

## PUTNAM VERSUS QUINE ON REVISABILITY AND THE ANALYTIC–SYNTHETIC DISTINCTION<sup>1</sup>

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### I. Introduction

In the following, I want to revisit the criticism of the analytic–synthetic distinction brought forth in Putnam’s “The Analytic and the Synthetic” and later writings and compare it with Quine’s familiar attack on the same distinction in the last two sections of “Two Dogmas of Empiricism”. My purpose in this comparison will be to find the features of Putnam’s argument that prepare the ground for his increasingly explicit acknowledgment of the utility of a successor distinction, which sharply separates his pragmatic, induction-based epistemological commitments from Quine’s naturalistic rejection of any such project. The following brief review of some doctrinal agreements vis-à-vis logical empiricism will set the stage for specifying the issue between Putnam and Quine regarding the epistemic analytic–synthetic distinction.

Following Quine, Putnam’s early writings on the analytic–synthetic distinction criticize the attempt undertaken by logical empiricism (especially Carnap and Ayer) to use a precise and general distinction between analytic and synthetic statements for empirically contentful systems of statements for the purpose of giving a ‘demystified’ account of the special status, interpretation, empirical meaningfulness and justifiability of statements (such as the laws of logic and mathematical truths, statements of semantics, and theoretical statements in scientific theories) that had traditionally resisted – since supposedly independent of experience in content and justification – ready integration into a broadly empiricist methodological outlook. The key to solving all three of the mentioned problems was to first identify apriority, necessity and analyticity, interpret the latter as ‘truth in virtue of meaning’, and then to specify for any empirically contentful language a

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precise class of analytic statements that gave as much traditional apriorities and necessities (which linguistified the *a priori*) as would be strong enough to explain the interpretability and acceptability-conditions of theoretical statements in terms of observation-statements.<sup>2</sup> Given the underdetermined character of the latter and the multiple options in the choice of linguistic frameworks of empirical justification, conventionalism had the remaining task of explaining the apparent necessity of logic and mathematics (as the choice to retain statements) and the acceptance and meaningfulness of non-observational statements (as the choice of theoretical frameworks for systematizing empirical knowledge). According to conventionalism and linguistified apriority, such choices concern the presuppositions of empirical argument. Since, as presuppositions, they cannot be forced on us by empirical arguments the success of which would have to already presuppose them, their acceptability and actual acceptance has to be due to other sources. Conventionalists overcome the psychologism of their predecessors by attributing these facts of selective, empirically underdetermined acceptance to social agreements on the analyticities to adopt. Likewise, and in parallel fashion, Carnap, Lewis and Ayer believed that the acceptance and empirical meaningfulness of observationally underdetermined theories could be explained as reflecting collective choices as much about connecting principles between observation and theory as about rivals that, given such principles, equally well accommodate the available empirical data. Regarding the explanation of apriority and necessity, it is now our commitment to certain agreed-upon norms of inquiry that explains why the statements expressing these norms are stable under all circumstances as long as the language and framework we use remains the same. This is the thrust of a conventionalist-linguistic explanation of mathematical and logical necessity.<sup>3</sup> Quine and Putnam both reject the idea of taking some statements (many of which are extra-logical) in science as true on merely communal *fiat* instead of empirical argument as incompatible with the empirical nature of the requirements on scientific judgment. Consequently, both oppose the linguistic conception of the substantive *a priori*<sup>4</sup> as much as conventionalism.

As to the first, Putnam is as critical as Quine of the proposal to understand epistemic special statuses, insofar as they are present in systems of empirical belief, on the model of linguistic stipulations (analyticities), changing or endorsing which is rather a matter of linguistic legislation than of empirical information and argument. On well-known holistic grounds, Quine and Putnam portray this project as an artifact of the requirements imposed by positivistic methodology that has no reasonable basis in the way statements are (epistemically or semantically) assessed in actual scientific practice. The main reasons for this stem from the inseparability of linguistic and empirical information within an empirically contentful system of knowledge. Such inseparability puts into question the idea of there being an identifiable subset of specific statements of the system that merely codifies a special,

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non-empirical kind of ‘knowledge of meaning’. The particular lines of thought undergirding these critical considerations are three. First, in virtue of the multiple inferential relations of beliefs within these systems, it is futile to non-arbitrarily isolate some of the system’s accepted statements as ‘truths in virtue of language’ if they are *logically* candidates for revision in case of conflicts between the system and incoming statements. Second, it is futile to try to trace the empirical significance or testability of statements that employ certain expressions to a given class of statements that allegedly have the sole function of specifying the application conditions of these expressions, since the application of any statement (also the meaning-specifying conditions) requires many stated and tacit auxiliary empirical assumptions which are, since required for application, equally *part of the application* conditions of the expressions. Third, given this inseparability and auxiliary-dependence of empirical applicability, the explanation of conventional-linguistic *constraints* on empirical knowledge would have to portray ‘knowledge of meaning’ as strong enough to constitute *empirically significant* (hence applicable) constraints, and thus would always also articulate ordinary empirical beliefs. But then such knowledge is vulnerable to changes in empirical belief like ordinary empirical knowledge. For these reasons, we fail to obtain the supposed explanans for epistemic priority, necessity and the empirical significance of empirically underdetermined theoretical statements. Consequently, in these writings, Putnam also agrees with Quine that it is an error to regard the ability to draw a fixed dichotomy between analytic and synthetic statements as important for our understanding of the empirical justification of hypotheses. This endorsement of the mentioned doctrines reflects Putnam’s commitment to two main methodological tenets that he shares with Quine, holism and the acknowledgement of the indefinite extent to which even deeply entrenched (‘a priori’) belief is revisable in generalized scientific terms under changes in empirical knowledge, which we could call ‘fallibilism’ or ‘(indefinitely) general *revisability*’. Both are customarily taken to undermine by themselves the analytic–synthetic distinction.

But, as I mentioned above, these agreements in doctrine do not issue in an agreement of the lessons, and it is in their positions vis-à-vis conventionalism that the differences take shape. While Quine concludes that drawing any kind of analytic–synthetic distinction within the body of our empirical knowledge is “folly”,<sup>5</sup> and that it “has been given no tolerably clear meaning even as a methodological ideal” (Quine 1956: 132, emphasis added) Putnam claims that drawing a successor distinction “is of logical and methodological significance” (Putnam 1962a: 249) and warns against the “danger of denying its existence altogether” (Putnam 1962: 33). At issue is the methodological significance of an *epistemic* analytic–synthetic distinction, i.e. the significance of an *apriori*–*aposteriori* distinction, given holism and general *revisability*.

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Succinctly put, Quine's point is that, given confirmation holism, general *revisability* and a standard empiricist view of the relation between hypothesis and observation, there is and ought to be no epistemically significant analytic–synthetic distinction. Now, Quine's wholesale abandonment is often seen (and portrayed by Quine) as a radicalization of empiricism in reaction to the identification of analytic truths with *conventions* by empiricists that I just described. Methodologists as diverse as James, Lewis, Poincare, Carnap and Reichenbach welcomed conventionalism on empiricist grounds as a 'demystified' (Friedman 1999: 64) reconstruction of the traditional notion of objective necessity: if statements are held true merely by convention and not held vulnerable to empirical (or other) evidence, then this reflects *our public agreement* to not at the same time hold them as possibly falsified by anything, and access to the set of necessities is just as easy to explain as the consequence of an *actual* and *public* act: we simply choose them or commit ourselves to them as fixed guiding principles (cf. Lewis 1923: 15–16). But according to Quine, precisely the empiricist outlook that produced conventionalism ought to prevent the acceptance of *any* empirically immunized assumptions.<sup>6</sup> On his apparently more radical, naturalized empiricism, our decision to *abandon* the search for epistemic analyticity altogether is the final missing move to license the conviction that 'all statements are epistemologically on a par'. On a par, that is, in being *empirical*.<sup>7</sup>

As I indicated before, Putnam also rejects conventionalism. Furthermore, the motivation for his rejection is the one just mentioned, viz. the view that scientists ought not to be construed as routinely making gratuitous empirically unfounded or immunized assumptions when they are in fact considering the empirical merits of alternative theoretical proposals. But Putnam does not claim that all statements are always and under all circumstances on a par. His point is rather that, given holism and general *revisability*, confirmation does neither require nor allow, as Carnap suggests, a *general and precise*, but only a *contextual and unpredictable* (i.e. informal) distinction between epistemic priorities. According to Putnam, we ought to reformulate epistemic analyticity or apriority as a *contextual and empirically sensitive* notion. This defuses conventionalism in a different way. According to this view, what is an unquestioned guide in inference and epistemic evaluation ('convention') in one context can, without change in meanings or language, appear as a factual assumption in need of empirical support in another. Whether or not it appears as one or the other in a context is likewise a matter of empirically informed argument. Therefore, no acceptance or rejection of scientifically relevant statements or norms can expect to rationally convince without attending to a lot of available empirical information. While the meta-methodological argument just attributed to Quine tends to turn on a philosophical doctrine about how to be a better empiricist, this line of argument emphasizes the *pragmatics* of scientific justification and the normative expectations regarding the sort of reasons in these practices. Thus, Putnam

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and Quine indeed both reject conventionalism, but on markedly different reasons: Putnam's *pragmatic* rejection requires a flexible distinction of epistemic priorities to make sense, while Quine's *naturalism* claims to do without any. Why would Putnam not have it the simpler, Quinean way? I will suggest that one important reason why a Putnamian, as opposed to a Quinean, would find a successor distinction methodologically *important* (as opposed to merely feasible) is that forcing its abandonment on Quine's reasons requires accepting a thesis that makes a form of confirmation skepticism inescapable and thus results in the collapse of empirical sensitivity for our system of knowledge and belief. However, this would sacrifice precisely the worthiest motive for Quine's suggestion: that changes in empirical information have epistemic effects on the statements we accept, and that we ought not in advance select the points in our system of beliefs where these effects will be noted. Putnam's position can then be seen as an articulation of conditions on empirical belief fixation that avoids this collapse without fear of a successor distinction to the epistemic analytic–synthetic distinction. I will therefore concentrate on the differences between Quine and Putnam in this regard.

