# Background Theories and Total Science

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**Abstract**

Background theories in science are taken both as proof and as disproof that theory choice is underdetermined by data. The proof is often thought to threaten the possibility of responsible scientific theory choice. Properly understood, it shows only that scientific inference is fallible and contextual. This is compatible with the disproof, which shows that no theory choice can be timelessly or noncontextually underdetermined. Philosophers have often replied to the disproof by focussing their attention on Total Sciences rather than theories. If empirically equivalent Total Sciences were at stake, then there would be no background against which they could be differentiated. I argue that Total Sciences are philosophers' fictions and that no respectable underdetermination can be based on them.

## 1 Introduction

Background theories in science are used to both prove and disprove that theory choice is underdetermined by data. The alleged proof appeals to the fact that experiments to decide between theories typically require auxiliary assumptions from other theories. If this generates a kind of underdetermination, it shows that standards of scientific inference must be appropriately contextualized. The alleged disproof appeals to the possibility of suitable background theories to show that no theory choice can be timelessly or noncontextually underdetermined: Foreground theories might be distinguished against different backgrounds. Philosophers have often replied to such a disproof by focussing

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their attention not on theories but on Total Sciences. If empirically equivalent Total Sciences were at stake, then there would be no background against which they could be differentiated. Below, I offer several reasons against the reconstrual underdetermination in terms of Total Sciences.

A word about terminology: *Underdetermination* is variegated and multiform. In this paper, I am concerned with underdetermination arguments that appeal to the rôle of background assumptions and auxiliary hypotheses in science. As such, there are forms of underdetermination that don't concern me here. To put it in a general form, however, we can say that underdetermination obtains when scientists are unable to responsibly decide between rival theories. Following common characterizations of underdetermination [Lau98][Mag03], say formally that underdetermination is a three place predicate that obtains for a set of rival theories, a standard for what would count as responsible theory choice, and a set of circumstances in which the standard allows no responsible choice among the rival theories.

## 2 How auxiliary theories make inference less than certain

The familiar Duhemian argument for underdetermination begins with the observation that experiments in modern science often require appeal to auxiliary assumptions for their probative force. For the sake of concreteness, consider the claim that the Earth is flat and the counter-claim that the Earth is round— less colloquially, that the Earth is an oblate spheroid. Call these claims $T_F$ and $T_R$ respectively. There have been many adherents of $T_R$, of course, and many attempts to demonstrate its superiority over $T_F$. Copernicus provides a typical argument:

> This [spherical] form of the sea is also discerned by sailors, seeing that land is visible from the top of the mast, even when it cannot be seen from the deck of the ship. And conversely if a light is held on the top of the mast, it appears to those on the shore to gradually descend as the ship moves away from land, until at last it disappears like the setting sun.[1]

The idea is simple enough. If the sea were flat, then an observer who could see a ship clearly should be able to see both the hull and the mast, as in figure 1a. Contrariwise, since the sea is curved, an observer may see the mast even at a distance at which the hull is not visible, as in figure 1b. The latter of these is observed, and the observation decides between these two depictions. The catch is this: The test implicitly assumes that light travels in a straight line— but of course the rectilinear propagation of light is independent of $T_F$ and $T_R$.

Without the implicit assumption, the observation may not favor $T_R$. Suppose $T_F$ is true— the Earth is flat— but that light sags slightly between the

[1] Bk. I ch. 2 of *De Revolutionibus*. The translation is my own.

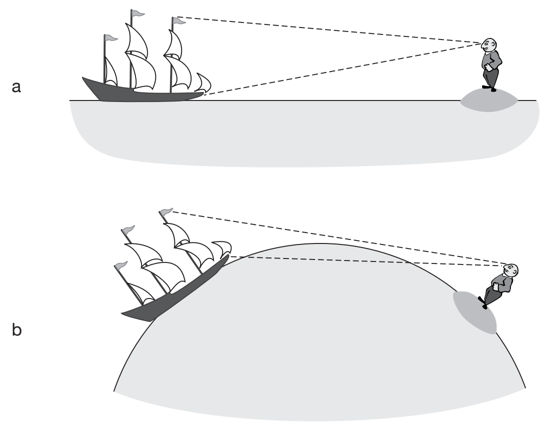


Figure 1: (a) If the Earth were flat, then an observer on the shore would see both the mast and prow of the ship if he could see either. (b) Since the Earth is round, the observer sees the mast even when the hull is occulted by water.

