

# Quine's 'Needlessly Strong' Holism

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**Abstract:** Quine is routinely perceived as having changed his mind about the scope of the Duhem-Quine thesis, shifting from what has been called an 'extreme holism' to a more moderate view. Where the Quine of 'Two Dogmas of Empiricism' argues that "the unit of empirical significance is the whole of science" (1951, 42), the later Quine seems to back away from this "needlessly strong statement of holism" (1991, 393). In this paper, I show that the received view is incorrect. I distinguish three ways in which Quine's early holism can be said to be wide-scoped and show that he has never changed his mind about any one of these aspects of his early view. Instead, I argue that Quine's apparent change of mind can be explained away as a mere shift of emphasis.

## 1. Introduction

Evidential holism, or the Duhem-Quine thesis, is the influential idea that hypotheses cannot be tested in isolation but only in conjunction with background theory. The thesis is often illustrated by recounting how astronomers dealt with the unexpected orbits of Uranus and Mercury in the 19<sup>th</sup> and early-20<sup>th</sup> century: where the peculiar orbit of Uranus led to the discovery of Neptune, the problem of the anomalous advance in the perihelion of Mercury was ultimately solved by giving up Newton's inverse square law; the advance turned out to

be a relativistic effect accentuated by Mercury's position close to the Sun. Two similarly structured problems, in other words, were solved in two radically different ways: one in which Newton's theory could be saved by giving up an auxiliary hypothesis (the number of planets) and one in which the main theory had to be revised. Testing a scientific hypothesis, the episode teaches us, cannot be done without presupposing a wide range of background theories; in testing Newton's inverse square law, astronomers relied on assumptions about the adequacy of their telescopes, about the existence of a certain number of planets, about the accuracy of their methods of measuring angles, distances, and time, about the precision of their observing skills, and about the exactness of their mathematical machinery. In the words of Pierre Duhem: comparing "calculated perturbations with the perturbations observed by means of the most precise instruments [...] will not only bear on this or that part of the Newtonian principle, but will involve all its parts at the same time" (Duhem 1914, 194).

Although evidential holism was first formulated and defended by Duhem, its contemporary influence is mostly due to the work of Willard Van Orman Quine, who extended the scope of evidential holism to "the whole of science" (1951, 42). Where Duhem argued that only *some* hypotheses cannot be tested in isolation but only in conjunction with *some* background theory, Quine, in "Two Dogmas of Empiricism", famously suggests that "[n]o particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field *as a whole*" (ibid., 42-3, my emphasis). In perhaps the most-cited passage in twentieth-century analytic philosophy, Quine concludes that in the light of adverse experience "[a]ny statement can be held true come what may" and, conversely, that "no statement is immune to revision" (ibid.).

Despite the extensive influence of his ideas, however, Quine seems to change his mind about the scope of evidential holism in later stages of his career. In 'Two Dogmas in Retrospect', for instance, Quine explicitly claims that he regrets his "needlessly strong statement of holism" in 'Two Dogmas of Empiricism':

In later writings I have invoked not the whole of science but chunks of it, clusters of sentences just inclusive enough to have critical semantic mass. By this I mean a cluster sufficient to imply an observable effect.<sup>1</sup> (1991, 393)

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<sup>1</sup> See also Quine (1975a, 71): "When we look thus to a whole theory or system of sentences as the vehicle of empirical meaning, how inclusive should we take this system to be? [...] modest chunks suffice, and so may be ascribed their independent empirical meaning".

Quine, in other words, seems to switch from what we may call an “extreme holism” to a more modest view.<sup>2</sup> In fact, Quine even seems to give up on his radical revisability thesis in the later stages of his career. Where the early Quine held that in the light of adverse experience we could even amend “statements of the kind called logical laws” (1951, 43), the later Quine appears to make a substantive exception for logic: even if we were to try to revise a logical law like the law of non-contradiction, we would “only [be changing] the subject” (1970, 81).<sup>3</sup>

Quine, in sum, seems to have abandoned his wide-scoped holism for a more moderate view about the logic of theory testing. In this paper, however, I argue that the standard story about the evolution of Quine’s holism is misguided. I show that a detailed examination of Quine’s views early and late reveals that there is no reason to presume that he changed his mind. More specifically, I argue (1) that there are no less than three ways in which Quine’s early variant of holism can be said to be wide-scoped, (2) that he does not significantly change his mind about any one of these aspects of his early view, and (3) that his apparent shift can be explained away by showing how he merely emphasized different aspects of his theory in later stages of his career.

What I offer, in short, is an argument for the claim that although the later Quine would have emphasized different aspects of his theory if he had had the chance to rewrite ‘Two Dogmas’, he did not believe his early theory to be false. This paper is structured as follows. After introducing Quine’s holism and dissolving some misconceptions about how we are to interpret it (sections 2-3), I distinguish three ways in which Quine’s view can be said to be wide-scoped (section 4). Next, I examine the evolution of Quine’s views with respect to these three aspects one by one (sections 5-7), delving into his views about the unity of science (sec-

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<sup>2</sup> This reading of the evolution of Quine’s position is omnipresent in the literature. See, for example, Massey (2011, 256): “Late-Quine rails against *extreme holism* [...] while advocating *moderate holism*”; and Loeffler (2005, 173): “at least since the mid-1970s Quine had moved away from radical holism [...] According to [Quine’s later doctrine of moderate holism] the unit of empirical significance is not an all-encompassing background theory (the ‘whole of science’) any more”.

<sup>3</sup> See also Fogelin (2004, 32): “not only does Quine’s extreme holism become muted in his later writings, the radical revisability thesis associated with it has become muted as well”. Quine’s apparent change of mind on the status of logic has inspired Arnold and Shapiro (2007, 276) to distinguish between a “radical Quine” and a “logic-friendly Quine”.

tion 5), the revisability of logic (section 6), and about how we should interpret his claim that wide-scoped holism should be viewed as an “uninteresting legalism” (1975a, 71) (section 7).

## 2. Evidential Holism

Evidential holism is a thesis about the logical relation between theory and evidence; or, in Quinean terms, about the relation between clusters of theoretical sentences and observation categoricals.<sup>4</sup> The logical relation between theoretical sentences and observation categoricals can be best described by what might be called a prediction thesis and a falsification thesis:<sup>5</sup>

(PT) *Prediction thesis*: a single hypothesis does not imply an observation categorical. Only clusters of theoretical sentences will imply observation categoricals.<sup>6</sup>

(FT) *Falsification thesis*: whenever a predicted observation categorical turns out to be false, one cannot logically determine which theoretical sentence is falsified. Rather, the cluster of theoretical sentences that implied the categorical is falsified as a whole.<sup>7</sup>

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<sup>4</sup> Observation categoricals are sentences of the form ‘Whenever P, Q’, where P and Q are observation sentences such that the categorical expresses “the general expectation that whenever the one observation sentence holds, the other will be fulfilled as well”. As examples of observation categoricals, Quine mentions ‘When it snows, it’s cold’, ‘Where there’s smoke, there’s fire’, and ‘When the sun rises, the birds sing’ (1995a, 25). It should be noted that Quine has not always explicated observational predictions in terms of observation categoricals. See, for example, Quine (1960; 1975b). I thank an anonymous referee for stressing this point.