For the sake of reconstructing the rift between both philosophers, I will take the constraints of a possible position to be set by the doctrinal agreements reported so far.<sup>8</sup> Thus, avoiding the danger Putnam warns us of has to be

- (a) compatible with the insights of a holistic and fallibilistic methodology (i.e. holds even the *a priori* parts of knowledge sensitive to changes in empirical information<sup>9</sup>) (Quine's lesson) but has to
- (b) deliver a distinction (the point in dispute) that
- (c) forms part of any successful account of the justificatory and epistemic practices of good scientists (Quine's point against imposing a distinction without a difference in practice), such that the result
- (d) does not entail a collapse of empirical sensitivity (Quine's problem).

The following discussion will thus move on a mainly *methodological* level.<sup>10</sup> The question is whether the epistemic features of practices of theory-acceptance that endorse holism and general *revisability* allow or even require a successor distinction to the analytic–synthetic distinction. Framing the issue in this fashion engages the two positions at precisely the level at which Quine, far from simply brushing analyticity aside as old-fashioned positivism, came to see a question of vital importance for his own naturalistic agenda.<sup>11</sup> As he writes: “the philosophically important question about analyticity ... is ... relevance to epistemology”, whereby the restatement of his position runs like this: “a need for analyticity as a key notion of epistemology ... lapses when we heed Duhem” (Quine 1986a: 207). The following considerations thus also aim at clarifying some contrasts between naturalist and pragmatic methodologies.

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My discussion will follow these steps. I first want to review what I take to be the central move in Quine's recommendation to abandon *any* analytic–synthetic distinction, semantic or epistemic, because of its methodological irrelevance. I will argue that this move rests on a contention that is independent of either holism or fallibilism but symptomatic for Quine's brand of empiricism, and which naturally issues in certain forms of skepticism (II). In the following section (III), I will present those parts of the methodological program that Putnam outlines in "The Analytic and the Synthetic" and subsequent writings that allow bypassing the impasse of Quine's wholesale repudiation of the analytic–synthetic distinction. The crucial idea of this program is to exploit the fact that in actual confirmation practice we evaluate the comparative inductive support for hypotheses on given evidence. Moreover, this inductive approach models what is given as evidence as not hypothetical at the same time as it is used for testing the hypothesis. Such a distinction between hypothetical and non-hypothetical elements in confirmation allows various contextual distinctions between epistemic statuses for parts of our knowledge that satisfy (a)–(d). The result will disallow any principled arguments against minimal degrees of revisability, ensure anti-skepticism, and it will better accommodate evidence that is left out of Quine's picture.

## II. Quine's principled argument for rejecting of the distinction

The connection Quine wishes to draw between revisability and the epistemic analytic–synthetic distinction is expressed in the most succinct form in the most famous passage from Quine's "Two Dogmas", where he says:

Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. ... Conversely, by the same token, no statement is immune to revision. ... A recalcitrant experience can ... be accommodated by any of various alternative reevaluations ... of the total system.

(Quine 1951: 43–44)

This is surely one of the most puzzling, but also one of the most symptomatic passages in Quine's work. Putnam, for one, has called it at different times a "trenchant paragraph" (Putnam 1983b: 91) and seen it as shot through with "unintelligible" uses of 'can' (Putnam 1994a: 256). It has been called the "holism argument" (Berger 2003: 373) and the "revisability passage" (Fodor/LePore 1992). In fact, this suggests two quite distinct roots of the claims, as we will see.

Of course, the passage does occur in the context of Quine's holistic account of the relation between experience and hypothesis that is analogous to points earlier discovered by Duhem. Holism regarding the empirically

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based acceptance of theories (confirmation holism, for short) arises on acceptance of *Duhem's thesis* that deriving observationally testable predictions from theoretical hypotheses requires taking for granted many additional assumptions from a system of belief, all of which and the hypothesis together imply what either agrees or conflicts with what we actually observe (cf. Duhem 1912: ch. VI, §2, esp. 185ff.). This is often expressed by saying that 'no statement is tested in isolation',<sup>12</sup> a thesis that both Putnam and Quine accept. In "Two Dogmas" Quine even exaggeratedly speaks of the "whole system of science" as the unit of empirical significance, a claim he later retracts. Once this thesis is accepted, one is also committed to the following *logical point*, which Duhem called the '*ambiguity of falsification*': In the event of an unexpected experience, i.e. one that conflicts with a given prediction, the inference by *modus tollens* is to the falsity of what implied the mistaken prediction. It thus affects not only the hypothesis-statement, but the disjunction of the hypothesis itself along with everything else that contributed to the derivation of the prediction. The upshot of accepting confirmation holism is twofold: (1) the selection of any one involved statement over others for revision is compelled by neither logic nor observational evidence alone but requires an *additional judgment*, and (2) all involved statements are *logically* exposed to the threat of being revised on unexpected empirical evidence. That is, holism suggests that confirmation is *not judgment-free* and the generalization of empirically (as opposed to, say, skeptically or normatively) induced ~~revisability~~<sup>13</sup>. However, the claims in the quoted passage significantly exceed these generalities in suggesting that the judgments in question are empirically unconstrained or scientifically arbitrary. The quoted passage *does* wear a repudiation of any analytic–synthetic distinction on its sleeves. If its suggestion is an entailment of holism or the generalization of ~~revisability~~<sup>14</sup>, then, as so often claimed, the repudiation itself is likewise part of endorsing holism and ~~revisability~~<sup>15</sup>. But it is doubtful that this is so. For example, the judgments in question could be constrained by further empirical or scientific evidence, as much as many logical *candidates* for revision might be exempt from revision for reasons that are grounded in our overall empirical knowledge. In light of the fact that the thrust of this passage is key for Quine's assessment of the methodological relevance of the analytic–synthetic distinction, we therefore must take a closer look at its connections with holism and revisability. So far, we see that the passage, although sufficient for trivializing the distinction, is not a mere reiteration of trivial consequences of either holism or ~~revisability~~<sup>16</sup>, and that it is rather unclear what commitment it does in fact express.

Apart from having resisted ready allocation, the overt claims in the passage have immediately provoked obvious criticism. The familiar criticisms of this passage take issue with one of the two categorical claims the passage contains. From early on until late, commentators have found difficulty in making sense of the claim that 'all statements are revisable' when it is

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applied to the logical truths that have to be in place to back up rules of valid deductive inference, because then it seems as if Quine were saying that we have a generally applicable argument of the form *modus tollens* (viz., Duhem's syllogism) that proves the invalidity of *modus tollens*.<sup>13</sup> Likewise from early on, other commentators have found difficulty in making sense of the claim that 'all statements are immunizable' when it is applied to appropriately obtained perceptual statements,<sup>14</sup> or taken as a general rule of acceptance for theoretical statements in the empirical sciences.<sup>15</sup> Such critics have taken the passage to face us squarely with the question how some sciences could be so much as *empirical* at all if they were to work as the passage says.

This line is pressed further by another sort of criticism that sees Quine's allegedly empiricist outlook collapse under the pressure of combining both generalizations. Suppose literally everything is revisable, and therefore it is also revisable what follows from what. Then it is unclear how the occurrence of a certain experience can force us to choose either rejection of the experience or changing our web of belief, or at least to epistemically *do something* because we could always revise the rule of inference connecting the inconvenient experience with the hypotheses. By iterating this process on whatever rules we use to defuse the connecting rules, we would embark on an indefinite regress, thus displacing the point of contact between hypothesis and experience equally indefinitely.<sup>16</sup> Once we sever any systematic *mandatory* connections of experiences and the content of our web of belief in this way, we therefore undermine this web's *revisability* on account of experience because we subtract the very *responsivity* to experience required by *revisability*. This surely shows how everything could be immunizable (in virtue of the free revisability of inferential connections), but at the price of sacrificing experiential constraint on belief.<sup>17</sup> We seem forced to choose between either exempting some statements about connections among statements from revision (i.e. accepting some sort of analytic–synthetic distinction) or embracing skepticism about experiential constraint. The thrust of the passage even seems to have caught Quine himself somewhat off guard. While he initially tried to deflect criticism with the ironically innocent remark that his points were "trivial" and such epistemological quandaries more a defect in the eyes of the beholders,<sup>18</sup> much of his later writings are aimed at showing that the unwelcome consequences pointed at in the criticisms are mostly owed to excessively radical interpretations of his holism.

The latter shows that Quine acknowledges the effects of the passage to be of methodological moment. Accordingly, Quine later moderated his holism ("the whole system of science ...") in order to shield certain privileged elements in empirical knowledge like observation sentences (Quine 1969: 89; 1981, 71; 1981a: 26; 1992: 7–8)<sup>19</sup> and logical truths (Quine 1991: 268; 1995: 53f) from its effects and to exclude irrelevant statements from its scope (Quine 1986b: 620; 1991: 269). In addition, Quine urges a distinction between a "legalistic" and a "practical" reading for such passages as the above (Quine 1991: 268;

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1986b: 620f). Examples of the contrast are that, ‘legalistically’, a scientist could *always* revise geometry and compensate for the reverberations, or ‘legalistically’, scientists always are faced with an indefinitely large array of alternative accommodations even once all the evidence is in, or at least faced with alternatives that are empirically equivalent but incompatible. But ‘in practice’, they respect a “maxim of minimum mutilation”, which in fact weeds out a great number of far-out choices, and a number of other rules of thumb. The practical reading would factor this in to explain why not every unpredicted experiential input of some importance produces a dazzling number of equally well-motivated but mutually irreconcilable theories. Quine recommends the practical rather than the legalistic approach and claims that this would defuse most of the mentioned problems.