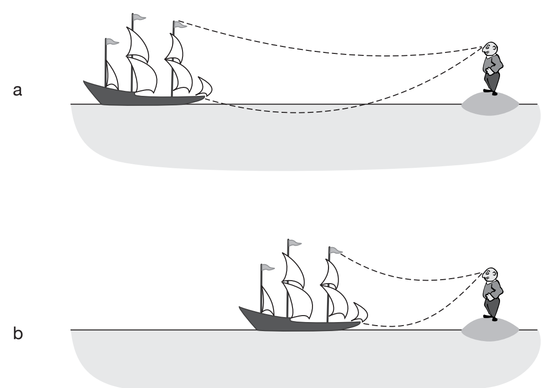


Figure 2: (a) Light beams sag between the ship and the observer, so the prow of the ship is occulted by water even as the mast is visible. (b) As the ship approaches, the observer can see both the prow and the mast.

object and the observer, curving down toward the surface of the Earth. At a distance, the light from the hull of the ship may sag down into the water while the light from the mast reaches the observer. Thus, the observer sees the mast even as the hull has passed from view. This situation, depicted in figure 2, would yield the relevant observation.[2]

Call the assumption that light travels in a straight line $T_L$, and call the observation of the mast of the ship when the hull is out of sight $O$. Although $O$ is offered as evidence of $T_R$ over against $T_F$, the best it can do is show that *if* light travels in a straight line *then* the Earth is round. One may conclude that this conditional is true, but not that $T_R$ is true or that $T_L$ is false.

Cases like this are used to underwrite what is sometimes called the Duhem-Quine (or DQ) Thesis: Theories are not tested in isolation.[3] As Quine puts it, theories "face the tribunal of sense experience not individually but as a corporate body" [Qui53, p. 41]. The point may be stated as a lesson about underdetermination. The experiment was aimed to decide between $T_R$ and $T_F$. This theory choice is underdetermined for a standard of judgement that denies you the assumption $T_L$; such a meager standard allows you only to conclude only $T_L \rightarrow T_R$. Worse still— since there is a great deal more to optics than the rectilinear propagation of light— the inference involves still other auxiliary assumptions $T_M$, $T_N$, and so on. If the DQ Thesis is correct, then the observation allows you only to conclude the rather uninteresting conditional $(T_L \& T_M \& T_N \& \cdots) \rightarrow T_R$.

So, $O$ only yields $T_R$ given an indefinite number of other assumptions, where the yield is understood as deductive entailment. We might have arrived at this conclusion directly. Let $L$ be the set $T_L, T_M, T_N, \ldots$. By hypothesis, $(O \& L) \rightarrow T_R$, and there is no $M \subset L$ such that $(O \& M) \rightarrow T_R$. We observe $O$. These assumptions validly entail $L \rightarrow T_R$ but leave $T_R$ indeterminate. Suspiciously, the conclusion follows without any consideration of the content of $L$, $O$, and $T_R$ and without any reflection on methodology or confirmation.

The crux of the matter is whether standards of responsible judgement should lead you to assume $L$ or treat it as being as much in question as $T_R$. One might argue that the right standards should promise us certainty. $T_L$ is open to revision, so conclusions drawn on the basis of it are *a fortiori* fallible. But no certain knowledge is to be had. Whether we may rely on auxiliary hypotheses to decide between rival theories depends on their actual content and on our epistemic situation. Little more can be said in the abstract. The Duhemian argument seems to fail.