<sup>5</sup> The terms ‘prediction thesis’ and ‘falsification thesis’ are from Morrison (2010). The distinction is quite common in the literature, albeit under different names. See P. L. Quinn’s (1974) distinction between a ‘separability’ and a ‘falsifiability thesis’ and Ariew’s (1984) distinction between a ‘non-separability’ and a ‘non-falsifiability thesis’.

<sup>6</sup> There is one set of trivial exceptions to PT: if one combines all the theoretical sentences that together imply an observation categorical into one long conjunction, this conjunction will imply the categorical by itself as well. See Quine (1975a, 72; 1986e, 620).

<sup>7</sup> See Quine (1990a, 13-4): “the falsity of the observation categorical does not conclusively refute the hypothesis. What it refutes is the conjunction of sentences that was needed to imply the observation

Applied to the stock example of evidential holism mentioned in the introduction, PT states that Newton's inverse square law does not by itself imply anything about the orbits of Uranus and Mercury, whereas FT states that whenever one's predictions about these orbits turn out to be incorrect, one cannot logically determine whether one ought to revise Newton's law or an auxiliary hypothesis. As such, the two theses aptly explain how it is possible that two similarly structured problems, the unexpected orbits of Uranus and Mercury, were solved in two radically different ways.

Why should we believe that PT and FT are true? According to Quine, PT is simply an empirical fact, firmly supported (1) by scientific practice, as is evinced by the example of the rise and fall of Newton's inverse square law, and (2) by the complexity of the language we use to express scientific theories. Our scientific language is so complicated that it cannot be learned by "continuous derivation" from observation sentences. In consequence, we also cannot follow this process backward and "reduce scientific theory to sheer observation" (1975c, 267). Non-holistic languages are possible, according to Quine, but they would never be rich enough to express our best scientific theories, or so he argues in a response to Robert Nozick:

[Nozick] asks whether a non-Duhemian language would be impossible for us. Let me say that the observation sentences, in my behaviorally defined sense, constitute already a rudimentary language of this kind [...] But I see no hope of a science comparable in power to our own that would not be subject to holism. (1986b, 364)

The prediction thesis, in short, is an empirical thesis; it is justified on the basis of observations about scientific practice and language learning.<sup>8</sup> It is probably because of

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categorical. In order to retract that conjunction we do not have to retract the hypothesis in question; we could retract some other sentence of the conjunction instead". As we shall see in section 7, the notion of implication used in PT and FT can be interpreted in different ways.

<sup>8</sup> See Gibson (1988, 32-4), who dubs these arguments the "scientific practices argument" and the "language learning argument". Duhem uses different arguments to justify PT. See Darling (2002).

arguments like these that even the strongest opponents to evidential holism admit that at least PT is true.<sup>9</sup>

### 3. Falsification and scientific practice

Although there is widespread consensus about PT, there is no such agreement when FT is concerned. We can distinguish two types of argument against FT in the literature. Before I return to the main purpose of this paper, i.e. providing a detailed account of the evolution of Quine's wide-scoped holism, it is important to see how these arguments rest on misconceptions about the nature of Quine's evidential holism.

The first argument against FT points to the way in which scientists actually test their theories and argues that, in general, scientists *do* have reasons to discriminate between hypotheses that are and hypotheses that are not falsified whenever a prediction fails. According to Sober, for example, there is a clear distinction between hypotheses 'under test' and auxiliary hypotheses 'in use' when we test a scientific theory:

When scientists want to test one hypothesis against another, they don't simply invent auxiliary assumptions that permit the competing hypotheses to issue in predictions. Rather, they try to find auxiliary assumptions that they already have good reason to think are true. This means that the auxiliary assumptions used in a test and the hypotheses under test differ in their epistemological standing.<sup>10</sup> (Sober 1999, 54)

To illustrate his point, Sober asks us to consider a woman who uses a pregnancy test to check her 'hypothesis' that she might be pregnant. Although Sober concedes that, strictly speaking, a prediction about the outcome of the test can only be derived by presupposing a bunch of

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<sup>9</sup> See, for example, Elliott Sober (1999) who criticizes FT but nevertheless maintains that "hypotheses rarely make observational predictions on their own; they require supplementation by auxiliary assumptions if they are to be tested" (1999, 54).

<sup>10</sup> For related objections to FT, see Glymour (1975, 403): "Scientists often claim that an experiment or observation tests certain hypotheses within a complex theory but not others"; and Popper (1963, 243-4): "It seems to me quite clear that it is [...] through [the] temporary successes of our theories that we can be reasonably successful in attributing our refutations to definite portions of the theoretical maze".

auxiliary hypotheses (e.g. about how to detect pregnancy markers), he argues that the results of this test do not bear on these auxiliary hypotheses at all. Of course no test is completely reliable; there will be some false positives and false negatives. Still, the woman's "degree of confidence in the reliability of the test procedure remains unchanged" whatever the outcome of the test in question (ibid., 55). While it could be reasonable for the woman to reject the test result by hypothesizing that it concerns a false positive or a false negative, it would be *unreasonable* for her to reject the test result by changing her views about the test's reliability: "it takes a very different experiment to assess the reliability of the test procedure" (ibid.).

From Quine's point of view, however, Sober's argument rests on an erroneous presupposition; it assumes that FT implies that there is no reasonable way to decide which hypothesis to refute in the light of adverse experience. This is a mistake, however, as Quine's point is only that there is no *logical* way to decide this. FT, Quine argues, is a thesis about the *logic* of theory testing and is perfectly compatible with the claim that there are other reasonable means to decide between competing hypotheses. Quine, that is, merely makes the logical point that "we do not *have to* retract the hypothesis in question" and that we "*could* retract some other sentence of the conjunction instead" (1990a, 14, my emphasis).<sup>11</sup> In fact, Quine has often attempted to list the 'pragmatic maxims' that describe our revision norms beyond the norms of logic.<sup>12</sup>

Applied to Sober's example about the pregnancy test, we can conclude that although it would be *unreasonable* for the woman to change her beliefs about the reliability of the test,

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<sup>11</sup> The same holds for Duhem (1914, 216-7): "Pure logic is not the only rule for our judgments; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices [...] constitute what is appropriately called good sense [*bon sens*]". Recently, the question of how to interpret Duhem's theory of good sense has been debated in this journal. See, for example, Stump (2007), Ivanova (2010), and Fairweather (2012).