However, on second thought such maneuvering with holism changes disappointingly little of the methodological substance of this bold passage. Even after all the mentioned moderations, we still get the result that for any hypothesis and relevant set of auxiliaries with a given validly entailed (which relation is now shielded) testable consequence and any given observation (which is now shielded), there are always *legalistically admissible* adjustments of the auxiliaries that save the hypothesis (even though most of them are subsequently overruled as a matter of *practice*). The methodological substance of the passage is apparently located elsewhere than holism. An indication of this is that Quine doesn’t declare the legalistic reading false (just ‘extreme’). This presumably means that it accounts accurately for the actually compelling logical and *empirical* constraints on (i.e. all the evidentially relevant parts of) scientific inference.

This impression is furthered when we examine the possible effects of these moderations. Quine argues on account of the ambiguity of falsification that the decision to revise or retain a statement that implies an unsuccessful prediction requires a judgment regarding the systems of belief that would result from various *admissible* ways of “inactivating the false implication” (Quine 1992: 16). Given an array of such possibilities, the ‘maxim of minimum mutilation’ and kindred principles counsel to perform that revision which, all things considered (particularly, all *entailed consequences*), allows the retention of as many past beliefs as possible. But the point is not whether Quine can account for the factors in actual revision behavior, but what Quine has to say about the *prior* question of what makes any such way *eligible* or *admissible* as an ‘inactivation of a false implication’. That alone gives us a clue to what Quine regards as ‘all the evidence being in’ and therefore as evidentially *relevant*. Now, according to Quine’s view, the maxim of minimum mutilation (and other principles of judgment) always operates on alternative modifications of the whole web of belief and selects the least disruptive from among them.<sup>20</sup> For this purpose, however, each of the individual modifications has to be an admissible *candidate* for acceptance on the evidence. It is here where the only constraints of generating eligible

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alternative modifications that Quine accepts are the drawing out of entailed consequences according to the web-internal logical relations to produce predictions (this is the ‘empirical content’ of the web), and to heed the available observational evidence. Therefore, any other candidate of acceptance is *epistemically equivalent* or equally evidentially admissible (no matter how implausible) if and only if it saves the phenomena and is consistent. Whatever else motivates the adoption of one of the candidates as better than others is an epistemically *optional* and *not further evidentially constrained* choice.<sup>21</sup> As to the evidentially relevant stakes of the choice, Quine’s position is clear: “Inactivating the false implication is *all* that is at stake” (Quine 1992: 16, emphasis added).

The practical reading is thus *epistemologically inert*. It seems questionable whether Quine’s moderation of holism plus pragmatic reading could so much as help defuse the epistemological problems revealed by the quoted passage. It is even less clear how Quine could be *entitled* to advantages of the practical reading such as the elimination of absurd empirically equivalent but incompatible alternative accommodations as, as he put it in the same passage, “intolerable”. For, this judgment would require the ability to marshal good reasons (“evidence”) against legalistically qualified candidates. But whatever *evidential* status a statement can acquire depends, as the legalistic reading has it, only on the logical and empirical constraints. It follows that whatever *epistemic entitlement* a scientist can claim for her practical decision is fully determined by whatever the legalistic reading gives us. The choices that enable us to disqualify given candidates thus go *beyond* what is evidentially covered. That is, they are, epistemologically speaking, quite arbitrary because (as opposed to the logically worked-out accommodations) they cannot be motivated by the admissible empirical evidence since the latter is already exhausted by generating the candidates. But this leaves the defense of the claim that a given candidate is ‘intolerable’ without evidentially relevant resources. And that is exactly the root of the problems the various critics noted. We are thus back to where the critics took off.

Let me take stock: I have argued that the quoted passage is a powerful expression of a basic commitment of Quine’s methodology, the logical force of which is not significantly diminished by later modifications of his holism. The point of my argument was that it is not holism, but only holism together with whatever methodological substance is expressed in the passage that produces its claims of *revisability* and *immunizability*. I now want to further separate holism from the passage’s role in Quine’s methodology by spelling out how it functions in the wholesale repudiation of the analytic–synthetic distinction. Insofar as the passage (and the wholesale repudiation) is independent of holism, we then need to clarify what additional substantive commitments enable Quine’s repudiation. The rift between a (naturalist) Quinean and a (pragmatic) Putnamian methodology is owed to disagreements in these commitments.

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I said before that the thrust of the quoted passage is key for the successful defense of Quine's wholesale rejection of the analytic–synthetic distinction. Let me explain why. This requires shifting attention to the claim that if any statement can be immunized then, *by the same token*, it can be revised. Quine suggests, and has frequently been understood to have successfully argued, that the uselessness and futility of any analytic–synthetic distinction is a consequence of confirmation holism. Representatively, he claims that “methodological monism follows closely on … holism. Holism blurs the supposed contrast between the synthetic sentence, with its empirical content, and the analytic sentence, with its null content” (Quine 1981: 71). Now, it is indeed obvious how the specific proposal to explicate epistemic priorities in scientific reasoning à la Ayer on the model of analyticity as ‘null content’ or ‘linguistic convention’ crumbles under Duhemian holism. But the further step to “methodological monism” is far less obvious. As illustrated in the case of Carnap, who endorsed Duhemian holism *and* the analytic–synthetic distinction (and, contrary to certain critics (Coffa 1991: 352) was coherent in doing so (cf. Friedman 1999, Isaacson 1992)), or in the case of Putnam, who rejects Carnap's specific proposal but defends a reformed notion of apriority within a holistic methodology, the acceptance of Duhemian holism is not sufficient to force abandoning any methodological *a priori*–*a posteriori* distinction as senseless. When Quine speaks of “blurring the distinction” and “methodological monism”, however, he has in mind more than the parochial point of having refuted a certain conception of analyticity as inoperative. What Quine is customarily taken to have accomplished with regard to the analytic–synthetic distinction is to have laid one of the foundations for naturalism, namely having shown that “all statements in science are epistemologically on a par”, *for all conceptions of epistemic priority worth the name*.<sup>22</sup> It is this more general feat – which is at work in Quine's own view about what can count as established for the purpose of naturalizing epistemology (Quine 1969: 1981: 71–72) – that requires another commitment over and above holism. And the passage quoted from “Two Dogmas” supplies the crucial step for this more general purpose. In my opinion, it is the suggestion that the methodological terms ‘held true come what may’ and ‘be revised’ always apply under the same conditions that forms the backbone of the attempt of what Quine calls “blurring the distinction” (Quine:1981, 71), displaying it as useless in methodology, unintelligible or “folly”.<sup>23</sup> To appreciate this, it suffices to distinguish between *criticizing* the analytic–synthetic distinction and certain (or even many) of its theoretical (ab-)uses, on the one hand, and *demonstrating its futility*, on the other. Obviously, the latter project requires a much more stringent argument than the former. My argument is then this: (a) the principled repudiation of the analytic–synthetic distinction is a cornerstone of Quine's “naturalized epistemology”, which makes the establishment of the repudiation mandatory for him. (b) Quine does have such a more stringent argument for repudiating the analytic–synthetic distinction, but (c) it seriously undermines empiricism.<sup>24</sup>

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The quoted passage culminates Quine's discussion of the tenability of the distinction between statements that hold "contingent on experience" and the analytic (i.e. a priori) ones that "hold come what may". It says that, whenever it is legitimate to take a statement true come what may on the occasion of an unexpected experience, it is legitimate *without a change in evidence* ("by the same token") to hold that same statement to be revised by that experience, if only one is prepared to pay the price by making consistency-preserving adjustments in the system. This is precisely what I called Quine's *symmetry-thesis* above.

The role of the symmetry-thesis in Quine's rejection of the methodological relevance of an epistemic analytic–synthetic distinction is in fact quite plain. It is clear that it is able to undermine the idea that any such distinction would make methodological sense, because according to the symmetry-thesis, a statement in our knowledge is revisable in a context by a given experience under precisely the same conditions as those under which it is immunizable against that same experience. If this is generally the case, then there is a *guarantee* that, as Richard Creath aptly puts it, "anything can always turn out to be analytic under the criterion" (Creath 2004: 52). Therefore, unfolding a distinction on the basis of these properties on an empirically contentful system of statements *cannot* yield any significant or stable result. The symmetry thesis *excludes* that any proposed epistemic analytic–synthetic distinction could mark out a difference and thus offers a *principled* argument against the analytic–synthetic distinction. No less than this fact – however established – supports the contention that drawing a distinction among statements between those that are more and others that are less vulnerable to revision *cannot* serve any methodological purpose. After all, if there were, in some case, *evidence* to the effect that a certain statement is, given the circumstances, more revisable than any others in a given Duhemian implier, then, in this case, it would be false that this statement under these circumstances could be declared *just as well*, i.e. *without change in evidence*, as immunizable. So: Quine is only entitled to think that the methodological irrelevance of an epistemic analytic–synthetic distinction is established if the symmetry-thesis is true. The next question is *whether* the symmetry-thesis is *true*, and what support it has.

To judge whether the symmetry thesis holds, we need to make the conditions more precise under which it would be true. Quine suggests that the quoted passage and the abandonment of a methodologically relevant epistemic analytic–synthetic distinction owe their force to, as he put it, "heeding Duhem". We already saw that the repudiation only follows from holism plus the symmetry-thesis. But perhaps holism suffices as a condition under which the symmetry-thesis is true.

