivalent alternatives to our own theory of the world are concerned, there can be no evidence against rival theories that is not also evidence against our own theory. Hence, expressions that cannot be incorporated into the language of our own theory cannot be rejected as meaningless on the count that they only make sense in a theory that we know to be false. Whether to regard those terms as meaningful should hinge primarily on how tolerant one is willing to be on matters of linguistic usage, not on matters of fact. The moral of the story, Quine wrote, is that “there are various defensible ways of conceiving the world” (1992a, p. 102). These are nonetheless conceptions of the same world: reality outruns theory, or so “one is prepared to believe” (1992a, p. 101).

As mentioned at the beginning, Quine’s vacillations on these matters are often seen as a symptom of a deeper tension within his philosophy. Considering how mitigated

<sup>26</sup> On this particular shortcoming of the use of thought experiments in philosophy, see Wilkes (1988), chapter 1.

the thesis of underdetermination had become by the end of (1975a), and how his reformulation of the thesis allowed the question about the truth of rival but empirically equivalent systems of the world to simmer down to a matter of words, that tension should now appear to run rather superficially. In any case, it should be clear by now that underdetermination is not a fundamental thesis within Quine's system: given its conjectural status, no other doctrine in his system can depend on it; the thesis itself is underdetermined by his system in the sense that if it were to fail, no consequences would follow for Quine's other doctrines. It is nonetheless a plausible thesis, even undeniable for anyone willing to acknowledge the “less-than-rigid” connections that obtain between theories and observations.

#### 4 Some misreadings of Quine on underdetermination

We are now in position to very briefly review a some common misrepresentations of Quine's views on underdetermination, and to reflect on why they happen so frequently in the literature. Some of these have implications for the larger debate on underdetermination. The following examples are fairly representative, and characteristic of contemporary discussions of the topic.

We begin with Laudan (1990, 271 ff.) and Kitcher (1993, p. 251), who misdescribe holism as the “Quinean ground” for underdetermination. As we have seen, in (1975a) Quine does say that holism “lends credence” to underdetermination, but he then immediately goes on to explain why it is insufficient to establish it on any firm ground. The thesis itself, he says, has a meager theoretical basis, and its justification is rather conjectural; Quine's view is not, in any case, that it rests on holism alone. In a somewhat related misreading, Laudan (1990, p. 268), Kitcher (1993, p. 250) and Leplin (1997, p. 210) assign to Quine the stronger version of holism explicitly criticized by Grünbaum.<sup>27</sup> Quine, as we have seen (1976, p. 132; 1990b, pp. 11–12; and 1992a, pp. 15–16), agrees with those criticisms and never really defended that version of the thesis.

Hoefer and Rosenberg (1994, p. 606) as well as Kukla (1996, pp. 139–140) maintain that Quine is committed to the view that rival but empirically equivalent systems of the world must be incompatible. We have seen that already by the late 1970s Quine had abandoned that characterization of the thesis, favoring instead the view that rival but empirically equivalent theories cannot be rendered logically equivalent through translation.

Stanford (2001, p. S8, footnote 8) suggests that Quine asserts underdetermination without actually arguing for it. As we have seen, however, Quine did argue for underdetermination. The conclusion of the argument may not be as categorical as one might have expected, but then the problem lies in the expectations of the reader rather than in the argument itself. Quine's thesis of underdetermination is in fact somewhat disappointing and anticlimactic from a metaphysical point of view. As mentioned at the beginning, most of the current literature on underdetermination accepts without objection the assumption that the thesis carries with it significant consequences for the debates on Realism and Anti-realism in the philosophy of science. That is not the case with Quine's view of underdetermination.

<sup>27</sup> See also Norton (1994, p. 3).

Finally, Newton-Smith (1978, p. 71), Norton (1994, p. 3), and Devitt (2002, p. 45) suggest that Quine believes that *all* theories positing unobservables have rivals which are equally warranted by observations. However, as we have also seen, Quine denies that this is so (1975a, p. 324; 1979, p. 66).

Perhaps a plausible explanation for these frequent misreadings may lie in the fact that Quine is a systematic philosopher whose views on one topic cannot be insulated from the rest of his philosophy. Different aspects of his thought converge into his views on underdetermination, and only in that context can his claim be fully appreciated and understood. A second reason may lie in the fact that the authors just mentioned—and indeed most others in the recent literature on the topic—view underdetermination as a metaphysically or epistemically loaded thesis: a thesis which, if true, would entail the falsehood of scientific realism or the truth of anti-realism or agnosticism about theoretical entities and principles. For Quine, the thesis does not have any such consequences. This seems to have rendered his description and defense of the thesis rather opaque and intractable to some. Quine himself describes his most important published discussion of the thesis as an investigation into “its meaning and its limits” (1975a, p. 313); for the most part it is not a defense of a thesis but the pursuit of an initially plausible idea that turns out to be assertible only in a very mitigated form. The bulk of that paper aims at dismissing formulations of the thesis which are too strong and therefore untenable. In the end, he asserts only a “vague and modest” (p. 327) thesis. Perhaps much too vague and modest to be adequately heard in the debates about realism and anti-realism that we currently witness. Finally, a third reason for the frequent misreadings may lie in the fact that Quine’s formulations of the thesis were revised over the years, and in the fact that he changed his mind several times about how to address some of the issues that motivate contemporary debates. In addition to those changes, which have been up until now only partially documented in the literature, there was also a change of subject in Quine’s writings. Up until 1975, Quine’s discussions of underdetermination are mostly concerned with establishing its plausibility. Afterwards, however, he was mostly concerned with the question of how to deal with a problem left over from his formulation of the thesis, namely, that of how to account for the truth of rival theories. In the recent literature, little distinction has been made between those two sets of texts. As a result his discussions of the metaphysical question of the truth of rival theories have been thought to be discussions of the formulation and justification of the thesis,<sup>28</sup> which they are not.

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<sup>28</sup> This may be the case of Yalçın (2001), who discusses the two sets of texts side by side.

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