<sup>12</sup> See, for example, Quine (1960, §5; 1990a, 15; 1995a, 49) and Quine and Ullian (1970, ch. 6). Although these works contain different lists of pragmatic maxims, two of them appear on all these lists: "the maximization of simplicity and the minimization of mutilation" (1990a, 15). On several occasions, Quine has attempted to explicate these maxims. In the end, however, he seems to have concluded that it is impossible to provide these norms with a formal explication: "No general calibration of either conservatism or simplicity is known, much less any comparative scale of the one against the other. For this reason alone—and it is not alone—there is no hope of a mechanical procedure for optimum hypothesizing" (1995a, 49).

this does not mean that *logically*, modifying this belief is not always an option. Sober’s distinction between hypotheses ‘in use’ and hypotheses ‘under test’ is therefore a pragmatic and not a logical distinction. Rather, which hypotheses we regard to be ‘under test’ reflects our ideas about which revisions would make good sense in the light of adverse experience: “The scientist thinks of his experiment as a test specifically of his new hypothesis, but only because this was the sentence he was wondering about and is prepared to reject” (Quine 1990a, 14).

Quine’s falsification thesis thus turns out to be a straightforward, almost trivial, view about the *logic* of theory testing. The triviality of FT becomes even clearer when we turn to a second argument that has been offered against it, an argument developed by Adolf Grünbaum (1962). Suppose a physicist deduces an observation categorical O from a conjunction of theoretical sentences H&A and that she subsequently observes not-O. As we have seen, FT implies that the physicist cannot logically determine whether she has to revise H or one of the auxiliary hypotheses in A in resolving this conflict between theory and evidence. According to Grünbaum, however, this diagnosis need not be correct. For, Grünbaum argues, it would be correct only if the physicist has *independently* established that there exists (1) an alternative H’ which together with A implies not-O, and (2) an alternative set of auxiliary hypotheses A’ which together with H implies not-O. In other words, FT’s diagnosis is only correct when both

- (a)  $\exists A'((H \& A') \rightarrow \text{not-O}),$  and
- (b)  $\exists H'((H' \& A) \rightarrow \text{not-O})$

are true. To see this, suppose that (a) is false, e.g. because it turns out to be impossible to adapt the auxiliary hypotheses in such a way that the revised theory, combined with H, correctly predicts not-O. The physicist, in such a scenario, would have only one revision option left, namely H, and FT would turn out to be incorrect. After all, the physicist in this scenario, *pace* FT, *can* logically determine which hypothesis to revise in the light of her failed prediction.<sup>13</sup>

Now, according to Grünbaum, Quine can only save FT if he is willing to defend the claim that (a) and (b) can be made true by changing the meanings of the terms in H, H’, A, or A’.

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<sup>13</sup> For a more concrete example of a scenario in which FT fails, see Grünbaum (1962, 23-33). For objections to this particular example, see Laudan (1965) and Giannoni (1967).

Suppose for instance that H is the hypothesis that ‘Ordinary buttermilk is highly toxic to humans’ and that not-O is the observation that buttermilk is actually quite wholesome. According to Grünbaum, Quine could technically save FT by arguing that a scientist always has the option to change the rules of English. In this example, for instance, the scientist has the option to change “the intension of the term ‘ordinary buttermilk’ [to] that of the term ‘arsenic’ in its customary usage”. The problem with this strategy, however, is that FT would then turn out to be “a thoroughly unenlightening truism” (Grünbaum 1962, 20); after all, we would only save the hypothesis by changing its meaning.<sup>14</sup> Grünbaum’s argument, in sum, proposes a dilemma: FT is either false (when changes of meaning are not allowed) or trivially true (when changes of meaning *are* allowed).

Quine has responded to Grünbaum’s dilemma in a 1962-letter. He wholeheartedly chooses the latter option, admitting that FT is trivially true, and explains that he never intended to propose FT as a substantive thesis in the first place:<sup>15</sup>

Your claim that the Duhem-Quine thesis, as you call it, is untenable if taken nontrivially, strikes me as persuasive. Certainly it is carefully argued. For my own part I would say that the thesis as I have used it *is* probably trivial. I haven’t advanced it as an interesting thesis as such [...] I am not concerned even to avoid the trivial extreme of sustaining a law by changing a meaning; for the cleavage between meaning and fact is part of what, in such contexts, I am questioning. (1962, 132)

Quine, in other words, admits that FT is trivial; there simply is no strict distinction between changes of fact and changes of meaning.<sup>16</sup> If Grünbaum is right in claiming that admitting semantic revisions trivializes FT, then Quine is happy to accept that FT is trivial.<sup>17</sup>

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<sup>14</sup> Cf. Fodor and Lepore (1992, 47): “It’s only epistemologically interesting that you could hold onto ‘Burning is the liberation of phlogiston’ in the face of Lavoisier’s results if ‘Burning is the liberation of phlogiston’ means that the burning is the liberation of phlogiston. It’s no news that you could hold onto it in the face of those results if it means that Greycat has whiskers”.

<sup>15</sup> See also Gibson (1987, 67-8) and Becker (2001, 78-9). It should be noted that Quine (1990a, 16) gives a slightly different response.

<sup>16</sup> Does this imply that scientists often resort to changing the meanings of their terms when confronted with a failed prediction like in the above buttermilk-example? No it does not, because like other radical revisions—e.g. revisions of logical truths—such radical changes of meaning are constrained by Quine’s maxim of minimum mutilation. See Becker (2001, 79-80).

#### 4. Three types of wide-scoped holism

Thus far, I have limited my discussion of evidential holism to the question of whether PT and FT are justified. I have argued that PT can be thought of as an empirical claim—firmly supported by observations about scientific practice—and that FT is an almost trivial thesis about the logic of theory testing, a thesis that simply follows from PT.

Now, let me turn to the main subject of this paper, the *scope* of Quine’s holism. In “Two Dogmas of Empiricism”, Quine defends a *wide-scoped* holism: he claims that “*total science*” is like “a field of force whose boundary conditions are experience”; that “*the whole of science*” is “the unit of empirical significance”; and that “*no statement*” is “immune to revision” (1951, 42-3, my emphasis). Quine, in other words, suggests that holism as defined by PT and FT applies to science as a whole.<sup>18</sup>

Still, Quine’s wide-scoped holism as described above is strongly ambiguous. Some of the above-mentioned quotes from ‘Two Dogmas’ deal with the domain of evidential holism, whereas other quotes seem to express Quine’s views about revisability and/or what he describes as “the unit accountable to an empiricist critique” (1951, 42). In order to unambiguously answer the question whether Quine changed his mind about the scope of his holism, we need to distinguish between three ways in which holism might be extended to the whole of science, i.e. between three varieties of ‘wide-scoped holism’: maximal inclusion, universal revisability, and maximal integration.