The symmetry of revisability and immunizability for any statement on the occasion of any unexpected experience obtains, in terms of Duhemian holism, if, in a given case, for any revising of a hypothesis in response to a

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given experience, there is an alternative adjustment of our system of belief that saves the same hypothesis from revision. In particular, this requires that for a set of auxiliaries and hypothesis that accommodates a given unexpected experience, there is a consistent alternative set of auxiliaries such that the denial of the same hypothesis together with them accommodates the same unexpected experience *and* that there is *no further evidence* such that either set is better supported by it than the other. It is only under these conditions that there are no evidentially relevant reasons to decide whether to revise or immunize a given hypothesis on the occasion of a conflict between experience and our system of belief and that there is an equal entitlement to consider the hypothesis as either revisable or immunizable relative to the experience in such a case. This is what the symmetry-thesis requires.

Now, Duhem's logical point shows indeed that it is possible for such circumstances to obtain. Duhemian holism entails that, since no hypothesis entails testable consequences without auxiliaries, there will always be more than one logical possibility of avoiding a mistaken prediction, and that there is always a logically suitable adjustment of auxiliaries that can save a theory from revision and accommodate the same experience. Of course, this by no means shows that all such logically suitable adjustments are in fact *equally scientifically acceptable*. But the mere logical possibility of empirically equivalent adjustments to save a hypothesis is not quite what the symmetry-thesis needs. The symmetry-thesis requires not only that such adjustments be *possible*, so that there might still be an arbitrarily high number of cases where we have only adjustments of the same experience that are evidentially distinguishable from one another. We just saw that such cases need to be excluded if the symmetry-thesis is to be true. The symmetry-thesis requires strictly that it be *inevitably the case* that, whenever there is a set of hypothesis and auxiliaries that entails a mistaken prediction, (1) there is a set of suitable auxiliaries such that the hypothesis can be saved from refutation and accommodate the same experience, and (2) the adjustment of auxiliaries required for saving the hypothesis from refutation and the set of the revised hypothesis and the unadjusted auxiliaries are, when empirically equivalent in the sense of accommodating the same experience, evidentially not further distinguishable. This is a much more stringent qualification of the required evidence than Duhemian holism as such warrants. It implies that all the auxiliaries used in some given case are equally hypothetical and sacrificeable, and requires that whenever two adjustments accommodate the same experience, they are evidentially indistinguishable. If and only if this is the case are all adjustments of our system of beliefs to a given experience equally available and equally scientifically acceptable. Now, we can only take (1) and (2) to hold guaranteed if, holding the experiential statements fixed, and considering only systems of statements that *logically entail* them, we regard all of these systems as (equally) supported by experience.

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Let me summarize this by saying that the symmetry-thesis only follows from Duhemian holism if it is granted that entailed testable consequences and logical relations among hypotheses, auxiliaries and experiential data exhaust the range of evidentially relevant considerations. For the symmetry-thesis to hold we have to identify empirical equivalence and epistemic indistinguishability. The uncontroversial point Quine seems to appeal to is the *non-uniqueness* of the results of evidential evaluation that follows from Duhemian holism because, given the variety of adjustments allowed by it on unexpected observations, our judgment between adjustments is not determined by logic or the empirical consequences of the statements in the implier (both of which are the same for all candidate revisions) (Fodor/ Lepore 1992: 216). But non-uniqueness under empirical equivalence does not imply evidential indistinguishability. What Quine's actual argument *must rely on* is the far more controversial point that *there are no other evidentially serious constraints to be met*. This is the substantive commitment behind the symmetry-thesis, and as such it needs to be supported if the repudiation is to be compelling. I now turn to this question.

In fact, Quine's brand of empiricism never ceased to be, methodologically speaking and with regard to his conception of what it is to be evidentially compelling, thoroughly hypothetico-deductivist. As an empiricist, he says "whatever evidence there is for science is sensory evidence" (Quine 1969: 75), and regarding how this evidence has to relate to our hypotheses in order to confirm or disconfirm them, he says that the scientific method is the hypothetico-deductive method and little else (Quine (1981): 72; Quine (1981a): 28; see also Quine 1960: §5-6). This issues an *independent* substantive view of what *in principle and in general* is evidentially relevant (and therefore *rationally* responsible for scientific acceptability). It gives us the root commitments from which Quine can actually obtain the symmetry-thesis. Thus, it is here where Quine's repudiation of the methodological relevance of an epistemic analytic–synthetic distinction and his very own substantive commitments (as opposed to Duhem's merely logical point) coalesce and align:<sup>25</sup> to guarantee the methodological inertness of any epistemic analytic–synthetic distinction, the symmetry thesis has to be true, which in turn only is the case if there are no evidentially decidable differences between empirically equivalent accommodations of the same experience, which in turn is only secured by the identification of empirical equivalence and epistemic indistinguishability. The latter, however, is only supported by a commitment to the conception of scientific or epistemic justification that Quine inherits from logical positivism.<sup>26</sup>

The symmetry-thesis thus has deep roots in Quine's methodology. A look at some of the consequences of accepting it illustrates what dangers Putnam warned us of. I just construed the identification of empirical equivalence and epistemic indistinguishability as indispensable support for the symmetry-thesis, but it is also required for Quine's wholesale repudiation of the methodological relevance of an analytic–synthetic distinction. As is

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well known, when combined with Duhemian holism, this identification simultaneously leads to the underdetermination-thesis (UD). For, any accommodation of an unexpected experience that ‘saves the phenomena’ is, evidentially speaking, as good as any other in virtue of the identification of empirical equivalence and epistemic indistinguishability. In virtue of the ambiguity of falsification resulting from Duhem’s thesis, this is so even for cases in which one of the accommodations contains a hypothesis and the other its contrary. Combining both, we get the idea that conflicts with experience open the specter of mutually incompatible, evidentially indistinguishable theories. Because the symmetry-thesis (and its preconditions) occurs in Quine’s methodology in a holist setting, it exposes his account to some well-known consequences that any position entailing UD has to face. They form a nested system of increasingly confirmation-skeptical positions.

### *Excessive epistemic egalitarianism<sup>27</sup>*

UD invites us to think that, whenever there is an unexpected experience and a hypothesis we would revise, then there is an evidentially indistinguishable readjustment of our system of beliefs such that we can hold the hypothesis in question true and accommodate the same experience. However, the exclusion of other evidentially relevant considerations for breaking the tie makes it irresistible to continue to the next step by saying that, if any statement may be held true come what may, any statement may also be held true come this or that experience. If, by the same token, no statement is immune to revision, then *any* statement held true could be held false, come this or that experience (Laudan 1990: 328). This entails that everything confirms everything if ‘confirmation’ is explicated via the hypothetico-deductive method. Quine’s view of *revisability* expressed in the quoted passage thus ultimately seems to collapse into the skeptical ‘egalitarian thesis’ that “every theory is as well supported by the evidence as any of its rivals.”<sup>28</sup> Repudiating the methodological relevance of the analytic–synthetic distinction in a principled way thus seems to come at the cost of no longer being able to explain how even deeply entrenched or highly abstract theoretical beliefs can become subject to ordinary empirical (evidence-driven) evaluation and possibly *rationally* revised on the strength of changes in empirical information.

### *General reversibility of epistemic assessment<sup>29</sup>*

The problem pointed at in the egalitarian thesis is that it is unclear how genuine determinate epistemic warrant for any hypothesis can *exist*. This worry gets exacerbated when we ask how any result in confirmation can actually count as a cognitive entitlement, even supposing some such epistemic relation had been selected somehow. For, suppose we are guaranteed, via the combination of Duhemian holism and the identification of empirical equivalence and evidential indistinguishability for every hypothesis, that

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there is a consistent accommodation with an adjustment of auxiliaries that entails the contrary of the hypothesis and the same experience. Suppose further that acquiring a rational entitlement for adopting a belief epistemically requires, among other things, exhausting all the available evidence. Then a guarantee of evidentially indistinguishable replacements presumably means that we are equally entitled to each of the two accommodations. Since the argument is perfectly general, the evidence supporting whatever choice we initially make always brings us simultaneously into the possession of an undefeated defeater of the very hypothesis we choose (viz., that same evidence, but used to support the – evidentially indistinguishable – contrary of the hypothesis). The repudiation of the methodological relevance of an epistemic analytic–synthetic distinction thus ultimately disqualifies the acquisition of our empirical entitlements as an essentially self-undermining procedure.

### ***Loss of content*<sup>30</sup>**

The grounds of Quine's rejection of the distinction ultimately issue in a thesis that is equivalent to the quoted passage, namely that, for all statements we accept on the strength of some experience in some context, we can specify a context in which, given the same experience, this statement is false. But it is by no means clear that we should always have *actual* alternative auxiliaries. It is, for example, much more likely that suspending or denying some auxiliaries like conservation laws regarding energy – which are involved in the planning of many experiments and the derivation of testable predictions from many hypotheses – without supplying adequate replacements disables the derivation of *any* empirical consequences from the hypotheses. However, for all that, nothing in the assumptions undergirding the symmetry thesis prevents setting them as false if only the phenomena are saved. According to the position forced by Quine's symmetry-thesis, we ought to accept the system with the denied conservation principle and the *then* empirically inconsequential hypotheses in question nonetheless as evidentially indistinguishable from those that are actually plausible whenever the conservation principle was part of the Duhemian implier. So, according to Quine, we are generally epistemically entitled to believe that a hypothesis we are testing might at the same time not actually have any specifiable empirical consequences. Of course, lacking empirical consequences because of the defunct indispensable auxiliary, the hypothesis would automatically be immune to revision. We have Quine's desired result, that any hypothesis is revisable iff immunizable. However, when all our hypotheses turn out to only optionally have empirical content and to be immune against any experience in particular, Quine's greatest insight, that revision *on the strength of experience* can strike anywhere and *oblige us* to change our belief somewhere, seems out of reach.

Let me sum up. Quine's principled argument against the methodological relevance of an epistemic analytic–synthetic distinction depends on the

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symmetry-thesis. Insofar as it does, it does constitute a powerful argument against conventionalism of the Carnapian sort by enabling the overextension-threat. Only given this can Quine suggest that the analytic–synthetic distinction is an independent and spurious addition to Duhemian holism, and that dropping it helps putting conventionalizing pet theories under empirical control, at no extra cost. But the costs of generating the overextension-threat for this argument are in fact quite high, and it also doesn't get at the root of the problem that Quine identifies with conventions (their blocking *empirical* belief-fixation). As Quine leaves intact the combination of the identification of empirical equivalence with evidential indistinguishability with Duhemian holism that characterized much of the logical empiricist conception of evidence and confirmation, his account allows mounting a global underdetermination thesis. This is doubly unfortunate. First, using UD enables privileging some non-revisions over others by *fiat* only, i.e. makes merely positing assertions as true easy. So, Quine's approach cannot undercut conventionalism as radically as talk of naturalism as a 'radicalized empiricism' would make us expect. Second, UD inevitably fosters the suspicion that intuitively empirical claims only appear to be sensitive to experience while in fact they cannot be. This is a skeptical result regarding external constraints on rational belief-fixation. I think that both of these reasons play a role in Putnam's resistance to Quinean wholesale repudiations of the distinction.

We saw already that the case is not hopeless. On the one hand, the symmetry-thesis depends not on holism or the acknowledgement of general **revisability**, but on particular methodological views about what counts as evidence, support and confirmation. They are not without alternatives. On the other hand, we saw that holism does not exclude a contextual analytic–synthetic distinction. So, there is logical space for explicating how holism and a more natural distinction between epistemic priorities conspire to explain epistemic contact between belief and experience. Putnam's view occupies this open methodological space.

### III. The methodological significance of a contextual distinction

Putnam's criticism of the attempt to combine empiricism and conventionalism pivots on the following principle of general **revisability**, "any principle in our knowledge can be revised for theoretical reasons" (Putnam 1962: 48). It is hedged by the caveat that "many principles resist refutation by isolated experimentation" (Putnam 1962: 48). How central this caveat is in Putnam's conception becomes patent in the following passage from his "The Analytic and the Synthetic":

I want to suggest that before the work of nineteenth-century mathematicians, the principles of Euclidean geometry ... had the

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following status: no experiment that one could describe could possibly overthrow them, by itself. ... the position was rather different, as physicists soon realized: give us a rival conceptual system, and some reason for accepting it, and we will consider abandoning the laws of Euclidean geometry. ... I mean to group them ... with many other principles: the law ' $f = ma$ ' ... , the principle that the world did not come into existence five minutes ago, the principle that one cannot know certain kinds of facts ... unless one has or has had evidence. These principles play different roles; but in one respect they are alike: They share the characteristic that no isolated experiment ... can overthrow them

(Putnam 1962: 48)

Putnam adds that the revision of principles with this characteristic is “(a) possible ... but (b) quite a different matter from the revision of an ordinary empirical generalization” (ibid.). To appreciate the full significance of these passages, it is important to attend to three features of Putnam’s methodological outlook on the experimental sciences: first, his interactivist view of cognitive contact with the environment, second, his view of theoretical assertions as delivering genuine and irreducible empirical information that can issue fully empirical reasons, and third, his induction-based approach to confirmation, testability and empirical evaluation.

First, in his criticism of the Carnapian analytic–synthetic distinction Putnam importantly speaks always of ‘experimental results’ and similarly large units of empirical significance as the sources of empirical content and evidence, as opposed to Carnap and Quine, who both base empirical appraisal and content on the presence of certain protocol or observation sentences. For Putnam’s view it is constitutive that we never deal with single sentences even in the realm of empirical evidence. In consequence, if there is any empirical evidence at all, then certain sets of beliefs that formally are indistinguishable from Duhemian wholes, like reports on experiments, are *correctly* counted as giving *contextually definite results*. While ascertaining empirical results always involves judgments in some sense and is a contrastive affair, such wholes often quite simply reach through to judgments of truth.<sup>31</sup> For Putnam, that is one part of what it means that a body of knowledge is subject to empirical testing: that empirical assertions that are, logically speaking, the conclusions of arguments that conform to Duhemian holism can, ought to, and often do serve as fixed evidence points or *data* for the epistemic assessment of other (‘theoretical’) statements (Putnam 1965: 88). It is therefore also part of what it means that a body of knowledge is empirically testable that *not all Duhemian wholes are radically underdetermined*. For Putnam, this is a fact about the use of evidence, so that any view that entails a global underdetermination thesis for *all* Duhemian impliers is false of actual empirical science. This rejection of wholesale underdetermination-claims signals, of

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course, the decidedly anti-instrumentalist conceptions of theoretical statements, as well as of the theory/observation distinction that frame Putnam's own holistic and fallibilistic methodological reflections.<sup>32</sup> But what I am interested in here is the less often observed methodological repercussions of such a rejection of wholesale underdetermination. Putnam regards the relevant difference in epistemic status between data-assertions and others not as the result of the *function* that experimental results are given by competent researchers in *contexts of assessment* (as opposed to a supposed principled difference grounded on, say, epistemic properties, semantic rules, or regularities in acceptance behavior). The key to Putnam's success in *recognizing empirical data as given constraints* while 'heeding Duhem' is his repudiation of any a-contextual, general distinctions among empirical statements as to their vulnerability to incoming experience. The same statement can, under relevant changes in context, appear as a testable and tested assumption even though it was correctly used as an evidence-datum before. By itself, Putnam's rejection of wholesale underdetermination thus does not directly commit him to any of the distinctions Quine suggests we should abandon, or beg any of the questions presently at issue.

Second, Putnam is committed (as is Quine) to what Bennett (1971) calls "knowledge empiricism".<sup>33</sup> Knowledge empiricism in the liberal sense here intended demands that whatever status an extra-logical statement may enjoy at a given time, its acceptance or revision cannot be warranted independently of *all* experience, but must involve reference to some experience (Putnam 1962a: 248). So, when Putnam qualifies the revision of the principles in question as being a revision 'for theoretical reasons', this is to be regarded as being a revision 'for empirical reasons' in spite of not being a refutation by experimental data. Settling the truth values of these principles crucially involves reasoning from statements, theories and inferential rules that themselves, as a whole, enjoy empirical support. Putnam's reference to 'theoretical reasons' is quite consequential in that it expands the scope of available evidence beyond the observational entailments of a hypothesis on the occasion of a given test, by admitting the independent support that theoretical auxiliary statements involved in such arguments have acquired on other occasions.

Third, the empirical assessment of theoretical principles has, according to this passage, not the form of a hypothetico-deductive argument but that of an inductive, comparative judgment of the relative support enjoyed by one of at least two 'rival systems' on the strength of the available evidence. Experimental data are one important factor in determining which of the two theories is better supported by the evidence, but not necessarily the only one. Such judgments are *essentially* non-monotonic and context-dependent, i.e., not determined by deductive relations among the statements related as evidence and hypothesis alone, and sensitive to changes in the alternatives as much as in the sort of empirical data. Although these two features cast a shadow

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on the identification of empirical equivalence and evidential indistinguishability and thus are indirectly relevant for evaluating the reasons for the rejection of the distinction, the fact that the judgments are contrastive is methodologically directly relevant. It means that, when two theories are compared as to the empirical support lent to each by *the same or shared* empirical evidence, not all involved statements can be equally revisable in this context. In order for there to be a *comparison*, at least the shared evidence has to remain invariant when other statements are up for grabs. As invariant for the purpose, these statements are thus less revisable in this context than those of the theory.

Putnam's conception of the peculiarities of the epistemic status for the framework statements is roughly this. In light of the general outlook along these three commitments, the confirmation and revision of the principles is not experimentally determined (because they are not capable of direct experimental test), but it is not therefore empirically unconstrained. But it is important to note what special roles the principles play for inquiry. For example, Putnam explains, they are "employed as auxiliaries to make predictions in an overwhelming number of experiments, without themselves being jeopardized by any possible experimental results" (Putnam 1962: 48). That is, they are *recurrent* parts of many Duhemian impliers, in all of which they do not count as good candidates for revision but are rather taken for granted or *counted among what is given* in a confirmation argument: "one is not expected to give much of a reason for" them (Putnam 1962a: 240). As a first approximation, we can say that we are, at a given stage of inquiry, not *normally* required to give any further support for assuming and not revising the statement. Instead, Putnam says, "holding them immune from revision [is] good methodology" (Putnam 1965: 92) so that their special status is that of not being seen *de facto* as rationally revisable at a certain stage of inquiry. Clearly, this status is a feature not of the statement, nor of its empirical consequences, but of the context and the way in which it is used that can, under the changed conditions of a later stage of investigation, become capable of being revised (and known to be false) on the strength of empirical evidence available then.

With regard to justification, the 'special status' thus cannot be that of classical apriority. For, if it is a traditional sense of a priori justification that it is justification by reasoning from principles the acceptance of which does not require any empirical information, then the framework principles Putnam talks about cannot be traditionally a priori. Their revision is supposed to be neither experimental nor non-empirical,<sup>34</sup> that is: not purely a priori, but also not analytic, if the latter is understood as 'factually vacuous'.