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<sup>17</sup> One could question whether Quine already thought of FT as a trivial thesis when he, in “Two Dogmas”, claimed that “[a]ny statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system” (1951, 43). I believe he did. For in the paper, Quine never mentions the requirement that a language should stay semantically stable when we are making these ‘drastic adjustments’. Evidence for this interpretation can be found in the introduction to the first edition of *Methods of Logic*, written around the same time, where Quine considers the possibility that some revisions can better be thought of as “the adoption of a new conceptual scheme, the imposition of new meanings on old words” (1950, xiv). I thank Gary Ebbs for both suggesting this problem and its solution to me.

<sup>18</sup> Supposedly, in contrast to Duhem (1914, 182-8), who is generally read as restricting the scope of holism to physics. Personally, I have my doubts about this interpretation, but I will leave this issue for another time.

*Maximal inclusion:* The strongest form of wide-scoped holism is directly connected to the definitions of PT and FT above. These definitions state that only ‘clusters’ of theoretical sentences imply observation categoricals and that only ‘clusters’ of theoretical sentences are falsified whenever these predicted categoricals turn out to be false. A question that naturally arises is how large these clusters are. Or, as Quine phrases it:

holism [...] says that scientific statements are not separately vulnerable to adverse observation, because it is only jointly as a theory that they imply their observable consequences [...] If it is only jointly as a theory that the scientific statements imply their observable consequences, how *inclusive* does that theory have to be? (1975b, 228-9, my emphasis).

A wide-scoped holist in the first-sense would answer Quine’s question by claiming that the theory should be *maximally inclusive*. Or, in terms of PT and FT, the first type of wide-scoped holism claims that only science as a whole ‘will imply observation categoricals’ and that ‘whenever a predicted observation categorical turns out to be false’, only science as a whole is falsified. The wide-scoped holist in the *maximal inclusion* sense, in other words, believes that “our scientific system of the world [is] involved *en bloc* in every prediction” (1975a, 71, my emphasis).

*Universal revisability:* A weaker form of wide-scoped holism is connected to Quine’s seminal claim that ‘no statement is immune to revision’. If one believes this to be the core of Quine’s wide-scoped holism, one need not ascribe anything as strong as *maximal inclusion* to Quine. After all, universal revisability does not imply that every statement is always up for revision whenever a predicted observation categorical turns out to be false. It only implies that every statement is revisable *in principle*, and hence that every statement is required in implying *at least one* observation categorical. Or, in terms of PT and FT, universal revisability is an answer to the question of how many theoretical sentences are revisable in principle because they are involved in implying *at least one* observation categorical.

In order to grasp the distinction between *maximal inclusion* and *universal revisability*, note that the set of *inclusive* theoretical sentences in the sense specified above is a (non-strict) subset of the set of *revisable* theoretical sentences. After all, if all theoretical sentences are required in implying *all* observation categoricals (*maximal inclusion*), they will by definition

play a role in implying *at least one* observation categorical (*universal revisability*). The converse is not (necessarily) the case, as there might be theoretical sentences that play a role in implying only a few but not all observation categoricals. These theoretical sentences are in principle vulnerable to revision, but not *every* time a predicted categorical proves mistaken.

*Maximal integration*: A third type of wide-scoped holism is connected to the fact that science, at least institutionally, is divided into a great number of subdisciplines. As stated, holism as defined by PT and FT is a thesis about ‘the logical relation between theory and evidence’. This invites the question what one means by ‘theory’. Quine in ‘Two Dogmas’ speaks about “the totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic” (1951, 42). In principle, however, it possible to endorse *maximal inclusion* and/or *universal revisability*, while rejecting this picture of science. That is, one could defend a pluralistic picture of science but still hold that *maximal inclusion* and/or *universal revisability* are true for every one of the specific subdisciplines of science. An interpretivist in the philosophy of social science, for instance, could accept that natural and social science are separate enterprises with radically different aims, methods, and theories, yet accept that PT and FT apply to both the natural and the social sciences, even in the wide-scoped sense as defined by *maximal inclusion* and *universal revisability*.<sup>19</sup> Wide-scoped holism in the third sense, or *maximal integration*, rejects such a pluralistic picture and claims that all scientific subdisciplines form an integrated whole.<sup>20</sup>

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<sup>19</sup> To clarify, this interpretivist would defend four theses: two claims about the social sciences (PT<sub>S</sub> and FT<sub>S</sub>) and two claims about the natural sciences (PT<sub>N</sub> and FT<sub>N</sub>).

<sup>20</sup> Although *maximal integration* is related to the historically influential ‘unity of science’ thesis, I have refrained from dubbing the third sense of wide-scoped holism ‘unity of science’. The reason is that the phrase ‘unity of science’ has too strong ontological connotations. Indeed, when Quine himself writes about the ‘unity of science’, he is mainly talking about an ontological unity, i.e. about the “dream of an overarching, unifying fact of the matter” (1995b, 471). And although Quine endorses this drive “for an unified all-purpose ontology” (ibid.), *maximal integration* as specified above is not necessarily connected to these ontological considerations. That is, one could accept *maximal integration* without accepting ontological unity; for instance, when one believes that the sciences are only *methodologically* unified.

I have specified three ways in which one can attribute a wide-scoped holism to the early Quine. In ‘Two Dogmas’, Quine defends the claim that “the unit of empirical significance is the whole of science” (*maximal inclusion*), that “no statement is immune to revision” (*universal revisability*), and that both these claims apply to “the totality of our so-called knowledge or beliefs” (*maximal integration*). In the remainder of this paper, I will argue that Quine has never changed his mind about these varieties of wide-scoped holism after ‘Two Dogmas’.

## 5. Maximal Integration

Let me start with Quine’s views about the domain of PT and FT. Reconstructing the evolution of Quine’s ideas about *maximal integration* is not a terribly complicated affair. Quine is well-known for his use of metaphors that illustrate how the different subdisciplines of science should be viewed as an integrated whole: besides the “man-made fabric” and “field of force” metaphors in ‘Two Dogmas’, his “web of belief” and “Neurath’s boat” metaphors have been particularly influential. And although these images have slightly different connotations,<sup>21</sup> they all suggest that Quine has always been strongly committed to *maximal integration*.