In my view, the most fruitful way of explicating Putnam's remarks about statements with this status is to embed them in a pragmatic or "default" conception of epistemic entitlement combined with a "defence commitment" (Williams 2001: 25) on the part of their users in this way: (a) as long as someone who *requires* a justification fails to produce credible circumstances

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that offer a counterexample to the statement, we are *normally* entitled to take it for granted without *further* empirical justification (Harman 2003: 23–34). (b) Putnam's claim that "holding [the principles] immune from revision [is] good methodology" (Putnam 1965: 92) means that as long as we are entitled to take something for granted without further empirical justification, the presumption is that *there are no good reasons against it*. Statements satisfying (a) and (b) are *not hypothetical in a context*.

Putnam's conception of evidence and confirmation combined with this view of epistemic entitlement has the consequence that there are statements that we are normally entitled to hold true without previous experimental test or further experimental reasons and which it is, under the given circumstances, *irrational to give up* without adequate replacement. Combined with the contextual nature of the status, the special status of the principles is then that we are entitled and required to use them in the relevant contexts of confirmation without further empirical reasons unless *special conditions* obtain where this ceases to be the case. A somewhat surprising but welcome effect of the position developed so far is that the possibility for such statements is a consequence of the principle of general *revisability* when the latter is embedded in the same conception of epistemic entitlement. For, if a statement in our knowledge can be revised for at least theoretical reasons and if we are entitled to hold true principles in our knowledge unless there are available theoretical principles against it, then there can be principles in our knowledge that we are entitled to hold because we do not have *any* theoretical reasons against them. That is the case when certain principles are needed as auxiliaries in a wide array of Duhemian impliers and we do not possess adequate replacements for them. Let me refer to statements for which we possess (or could adduce) support by past evidence and for which we do not need to give further evidence to be justified in employing them as auxiliaries in Duhemian impliers as *non-hypothetical*.

We now face two questions. On the one hand, why are such statements not just another version of the conventionalist's arbitrary stipulative exemptions from empirical revision? On the other hand, precisely insofar as they are not, why are they not just *plain revisable statements* like all others? Why regard "non-hypothetical" as epistemologically exciting if they remain revisable in some context? The induction-based outlook described above gets explanatory bite right here, because the answer requires attention to the *relative support* enjoyed by certain contrasting statements in *given* situations.

That certain statements in Duhemian impliers are less hypothetical than others is actually not uncommon, and in relatively few cases does this have anything to do with conventions. It is, for example, a feature of statements that we take for granted as auxiliaries in a particular context of confirmation, but which are independently testable (say, assumptions about the reliability of a thermometer under certain conditions). Intuitively, when such

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statements are challenged and independently tested, the resulting support in case of a positive result of the test makes them less revisable than the other auxiliaries in the same context that are similar in initial credibility but merely taken for granted without additional test. So, independently testable auxiliaries can indeed be non-arbitrarily epistemically privileged members of Duhemian impliers. However, since they acquire this status in virtue of *being tested* and therefore not ‘without further empirical investigation’, they can only be contextually *non-hypothetical* but not even contextually *a priori*. But it is important to recognize that testables independently have this status. For this *already* shows that Duhemian impliers do not simply comprise equally contextually hypothetical statements, and also that the empirical import of a Duhemian implier does not simply consist in the observational entailments in a particular situation. Independent tests offer an *unimplied source of support and evidence*. We have driven the first wedge between empirical equivalence and evidential indistinguishability by specifying parts of the evidence that is available in a given context but not encoded in the particular Duhemian implier as such.

Now consider the case of recurrent statements – like fundamental statements of physical geometry or conservation laws, or similar high-level theoretical principles. Suppose we took such a statement – say, a conservation law – for granted as an auxiliary in an experiment that tests some theory by displaying scatters of particles on a screen, and that the experiment showed an unexpected result. Then the special status of a statement like the conservation principle can be illuminated in terms of Duhemian holism in three directions: (a) vis-à-vis the theoretical statements under scrutiny (our hypothesis), (b), vis-à-vis independently testable auxiliaries, and (c) vis-à-vis the testability-conditions for hypotheses.

(a) Duhemian holism exposes the auxiliary statement, our law of conservation, to possible revision. This might seem to suggest that, in spite of its apparent epistemic priority, there could be experimental reasons for doubting this statement just if there are such for doubting the hypothesis and all the other statements involved in deriving the prediction. However, this does not work. Suppose, for example, that we deny the conservation law and in this way no longer derive the prediction about our particle scatter that conflicted with the observation. But the reason for this is that we can no longer predict *any* reaction of the screen *rather than another* on the basis of our hypothesis and the remainder of auxiliaries. Moreover, the same crumbling of determinate empirical consequences happens in most *other* cases where the conservation law plays a role in prediction. In this sense, we actually would not have specified any *empirical* context of which the conservation law is clearly false. Moreover, once suspended, the absence of the conservation principle will likewise block the derivations of many other empirical consequences in past cases that were not actually at issue. Therefore, we can take our lack of relevant empirical evidence for

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each of the simultaneous failures of prediction that we would produce by suspending the conservation principle as empirical information capable of preventing its revision. Of course, there is an appearance of circularity here, since the empirical information on which we rest our retention of the conservation principle is partly dependent on that same principle's application. But the considerations at issue are in an important sense *contrastive* and therefore not exhausted by a simple reinstatement of the conservation principle. The retention of the principle does not merely 'rest on' its successful applications, but also on the fact that the alternative is an empirically entirely unmotivated and inexplicable wholesale loss of large stretches of empirical information and support in unrelated cases, which is equivalent to assuming a miracle (viz., that a failed experiment can remove conservation). If the alternative were more empirically palatable, so many successful applications of the principle might not be able to save it *even if its success in predicting the cases remains constant*. Its privilege is thus not merely *conventional* and not simply by "default".<sup>35</sup> Rather, the unproblematic status it has as a default can – in case anyone were to propose sacrificing the principle – be itself defended as a result of assessing empirical information we possess, even though not of any particular experimental result.<sup>36</sup>

(b) But – in contrast to the independently testable hypotheses considered before – it also does not seem quite right to take the auxiliary as more *supported* than before by such considerations. It is correct, of course, that we did not turn up any experimental evidence against holding the conservation law. But the alternatives in which the conservation law was considered as false (and presumably 'found true' via *reductio*-arguments) were not seriously considered by us as other *empirically specified* contexts. We just found that they were not such in the first place. Therefore, they are also not contexts in which *any* empirical support could accrue, positive or negative. So it is not the case that the fact of lacking evidence against the conservation law *independently* contributes to the *support* of the statement. Conversely, in virtue of the absence of an adequate set of empirically specifiable contexts for its empirical re-evaluation it is also not properly regarded as just one more *plain revisable statement* because we *can* have no theoretical or experimental reasons against them. We can thus explain how some statements that are members of Duhemian impliers in the context of confirming particular hypotheses at a given stage of inquiry can be relatively non-hypothetical while their acceptance is not contingent on further empirical evidence, or *a priori*. The contextual status they have is due to a significant, albeit quite un-empiricist and non-physicalist (i.e. neither logical nor observational), collection of facts, viz. the historical and social facts that make up a situation of which it is true that there actually are no adequate alternatives to a given statement in the field, and no empirical counter-examples available.<sup>37</sup> I think this fairly captures an intuitive, generic notion of contextual apriority.

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(c) The impression that such a methodologically motivated notion of contextual apriority is even *positively suggested* by Duhem's thesis acquires more force when we add another consideration about confirmation practice, the fact that in most cases, theoretical hypotheses are not tested by exposing them to refutation, but by weighing the support they gather by given observations *relative to salient competitors*. Thus, what a given observation actually tests is an *array* of related theoretical hypotheses, and what we want to find out is which of these is *most supported* by the evidence. The most obvious case for this are estimated quantitative predictions like 'given experiment E and the mutually exclusive theoretical hypotheses T1 and T2, the quantity Q will either (case 1) be between n and m, or (case 2) between k and l, or (case 3) between j and i; case 1 will support T1, case 2 be neutral, and case 3 support T2'. According to Duhem's thesis, each of the theories, T1 and T2, require auxiliaries to predict any values of Q, given E. Now, given the problem, E is a means of determining the value of Q. Therefore, no matter which of our hypotheses we take E to test, we will have to use that *common* set A which (together with, respectively, T1 and T2) allows predicting values of Q given E. In our example, the value of Q confirms T1& a iff it conflicts with the prediction from T2& a and vice versa, such that there would always *seem* to be a possibility to revise A, *if it weren't that the evidence for doing so entails A*. No matter whether T1 or T2 turn out true or false, A is needed for supplying the evidence in *any* of the possible outcomes. A is thus *invariant* under the outcomes of the test; therefore, it is *not tested* when any of the alternative theoretical hypotheses are. Wherever we regard testing in this way as an essentially *contrastive* affair, then there are, *precisely for Duhemian reasons*, always some shared background auxiliaries that enable the shared evidence for any of the considered outcomes.<sup>38</sup>

The methodological cash value of these reflections is that principles that we don't have theoretical reasons to regard as actually revisable in this way become *part of the evidence* brought to bear on the assessment of hypotheses, even though they are, by nature, theoretical principles and as such in a formal sense 'revisable' because they appear, as auxiliaries, in a Duhemian implier. But, for all that, they are *neither hypothetical* in the relevant contexts *nor justified* in the context of their use on the strength of independent confirmation. This is what justifies calling them 'a priori'. Since they can become part of the *undisputed* evidence only in this status, their possessing this status is evidentially relevant. Drawing an a priori-a posteriori distinction in a context in this sense and finding statements with this status thus *affects the available evidence* and failing to draw it *changes the available evidence* by misrepresenting non-hypothetical elements from the auxiliaries as hypothetical and thus not part of the evidence.