This is not to say, however, that Quine refuses to accept that science is compartmentalized in practice, i.e. that he denies that most scientists in their everyday work are predominantly concerned with their own subdisciplines. To the contrary, when Quine explicitly comments on science’s “degree of integration” (1975b, 229), he accepts that science “is variously jointed, and loose in the joints in varying degrees” (ibid., 230). In practice, Quine maintains, “widely separate areas of science can be assessed and revised independently of each other” (1986d, 620).

Still, this “practical compartmentalization” (ibid., 620) does not affect Quine’s view that science is an integrated whole *in principle*. The reason why he believes this, becomes clear when we consider the ideas of Penelope Maddy, who has challenged Quine on this issue by defending a form of methodological pluralism. In her *Naturalism in Mathematics*, Maddy

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<sup>21</sup> For example, where the fabric metaphor seems especially suited to illustrate Quine’s rejection of the analytic-synthetic distinction (e.g. 1954, 132), the boat metaphor is mostly used to illustrate Quine’s naturalism (e.g. 1969, 126-7).

ponders the question on what basis mathematicians should accept or reject candidate axioms for set theory and argues that only intra-compartmental (in this case, intra-mathematical) norms should play a role:

Where Quine holds that science is not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method, [my] naturalist adds that mathematics is not answerable to any extra-mathematical tribunal and not in need of any justification beyond proof and the axiomatic method. Where Quine takes science to be independent of first philosophy, my naturalist takes mathematics to be independent of both first philosophy and natural science [...] in short, from any external standard. (Maddy 1997, 184)

The difference between Maddy and Quine shows how diverging views about *maximal integration* can have considerable practical ramifications. For, where Quine proposes to evaluate candidate axioms for set theory by submitting them “to the considerations of simplicity, economy, and naturalness that contribute to the molding of scientific theories generally” (1990a, 95), Maddy proposes that we set aside these scientific norms because mathematicians themselves appeal to norms “of a sort quite unlike anything that turns up in the practice of natural science: crudely, the scientist posits only those entities without which she cannot account for our observations, while the set theorist posits as many entities as she can, short of inconsistency” (Maddy 1997, 184).

In the wake of Maddy’s book, two questions have dominated debates about her strict distinction between science and mathematics: (1) *why* should mathematics be evaluated on its own terms? and (2) *to what extent* should mathematics be shielded from external influence?<sup>22</sup> Now, it is not my intention here to recap the debate that was spawned by Maddy’s book. Still, I think that the *communis opinio* on both questions today shows why Quine believes that science should be viewed as an integrated whole in principle, even if in practice most scientists are solely concerned with their own subdisciplines. For, in discussing the question why mathematics (and not, for example, astrology) should be evaluated on its own terms, many commentators have pointed out that mathematics deserves this privilege because it is, as Maddy admits, “staggeringly useful, seemingly indispensable, to the practice of natural

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<sup>22</sup> See, for example, Dieterle (1999), Hale (1999), Rosen (1999), Tennant (2000), Tappenden (2001), Decock (2002), and, for an overview, Paseau (2013).

science, while astrology is not” (Maddy 1997, 204). If mathematics, like astrology, had been a useless discipline, in other words, nobody would have felt the appeal of Maddy’s proposal. After all, no serious naturalist would deny that we can and should scientifically evaluate (and dismiss) astrological claims. Still, this very use of mathematics also shows why Maddy’s strict distinction between science and mathematics ultimately cannot be maintained, i.e. why people like Quine believe science ought to be viewed as an integrated whole in principle. For, as many commentators have argued, even if Maddy is right that mathematical decisions are solely made on the basis of intra-mathematical considerations *in everyday practice*,<sup>23</sup> it is mathematics’ use in the scientific enterprise as a whole that *ultimately* justifies why we consider it to be such a tremendously valuable enterprise in the first place, meaning that, when push comes to shove, scientific norms should also play some role in mathematical decisions:

as Maddy herself points out [...] mathematics is ‘staggeringly useful [...] to the practice of natural science’. One might add also: to engineering and technology; to medical diagnostics; to the financial markets; to actuarial science; and to a host of other areas of human activity in which everyone’s interests and concerns are engaged. So [...] those outside the community of professional mathematicians have a permanent and legitimate concern in the nature of the norms governing the latter’s practice. In a word: mathematicians cannot be allowed to be a law unto themselves. What they do is too important, and ought to be subject to outside constraints designed to protect everyone’s interest [...] For Quine, it is the evidential holism in our theory of nature that truly naturalizes other areas of thought, such as mathematics. Natural science as a whole has to be understood in its own terms. To be anti-holistic, and separate mathematics off from science as a whole, as Maddy does, is to divert the springs of naturalism at their very source.<sup>24</sup> (Tennant 2000, 238-30)

Maximal integration, in sum, is not a thesis about scientific practice, it is a claim about the ultimate justificatory structure of science; even if in everyday practice mathematical

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<sup>23</sup> Something that can be doubted as mathematicians sometimes *are* influenced by extra-mathematical considerations. See Hale (1999, 395) and Paseau (2013, §5.1).

<sup>24</sup> See also Dieterle (1999), Rosen (1999), and Tappenden (2001). For an evaluation of Maddy’s reply to these arguments, see Verhaegh (2015, ch. 6). It should be noted that defining the value of mathematics in terms of its use in science has consequences for the inapplicable parts of mathematics. Quine has vacillated quite a lot on this issue. See, for example, Quine (1986c, 400; 1995, 56).

decisions are made on the basis of intra-mathematical considerations, in periods of crisis mathematicians have to appeal to scientific standards, because it is mathematics' use in science that, in the end, justifies why we consider mathematics to be such an immensely valuable practice. Or, as Quine puts it in *Pursuit of Truth*—this time using one of Wittgenstein's metaphors—science in its entirety defines “a particular language game [...] the game of science” (1990a, 20). The subdisciplines of science form an integrated whole because they are all ultimately part of the same game, because they all contribute to the same goal, maximizing prediction and understanding. It is because he views all the subdisciplines of science as part of this one single game, that Quine, throughout his career, has cherished *maximal integration* as a regulative ideal.

## 6. Universal revisability

Let me turn to the second type of wide-scoped holism distinguished above. Where Quine remains strongly committed to *maximal integration* after ‘Two Dogmas’, he *does* seem to change his mind about *universal revisability*. Indeed, where the early Quine argues that “no statement is immune to revision” and that “[r]evision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics”, in *Pursuit of Truth*, Quine seems to explicitly argue that logical truths are exempted from revision:

Over-logicizing, we may picture the accommodation of a failed observation categorical as follows. We have before us some set S of purported truths that was found jointly to imply the false categorical [...] Now some one or more of the sentences in S are going to have to be rescinded. *We exempt some members of S from this threat on determining that the fateful implication still holds without their help.* Any purely logical truth is thus exempted, since it adds nothing to what S would logically imply anyway. (1990a, 14, my emphasis)

Quine, in other words, argues that logical truths are not on a par with ordinary scientific statements. Even if we were to decide to remove a logical truth  $\sigma$  from our belief set in the light of an unexpected experimental result, this would be a pointless manoeuvre. For  $\sigma$  would

simply pop up again as it is directly implied by S's underlying logic. No matter how often one decides to rescind  $\sigma$ , one will never get rid of it because it follows from the empty set.<sup>25</sup>

Despite appearances to contrary, however, Quine's slightly altered perspective on logical truths here does not imply that logic is unrevisable. For it might still be possible to revise the underlying logic that causes  $\sigma$  to pop up every time; if  $\sigma$  automatically follows from one's underlying logic L, then it might still be possible to revise  $\sigma$  via a revision of L, even though it is impossible to revise  $\sigma$  directly.<sup>26</sup> This is certainly not a concession to *universal revisability*, as similar situations occur in other fields of inquiry.<sup>27</sup>

Nothing in the above quoted passage thus directly implies that, when it comes to *universal revisability*, Quine makes a substantive exception for logic; indirect revisions of logic remain a possibility. Still, the question whether or not Quine allows such modifications has been a matter of some debate. For Quine sometimes seems to suggest that logical principles cannot be revised even in this indirect way; for example, when he claims that the very idea of someone accepting a sentence of the form 'p and not p' is "meaningless" (1954, 109), or when he claims that logic is analytic in that a deviant logician who "tries to deny ['p and not p']", only "changes the subject" (1970, 81).<sup>28</sup>

It is sometimes thought that these ideas about logic are a consequence of Quine's late view that it *is* possible—*pace* "Two Dogmas"—to defend a limited notion of analyticity. For Quine, from *The Roots of Reference* (1973) onwards, has argued that some sentences *can* be called analytic: if every member of a language community learns that a certain sentence is true by learning how to use one or more of its component terms, then there are no obstacles toward counting the sentence as true in virtue of meaning.<sup>29</sup> Central to Quine's early holism

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<sup>25</sup> See also Quine (1994, 431): "Even mathematical truths share [...] in the empirical meaning of sciences where they are applied [...] This cannot be said of logical truths. Any sentence already implies any logical truth, and thus gains no further implying power by being conjoined with it".

<sup>26</sup> One could, for instance, modify L's underlying consequence relation. See Tamminga and Verhaegh (2013).

<sup>27</sup> E.g. certain low-level empirical laws too will have to be 'exempted from revision' if the general laws from which are derived are not modified.

<sup>28</sup> See also Quine (1991, 396): "Anyone who goes counter to *modus ponens*, or who affirms a conjunction and denies one of its components, is simply flouting what he learned in learning to use 'if' and 'and'".

<sup>29</sup> See Quine (1973, §21). Quine's views here are partly influenced by Putnam's (1962) ideas about one-criterion words. See Quine (1960, §12; 1986d, 427).

was the idea that we cannot strictly distinguish between changes of theory and changes of meaning.<sup>30</sup> Quine's renewed ideas about analyticity, however, partly go against this view in acknowledging that a change of logic *can* be a pure change of meaning. For if 'p & q  $\vDash$  p' is analytic in Quine's sense, then to deny this logical law is simply to change the meaning of 'and', not to propose a change of theory.

In response to these new Quinean ideas about analyticity, scholars have distinguished between what Arnold and Shapiro (2007) call a 'logic-friendly' and a 'radical' Quine:

It is sometimes said that there are two, competing versions of [...] Quine's unrelenting empiricism [...] [The] *logic-friendly* Quine holds that logical truths and, presumably, logical inferences are analytic in the traditional sense: they are true solely in virtue of the meaning of the logical terminology. Consequently, logical truths are knowable a priori, and, importantly, they are incorrigible, and so immune from revision. No amount of empirical data can get us to revise them. [...] The other, *radical* version of Quine does not exempt logic from the attack on analyticity and a priori. Logical truths and inferences are themselves part of the web of belief, and the same methodology applies to logic as to any other part of the web [...] Everything, including logic, is up for grabs in our struggle for holistic confirmation.<sup>31</sup> (Arnold and Shapiro 2007, 276-7)

There has been considerable controversy about the extent to which the logic-friendly and the radical Quine are compatible, and if not, which one of these characters best approaches the 'real Quine'.<sup>32</sup> With one exception, however, all scholars (including myself, see Tamminga and Verhaegh, 2013) have overlooked the fact that Quine has answered this question explicitly on two occasions.<sup>33</sup> According to Quine, the two perspectives on logic are perfectly compatible because his renewed talk about analyticity does not have any significant

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<sup>30</sup> Recall Quine's letter to Grünbaum: "I am not concerned even to avoid the trivial extreme of sustaining a law by changing a meaning; for the cleavage between meaning and fact is part of what, in such contexts, I am questioning" (1962, 132).

<sup>31</sup> See also Haack (1977), who distinguishes between a 'conservative' and a 'radical' Quine; and Parent (2008), who contrasts a 'Principle of Logical Charity' with a 'Revisability Doctrine'.

<sup>32</sup> See Haack (1977), Shapiro (2000, 334), Burgess (2004), Maddy (2005, 443), Weir (2005, 463), Arnold and Shapiro (2007), Parent (2008), and Tamminga and Verhaegh (2013).

<sup>33</sup> See Quine (1968; 1990b). The exception I am referring to is Parent (2008). Still, even he takes into account only one of these two papers.

epistemological consequences. Even if changes of logic are now viewed as changes of meaning, the fact that logical laws are analytic does not have any implications for Quine's views about the epistemological status of logic and, hence, for *universal revisability*. For, Quine argues, if we are unwilling to treat a logical revision as a change of theory, this only reflects how deeply embedded the laws of logic are in our system of beliefs; the maxim of minimum mutilation still suffices to account for the ground of logical truth.