The lesson from this is that the *evidence* for or against a statement is constrained but not determined by the formal relations between the hypothesis and its entailed empirical consequences. An actual determination of what

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the relevant evidence in a case is can only come from factoring in and prudently judging doxastic and non-doxastic contextual information. Clearly, such differences in revisability-conditions within Duhemian impliers crucially depend on non-arbitrary doxastic and non-doxastic features of our epistemic situation<sup>39</sup> and the contexts in which statements are used, both of which are not in our hands and open-ended. In light of this, adjustments of our system of belief that accommodate a given unexpected experience and retain a given hypothetical statement at the cost of violating either independently testable or contextually a priori parts of the relevant Duhemian implier are *evidentially distinguishable* in the relevant contexts from adjustments that accommodate the same experience but consider the hypothetical statements as revised. The fact that two adjustments are empirically equivalent is thus not generally sufficient for their evidential indistinguishability because in actual fact, auxiliaries as such most of the time carry information about their previous confirmation and contexts of their further employment with them into Duhemian impliers.<sup>40</sup> Thus, when unexpected results occur, revision does not strike indiscriminately. As this undercuts the symmetry-thesis, no general UD-threat follows.

Recall that Quine's principled argument against the methodological relevance of any epistemic analytic–synthetic distinction ultimately rested on the identification of empirical equivalence and evidential indistinguishability. Giving it up has the cost of sacrificing the symmetry-thesis and Quine's principled argument. But the methodologically relevant a priori–a posteriori distinction in terms of degrees of hypotheticity in Duhemian impliers does not oblige us to accept any non-empirical sort of justification, and the context-sensitive strategy it is part of captures much more of the actually available evidence. Quine's principled argument is thus unconvincing in the light of alternatives like the Putnamian line here presented. Breaking the Quinean grip leaves it open whether more 'traditional' approaches to epistemic privilege for areas of inquiry like the understanding of logical necessity are needed, and how adequate notions of epistemic priority can be developed for such purposes on the model of the pragmatic, contextual notion laid out here. The need is illustrated when even Quine in his late work resurrects the very account of logical necessity (Ayer's) that he combated in "Two Dogmas". Putnam's current work<sup>41</sup> as well as that of many others (Floyd, Casullo, Field, etc.) explores the latter question. Contrary to Quine, finding ways of drawing the epistemic analytic–synthetic distinction is still of central methodological importance.

### Notes

1 This chapter is the result of a long process during which many people helped it grow. Throughout, discussions of the various stages with Cristina Lafont were a continuous source of improvement. When I read sections at the Central Division meeting of the APA in April 2003, Jay G. Campbell and Louise Antony made

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me see important points. The adjusted chapter profited from comments of Carlos Pereda, Maite Ezcurdia at the UNAM in Mexico, and at the Alice Berlin Kaplan Center for the humanities from comments by Tom McCarthy, Bob Gooding-Williams and Tom Ricketts. I am especially grateful for encouraging feedback I received from Arthur Fine, and for Thomas Ricketts' continuous incisive, patient and collegially cordial criticism. Colloquia at Grinnell College and the University of Illinois Urbana-Champaign brought additional valuable comments from the audiences. The final and most enormous debt I owe, of course, to Maria Baghramian and Hilary Putnam for giving me the opportunity to present the final version at the conference in honor of Putnam's 80th birthday at University College in Dublin. There, Alan Berger's and Hilary Putnam's extensive direct comments on the chapter, but also many other papers and conversations, enriched me more than I can have been able to express. My deepest thanks go to all of them.

- 2 For a succinct representative account that explicitly makes the claim that logical and mathematical necessity as well as epistemic necessity (irrevisability) is to be reduced to analyticity (understood as empirical vacuity or 'being a tautology'), cf. Ayer (1936: 39). For more technical presentations of the identification of necessity, analyticity and apriority, cf. the joint product of Carnap (1934: §§ 52, 67–69 and 79), as well as, in semantic terms, Carnap (1956: §§ 2) (translates 'analytic' as 'L-true') and 39 (identifies 'necessary' and 'L-true'), for the identification of analyticity and apriority, Carnap (1974: ch. 18, esp. 181–82).
- 3 The claim that conventionalism accounts for the necessity of mathematical and logical truth should not be confused with the claim that there is a conventionalist account of the *grounding* or *generation* of logical truth. The latter was trenchantly rebutted in Quine (1934) and Quine (1956). The difference between both projects is illuminatingly pointed out in Hellman (1986), as well as in Putnam 1994a, esp. 248.
- 4 Cf. Putnam (1962): "analytic statements ... are true because they are accepted as true, and because this acceptance ... has no systematic consequences beyond ... allowing us to use pairs of expressions interchangeably" (68–69), and Quine (1986): "I see little use for [the psychogenetic notion of analyticity] in the epistemology or methodology of science" (95). For an excellent recent exposition and discussion of the thesis that analyticities are one and all epistemologically harmless, cf. Nimitz (2003).
- 5 Quine (1951): "it becomes folly to seek a boundary between ... statements, which hold contingently on experience, and ... statements, which hold come what may" (43).
- 6 Quine (1970) deems the linguistic conception as "too generous" because it "views these domains [logic and mathematics] as immune likewise to empirical refutation" (99).
- 7 The fundamental significance of Quine's attack on Carnap's way of drawing the distinction for the development of the program of philosophical naturalism has been stressed by Quine throughout his work. That this rejection *has to* take the form of a *principled* rejection of *any* successor distinction and the counter-thesis that all statements are empirical has been forcefully argued by Friedman (1997 and 2000) and. That this form of rejecting the distinction in principle has the same foundational and, as it were, transcendental role for the defense of naturalism as the defense of the analytic–synthetic distinction had for Carnap's program of the logic of science has been recognized and argued by Lepore (1995), and is supported by reflecting on the role of logic as a fundament, which Quine's philosophy transforms relative to Carnap but never abandons, as Ricketts (2004) argues. A particularly uncompromising representative of a naturalist

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program in regard to the methodological non-role of the a priori is Devitt (forthc.).

8 This distinguishes the argument here proposed from criticisms of skeptical consequences of Quine's methodology that are intended as proofs of traditional a priori claims, like BonJour (1998: 62–97).

9 Putnam and Quine are here in the same camp in comparison with neo-rationals like BonJour or Katz, who defend certain forms of a priori justification. While Katz's account is heavily bent on maintaining the connection between apriority and linguistic meaning, BonJour (1998) takes on the question of the distinction in the explicitly epistemological/methodological way here pursued. Cf. *ibid.*, 76.

10 I am thus sidelining more general discussions of a priori knowledge as such, but also much of the literature on holism and analyticity, which mostly concentrates on issues in the theory of meaning. In doing so, I simply take for granted with both authors that the linguistic conception of apriority is not an option for the reasons sketched above.

11 Which, as is well known, has as one of its foundational axioms the futility of *any* such distinction. Cf. Quine (1969: 82ff.), Friedman (1997).

12 An additional complication arises, of course, from the fact that theoretical hypotheses in technical languages are typically formulated with the help of 'cluster terms' (Putnam 1962). I will neglect this meaning-holistic aspect in what follows and concentrate on the confirmation relations between *understood* theoretical hypotheses and the evidence. My working hypothesis is that what I will say will apply, mutatis mutandis, to Duhemian impliers that are expanded so that they capture the relevant parts of the theory needed to specify the use and application of the hypothesis.

13 Bennett (1958–59) was the first to note this problem; more recently, Berger (2003) gives an illuminating account.

14 Numerous epistemologists have taken Quine to task on this point. A thorough and elucidating example is Travis (2004).

15 Popper (1963/2002), Grünbaum (1960), (1962), and Wedekind (1963) critically scrutinize Quine's argument in this direction early. More recently, the most compelling and sustained criticism has been formulated by Laudan (1990) and Laudan/Leplin (1991).

16 This line of criticism originates in Bennett (1958–59) and Frankfurt (1960). Dummett (1973: 596–608) constitutes another milestone, clarified by Williams (1998: 239). More recent kindred arguments to the conclusion that Quine's methodology is incoherent on count of the general *revisability* thesis can be found in Wright (1986: 196ff.), and in BonJour (1998: 90–97).

17 This is one of the issues raised by McDowell (1994), who comments on Quine's deflation of apriority in terms of centrality, i.e. "the relative likelihood, in practice, of our choosing one statement rather than another for revision in the event of recalcitrant experience", that this means that for Quine, "it is not that it is right to revise one's belief system thus and so in the light of such-and-such experience, but just that that revision is what would probably happen" (133). McDowell observes that this converts Quine's talk of beliefs' 'facing the tribunal of experience' into empty rhetoric, leaving us with "the awkward position of experience in Quine's thinking" (135).

18 Quine (1963: 133). Ricketts (1982) takes this reaction as a clue to explaining Quine's strategy in "Two Dogmas". He argues that Quine's 'Duhemian argument' is overrated because it is mistaken to make an epistemologically interesting point where it is best understood as a trivializing argument against yet another

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candidate for an analyticity-criterion. It goes without saying that he does not think that this deflation of the argument would be damaging to Quine's overall philosophy, or particularly, to non-trivial readings of his revisability-theses (e.g. as basically negative contributions to research into confirmation theory). In what follows, I agree with Ricketts that the 'Duhemian argument' might be *intended* as a trivializing argument, but I disagree with the suggestion that it is itself inconsequential ('trivial') or that Quine's methodology could live without the non-trivial, epistemological readings of what's behind the passage. Instead, the trivialization comes at a *high price*.