Quine's point here can be illustrated using his ideas about translation. Consider a lexicographer who aims to translate a native tribe's language into English; and suppose that all members of the tribe are inclined to assent to 'q ka bu q', which seems to mean 'p and not p' if the linguist would follow the translation manual she has drawn up thus far. Now, in response to this situation, the linguist has at least two options. She can either stick to her earlier conclusions and interpret the natives as accepting contradictions; or she might take the natives' utterances as evidence that her existing translation manual cannot be correct. Now, according to Quine, it would be absurd to choose the former option:

if any evidence can count against a lexicographer's adoption of 'and' and 'not' as translations of 'ka' and 'bu', certainly the natives' acceptance of 'q ka bu q' as true counts overwhelmingly. We are left with the meaninglessness of the doctrine of there being pre-logical peoples; pre-logicality is a trait injected by bad translators. (1954, 190)

If Quine is right, then we cannot but conclude that the natives agree with us when it comes to logic. The very idea of an empirical observation that would justify the lexicographer in ascribing to them an alternative framework is ruled out from the beginning. Yet (and here lies the explanation of why Quine deems his renewed account of analyticity to be epistemologically irrelevant) the fact that we choose to interpret the natives as agreeing with us when it comes to logic, does not tell us anything about the *ground* of logical truth. We only interpret the native in terms of our logic because it is a basic *pragmatic* rule to interpret one another as charitably as possible:

What is interesting to ponder is the connection between this rigidity of logic in translation and the question of the immunity of logic to revision [...] generally, we are well advised in translation to choose among our indeterminates in such a way, when we can, that sentences which natives assent to as a matter of course become translated into English sentences that likewise go without saying. This policy is regularly reflected in domestic communication:

when our compatriot denies something that would seem to go without saying, we are apt to decide that his idiolect of English deviates on some word [...] We see, then, how it is that ‘Save logical truth’ is both a convention and a wise one. And we see also that it gives logical truths no epistemological status distinct from that of any obvious truths of a so-called factual kind.<sup>34</sup> (1968, 317-8)

Confronted with the objection that his ideas about the relation between logic and translation appear to be in conflict with *universal revisability*, therefore, Quine responds by showing that the two theses can be easily combined. His views concerning the interpretation of deviant logicians (both domestic and abroad) are not intended to express anything fundamental about the epistemological status of logic. Rather, Quine only intends to show that translation practices are constrained by the principle of charity, a principle closely related to the maxim of minimum mutilation in theory revision; in updating our theories and in making sense of one another we are inclined to ‘save the obvious’, nothing more, nothing less. Obviousness is not a trait that is exclusive to logic, nor does the fact that a truth is obvious imply that it cannot be revised: “Obviousness resists change but does not preclude it” (1990b, 36).<sup>35</sup>

## 7. Maximal inclusion

Let me briefly recap the main conclusions of our discussion thus far. I have shown that Quine’s evidential holism is a thesis about the relation between theory and evidence as described by PT and FT and that his ideas about the *scope* of evidential holism ought to be

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<sup>34</sup> See also Quine (1990b, 36): “Is change of logic a change of language, or is it a change of substantive theory on a par with changes in physics [...] I have seemed to oscillate between those positions. But are they really two positions? If someone persists in a simple logical falsehood, we do indeed conclude that he has mislearned our language or is tampering with it. But this is equally the way with any obvious falsehood, logical or not”.

<sup>35</sup> In response to the above sketched dilemma of Arnold and Shapiro, we can thus say that the ‘logic-friendly Quine’ and the ‘radical Quine’ are perfectly compatible. Their description of these ‘two Quines’ ought to be adapted however. For although the logic-friendly Quine argues that logic is analytic, he does not believe that it is “analytic in the traditional sense”, nor does he believe that logical truths “are knowable a priori” or that they are “incorrigible, and so immune from revisions”. Conversely, the radical version of Quine *does* “exempt logic from the attack on analyticity”.

divided into three sub-theses: *maximal inclusion*, *universal revisability*, and *maximal integration*. I have argued that although Quine seems to have changed his mind about the latter two claims—revising his ideas about analyticity and admitting that science is compartmentalized in practice—these modifications have not affected his commitment to either *universal revisability* or *maximal integration*. Quine still maintains that “that logic is integral to our system of the world and accessible to change in the same way as the rest” (1990b, 36) as well as that the compartmentalization of science is “a matter of practice rather than principle” (1986d, 620).

Now, let me finally turn to *maximal inclusion*, the strongest type of wide-scoped holism Quine defends in “Two Dogmas”. *Prima facie*, it seems obvious that Quine *has* changed his mind about this issue. Consider, for example, the following passages:

Looking back on [“Two Dogmas”], one thing I regret is my needlessly strong statement of holism [...] In later writings I have invoked not the whole of science but chunks of it, clusters of sentences just inclusive enough to have critical semantic mass. By this I mean a cluster sufficient to imply an observable effect of an observable experimental condition. (1991, 393)

When we look thus to a whole theory or system of sentences as the vehicle of empirical meaning, how inclusive should we take this system to be? [...] modest chunks suffice, and so may be ascribed their independent empirical meaning. (1975a, 71)

I see science as a considerably integrated system of the world [...] But we can appreciate this degree of integration and still appreciate how unrealistic it would be to extend a Duhemian holism to the whole of science, taking all science as the unit that is responsible to observation. (1975b, 229-30)

In all these passages Quine is discussing *maximal inclusion*; and in all these passages Quine seems to retract his ‘Two Dogmas’ claim that ‘the unit of empirical significance is the whole of science’. Rather than claiming that science as a whole is falsified whenever an observation categorical turns out to be incorrect, Quine now seems to maintain that science is not *maximally* inclusive; chunks of theory suffice to imply observation categoricals and, as a result, only chunks of theory are falsified whenever these predictions turn out to be false (in what follows, I will call this adapted thesis *moderate inclusion*).

Yet, Quine did *not* change his mind about *maximal inclusion*. Rather, as I will argue in this last section, Quine believes that *maximal inclusion* is a strictly true but ultimately uninteresting thesis. That is, I will argue that although Quine does not believe *maximal inclusion* to be false, he does want to shift the focus towards the aspects of holism that he thinks *are* important.

As a start, let us consider how the above three passages continue:

Looking back on [“Two Dogmas”], one thing I regret is my needlessly strong statement of holism. ‘The unit of empirical significance is the whole of science’ [...] *This is true enough in a legalistic sort of way, but it diverts attention from what is more to the point: the varying degrees of proximity to observation.* (1991, 393)

When we look thus to a whole theory or system of sentences as the vehicle of empirical meaning, how inclusive should we take this system to be? [...] *It is an uninteresting legalism [...] to think of our scientific system of the world as involved en bloc in every prediction.* (1975a, 71)

we can [...] appreciate this degree of integration and still appreciate how unrealistic it would be to extend a Duhemian holism to the whole of science [...] *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.* (1975b, 230).

In all three instances, Quine does not reject *maximal inclusion* because it is false, but because it is somehow unimportant: he claims that ‘it diverts attention from what is more to the point’, that it is ‘uninteresting’ and that ‘little is gained’ in advancing the thesis. Furthermore, instead of claiming that *maximal inclusion* is false, in all three passages Quine claims that the thesis is true (or at least defensible) ‘legalistically’.<sup>36</sup> Quine’s point is the same as the one we have encountered on a few occasions above: although *in practice* relatively small chunks of theory are inclusive enough to imply an observation categorical, *logically* science as a whole is involved. After all, even if one deduces an observation categorical from a small chunk of

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<sup>36</sup> See also, Quine’s (1986d, 427): “I see extreme holism itself as ‘pure legalism’”; (1970, 5): “Legalistically, one could claim that evidence counts always for or against the total system, however loose-knit, of science”; and (1986e, 620): “Holism at its most extreme holds that science faces the tribunal of experience [...] as a corporate body [...] Legalistically this again is defensible”.

theory, one could still decide to save the sentences in the chunk when the categorical turns out to be false; for example by revising the logic that is shared by all the chunks or by revising the rules of one's language. In this respect, Quine's claim that *maximal inclusion* is an 'uninteresting legalism' is on a par with his earlier claim that he agrees with Grünbaum that FT is 'a thoroughly unenlightening truism'. In other words, although relatively small chunks of theory will imply observation categoricals *given* a certain language and a certain logic, Quine's believes it to be legalistically true that only science as a whole implies observation categoricals when one takes into account the scientist's freedom to revise her logic and/or her language in the light of adverse experience. And although it is trivially true that one can revise anything whatsoever if one is willing to modify the rules of one's language, this is exactly why Quine believes *maximal inclusion* to be 'true in a legalistic sort of way'.<sup>37</sup>

So Quine does not believe *maximal inclusion* to be false. A question that remains to be answered, however, is why he so strongly emphasizes *moderate inclusion* (his chunk-view) in later work. I believe that Quine backs away from *maximal inclusion* in later stages of his career, because it is only then that he starts to realize that *maximal inclusion* does not do any epistemological work in his philosophy. Rather, Quine realizes, *universal revisability* and *moderate inclusion* suffice.

To see this, consider the two dogmas Quine wants to dismiss in "Two Dogmas of Empiricism". The first dogma Quine aims to reject is the analytic-synthetic distinction. In section 6 of his paper, Quine argues for the conditional claim that *if* his holistic picture of inquiry is correct, "it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements which hold come what may". This is folly, according to Quine, because on the holistic picture "[a]ny statement can be held true come what may" and, conversely, "no statement is immune to revision" (1951, 43). Now, for Quine's argument here to be valid, he does not require *maximal inclusion*; his more modest 'chunk-view' is sufficient to establish that one can hold any statement true come what may. After all, if a relatively small chunk of theoretical sentences is always involved in implying an observation categorical, one has the logical freedom to decide never to revise a particular

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<sup>37</sup> Note that Quine has not changed his mind about this issue either. For not only had Quine already admitted to Grünbaum that FT is a 'thoroughly unenlightening truism', also in the first edition of *Methods of Logic* (1950), Quine already recognizes that some radical revisions can be better treated as changes of language, as we have seen in footnote 17.

hypothesis; there will always be at least some auxiliary hypotheses that can be blamed whenever a prediction turns out to be false. Quine *does* need *universal revisability*, however, if he wants to maintain that ‘no statement is immune revision’. For if Quine would moderate his view about revisability, he could obviously not maintain that any statement is revisable in principle.

Quine in sum, only needs *universal revisability* and *moderate inclusion* in his argument against the analytic-synthetic distinction.<sup>38</sup> A different way to make the same point is to ask why the analytic-synthetic distinction was so important to Carnap and the logical positivists. As Quine views the matter, Carnap required the analytic-synthetic distinction in order to be able to solve an age-old problem for empiricism, namely the problem of how to explain our logical and mathematical knowledge. In arguing that our logical laws and mathematical statements are analytic, the positivists could maintain that these statements are meaningful while remaining faithful to the empiricists’ core idea that all our knowledge about the world originates in sense experience. After all, if logical and mathematical statements are true in virtue of meaning, then they do not claim anything about the world.<sup>39</sup>

Now, according to Quine, Carnap’s need for an analytic-synthetic distinction is caused by the second dogma, ‘radical reductionism’.<sup>40</sup> For if all synthetic statements can be tested in isolation, there will be statements, i.e. our mathematical and logical statements, that are never tested and hence statements which are meaningless from a strictly empiricist point of view. Yet when we give up on the second dogma (and embrace holism), we can explain why our logical and mathematical statements are meaningful without appealing to the notion of analyticity; they are meaningful because they play an important role in squaring theory with evidence.<sup>41</sup>

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<sup>38</sup> This remains true when he slightly changes his views about analyticity in the 1970s (see section 6), because Quine, in these later stages of his career, still aims to undermine epistemologically inflated versions of the analytic-synthetic distinction. See Hylton (2002).

<sup>39</sup> See Quine (1988, 26): “How, Carnap asked, can mathematics be meaningful despite lacking empirical context? His answer was that mathematics is analytic”.

<sup>40</sup> Quine’s interpretation here is dubious, as Carnap seems to reject radical reductionism in his *The Logical Syntax of Language* (1934, 318). I will limit myself here to Quine’s interpretation, however, acknowledging that this reading might not do justice to the historical Carnap.

<sup>41</sup> See Quine (1986a, 206-7): “the second dogma creates a need for analyticity as a key notion of epistemology, and [...] the need lapses when we heed Duhem and set the second dogma aside. For given the second dogma, analyticity is needed to account for the meaningfulness of logical and

Now, when we ask the question what types of holism Quine needs in order to dismiss the second dogma, again the answer is that *maximal inclusion* is not required. After all, in order to show that mathematical and logical truths play an important role in squaring theory with evidence, Quine does not need to maintain that they are involved in implying each and every observation categorical. If they are involved in implying at least a few categoricals, and hence are revisable in principle, he can legitimately claim that we do not require analyticity in the Carnapian sense in order to justify our logical and mathematical knowledge.<sup>42</sup>

Although Quine believes *maximal inclusion* to be true legalistically, in other words, he has no reason to advance the thesis as such. Both dogmas Quine wants reject turn out to be false if weaker versions of his wide-scoped holism—viz. *moderate inclusion* and *universal revisability*—are true. Just as Quine has never changed his mind about *maximal integration* (section 5) and *universal revisability* (section 6), in sum, he has never changed his mind about *maximal inclusion*; he just literally believes it to be ‘needlessly strong’.

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mathematical truths, which are clearly devoid of empirical content. But when we drop the second dogma and see logic and mathematics rather as meshing with physics and other sciences for the joint implication of observable consequence, the question of limiting empirical content to some sentences at the expense of others no longer arises”. See also Quine’s unpublished manuscript entitled “Afterthoughts”, in which he calls the second dogma “the real villain of the piece” (1987, 10, my transcription).

<sup>42</sup> See Quine (1988, 27): “Once we appreciate holism, *even moderate holism*, the notion of analyticity ceases to be vital to epistemology”.

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