- 19 Quine (1975: 314–16) speaks of the "strong presumption in favor of the observation statements ... that makes science empirical"; Quine (1991a: 109): "piecemeal is how the sentence relates to theory".
- 20 M. Williams notes that Quine's view of the application of the maxim is predicated on the assumption that holism means that "local justification presupposes global" (Williams 1991: 292).
- 21 That this is Quine's view of the matter becomes particularly evident in his expanded explanation of how 'centrality' can account for the apparent necessity of mathematical truth. He writes:

why is mathematical truth necessary rather than contingent? ... Holism's answer is that when a critical mass of sentences jointly implies a false prediction, *we are free to choose* what component sentence to revoke so as to defuse the implication. In so choosing we choose to safeguard any purely mathematical truths among those sentences.

(Quine 1999: 22 (emphasis added))

See also Quine (1992: 15–16). Clearly, the work in this explanation is not done by Duhemian holism, but by Quine's insistence on the freedom to choose.

- 22 Isaacson (2004) speaks fittingly of the "impossibility argument in 'Two Dogmas'" (242).
- 23 When Quine, pressed by Hookway's criticisms, claims at a later point that he "never found these [passages, AM] useful or illuminating" (Quine 1994: 503), this confirms my suspicion that the *principled* argument against the analytic–synthetic distinction (as opposed to other, more local ones) is not even by his own lights Quine's best. But that does not mean there'd be a better one. The wholesale repudiation (needed for grounding Quinean naturalism) could be *ill-motivated*.
- 24 Lepore (1995: n. 6) makes a similar point in the context of determining the scope of Quine's semantic holism when he adverts that even though there is an interpretation of Quine's criticism (one of Putnam's) that comes to saying that "Quine rejects the a/s distinction because it has no role to play in accounting for knowledge. Since there are no a priori truths, we don't need an a/s distinction to account for them", this interpretation "can't be the entire story" because it "is consistent with the a/s distinction being perfectly intelligible; it is merely superfluous." The latter, however, is incompatible with Quine's claim that the distinction 'has not been given any methodological sense'.
- 25 Quine affirms his commitment to hypothetico-deductivism until his last and definitive work on scientific method:

On the heels of observation sentences we saw science emerging with the inception of observation categoricals. ... The categorical was a miniature scientific theory. Its antecedent clause was the experimental condition, and

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its consequent clause the prediction. ... observation categoricals ... are the lifeline of science; for I see them not just as miniature scientific theories individually, but as the ultimate checkpoints of science generally. A theory is tested by *deducing an observation categorical from it and testing the categorical*.

(Quine 1995: 43–44, emphasis added)

Since the observation categorical's relation to its compound observation sentences is likewise deductive in virtue of its being an implication (cf. *Pursuit of Truth*, p. 12), we arrive at full fledged hypothetico-deductivism. Thus, whereas in the practical decisions of scientists

much ... is accepted without thought of its joining forces with other plausible hypotheses to form a testable set[,] ... having reasonable grounds is one thing, and implying an observation categorical is another. / ... implication ... is the lifeblood of theories, or perhaps better the finger of their fate. *It is what relates a theory to its checkpoints in observation categoricals.*

(ibid. 48–49/51)

- 26 It is very important not to confuse the point I just made with the very different claim, which I do not endorse, that Quine's *explanation of acceptance-behavior* regarding scientific theories or scientific/epistemic reasoning did not advance over his logical positivist predecessors. Quite the contrary. But while Quine pushes for the recognition of the *inevitable encroachment of pragmatic arguments in scientific reasoning*, he does not adopt a non-traditional, pragmatic conception of *evidence* (according to which such things as historically salient alternatives, known limitations on instrumentation to test predictions, available and plausible idealizations and background theories, appreciation of the support of theories on account of other parts of the empirical knowledge with which they make epistemic contact, etc. get to be part of calculating the *evidence* or the *support* of theories, and thus of the justification of selecting some but not others on account of the available perceptual, hypothetico-deductive and other evidence). To be sure, Quine's holism was one of the most important turning points in making such conceptions available, but *Quine himself* did not significantly stray from the reductive picture of evidence that he inherits from traditional logical empiricist methodology.
- 27 The line of criticism rehearsed in this paragraph has been most convincingly developed in Laudan (1990) and Laudan/Leplin (1991).
- 28 I take the formulation and name of the thesis from Laudan (1990: 324).
- 29 For this line of objection, see Bergström (1993), Bergström (2004).
- 30 The expansion from confirmation skepticism to loss of content is famously developed in McDowell (1994: 130–35).
- 31 I borrow this expression from Fine (1996: 153).
- 32 These commitments, unequivocal anti-instrumentalism about scientific theories and a total rejection of the theory-observation distinction, are two further salient methodological incompatibilities between Putnam and Quine. Quine's wavering between what he calls 'robust realism' and instrumentalist passages is notorious, and his pursuit of a modified theory–observation distinction is clearly present in his persistent preoccupation with an adequate definition of observation sentences, the parts of the web of belief that are (a) exempt from Duhemian considerations, (b) universally accepted on the same causal prompts, (c) basic justifiers, and (d) the only source of empirical content. Given some adequate and complete conception of observation sentences, and given the semantic and

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epistemic roles Quine attaches to observation sentences, a resurrection of the theory–observation distinction as that between observation sentences (and simple compounds thereof, as observation categoricals) and the rest of the web is inevitable. Quine is *de facto* elaborating a variant of precisely the theory–observation distinction that Putnam completely abandons without remnant in favor of a contextual distinction between justifying and to-be-justified beliefs as early as “What theories are not” (1960). For an illuminating discussion of the role of this point in Quine’s naturalized epistemology, cf. Williams (1991: 261–65).

- 33 Cf. Bennett (1971). The reference to knowledge empiricism is often used in similar ways as I do here by Boyd (cf. Boyd 2002: sec. 2).
- 34 In Putnam’s words, they are “empirical in the sense of being about the world” (Putnam 1967: 109).
- 35 I use this expression in the sense of Field (2000), who calls a statement “default reasonable” when “it is reasonable to believe or employ it without first adducing evidence or arguments in its favour” (124).
- 36 John Earman argues a similar point with the example of determinism. In an epistemic situation where we don’t yet know exactly in many important contexts how to transform the existing deterministic laws in not-yet-existing indeterministic laws, for example, that fact would engender “a scientific claim to be argued over the way one argues over other deep scientific claims, *none of which* ever gets *definitely* settled by the dictates of experimental evidence” (Earman 1993: 14).
- 37 In the case of what is nowadays referred to as “folk psychology”, he says: “The acceptance of [a] conceptual system, or explanatory scheme, is justified, as is the acceptance of many an empirical hypothesis, by the joint facts of explanatory power and no real alternative” (Putnam 1969: 447). Recently, Philip Kitcher defends the contextuality of the *a priori* on precisely on this reason, which he terms the denial of identifying the apriority of a part of knowledge with its “tradition-independence” (Kitcher 2000: 90). Historical experience (or experience of working with a theory) is here on the same epistemological footing with sensory experience when it comes to explaining the (un-) revisability of parts of scientific knowledge (like mathematics) that serve as *a priori* in the weak sense.
- 38 This defense of methodological apriority has been argued in great detail in Sober (1999) and Sober (2000), esp. § 12 (“Duhem’s Thesis”).
- 39 Allison (1983: ch.1) uses the term “epistemic condition” in a similar sense. The term also occurs in a similar context in Putnam (1994a: 258).
- 40 This point has been convincingly argued in Laudan/Leplin (1991). My argument can be seen as parallel to that of Stathis Psillos, who uses Leplin/Laudan’s insights to construct an argument in favor of realism by exploiting its power to disarm underdetermination-based anti-realist ploys (cf. Psillos 1999: 168–71). Relatedly, Okasha (2002), argues on account of Duhem’s thesis, i.e. the need for determinate auxiliaries in order to derive *any* empirical consequences, that holism is incompatible with global underdetermination (307ff.). In a similar way, I am using both points to disarm Quine’s principled argument against the methodological relevance of the *a priori*–*a posteriori* distinction insofar as it must be based on the global (proto-underdeterministic) symmetry-thesis.
- 41 The generic notion of apriority developed in the text is not, like traditional apriority-concepts, modeled on the paradigm of logical truth but developed from within the practice of empirical justification. The relevant distinction there is between hypothetical and non-hypothetical elements in justification, which itself presupposes standards of valid deductive inference. Thus, as such, it is of only limited immediate explanatory use for the status of logical truth. On its own, moreover, this concept of apriority does not suggest the idea of statements that

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are impossibly false but rather that of fixed points of a particular inquiry. Putnam noted this in Putnam (1983a) and Putnam (1994), where he says that reflecting on logical truth shows that their status and unrevisability is not merely owed to contextual apriority of the kind involved in the role of, e.g., conservation principles in physics. But then again, it surely is no mistake to represent logical truths as non-hypothetical and contextually a priori. The notion turns out norms of deductive inference expressed in logical truths as in most (in the core of first order logic: *all*) cases non-hypothetical. The real complaint might be that this is *too little* to get right the *specific way* in which logical truths are non-hypothetical, and too little to make us see why *all* logical truths are equally non-hypothetical if any are, and how they can *acquire* this status when the rules of inference they articulate are everywhere presupposed. This is right, but perfectly consistent with saying that we have no theoretical reasons to revise any of them, as trifling and quaint as that may be. The most basic laws of logic might, for all we know, turn out to be non-hypothetical and contextually a priori for *all debatable cases* (i.e. 'in all possible worlds') and thus preclude evaluable talk of their 'revisability'. It would be the task of philosophers of logic to spell out what specific kind of privilege these laws have, and what its basis is. That a notion of epistemological privilege that grows out of methodological concerns that have their paradigms in principles of the *empirical sciences* would not automatically come equipped with an account of logical truth but rather require us to work one out as a *special and peculiar case* should come as no surprise.

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