[2] This example appears in Copi and Cohen's introductory logic, wherein the authors attribute it to C.L. Stevenson. They invoke it to show that no 'crucial experiment' can be deductively binding, but concede, "Within the framework of accepted scientific theory that we are not concerned to question, a hypothesis *can* be subjected to a crucial experiment" [CC90, p. 447].

[3] The phrase 'Duhem-Quine Thesis' is sometimes used as a synonym for 'underdetermination' (see, e.g., [Kou03, p. 23]). Given the general characterization of underdetermination that I gave in §1, the DQ Thesis and the problem of empirically equivalent rival theories count as varieties of (rather than synonyms for) underdetermination.

## Finding Duhem in the Duhemian Argument

Although Quine is often cited as having established the force of underdetermination in 'Two Dogmas of Empiricism', he writes there that the "doctrine was well argued by Duhem" and offers it without much positive argument [Qui53, p. 41 fn. 17]. Admittedly, Duhem does seem to draw a strong conclusion. He writes "that comparison is established necessarily between the *whole* of theory and the *whole* of experimental facts..." [Duh54, p. 208, italics in original].[4] It is important to note, however, that this passage is a quick summary of his position, offered after it had been developed with greater care in prior sections. Moreover, Duhem did not see his holism as entailing any pernicious underdetermination. It does mean that theory choice cannot be a matter of deductive or logical certainty, but it leaves room for fallible theory choice. Duhem explains that "what impels the physicist to act thus is *not* logical necessity. It would be awkward and ill inspired for him to do otherwise, but it would not be doing something logically absurd..." [Duh54, p. 211, italics in original].

We should, of course, acknowledge that scientific theory choice is not a matter of deduction. It lacks even the plausible pretense of certainty. Duhemian concerns show us that, given a decontextualized standard of judgement, underdetermination is rampant. If there are standards of good sense that allow querists in a context to decide between theories, as Duhem thought there were, then the underdetermination disappears when we consider choices relative to those standards. Duhem is, I concede, not always as clear on this point as he could be, and commentators have often recapitulated the ambiguity.[5]

Duhem thinks that "good sense" should save the physicist from awkwardness and ill inspiration, but also that

> these reasons of good sense do not impose themselves with the same implacable rigor that the prescriptions of logic do. There is something vague and uncertain about them; they do not reveal themselves at the same time with the same degree of clarity to all minds. Hence the possibility of lengthy quarrels between the adherents of an old system and the partisans of a new doctrine, each camp claiming to have good sense on its side, each finding the reasons of the adversary inadequate. [Duh54, p. 217]

This reveals possibilities for underdetermination. On the cusp of controversies, the evidence will be insufficient to settle matters between rival camps— not because many scientists are undecided between rival views, but because good sense is vague enough to permit disagreement. Yet, new evidence is collected, old evidence is reconsidered, and each doctrine is run through its paces. In time, the question may be settled. There is not some instant in time before

[4] The critical turns in Duhem's argument occur in his ch. 6 §§2–3, 8.

[5] For instance, Laudan treats what can "carry logical weight" as an issue of whether "a scientist is forced to relinquish" an hypothesis. If the responsible theory choice is the choice that a scientist is *forced* to make, underdetermination will be ubiquitous. Yet Laudan also allows for responsible choice in a less draconian sense; he concedes that one experiment he considers "would cause a rational person to cease to expound [the hypothesis]" [Lau65, p. 299].

which the old theory is *the* reasonable choice and after which the contender is triumphant, but agreement may be secured by an array of new evidence along with the inconstant nudgings of good sense.[6] Note, however, that this agreement may come about even though the theories in question still rely on auxiliaries, and reasonable disagreement may occur even when the interlocutors agree on the relevant auxiliaries. There is a kind of underdetermination that, as Duhem might say, follows from the vagueness of good sense, but it is neither ubiquitous nor established by scientists' reliance on background theory.

## 3 How auxiliary theories make empirical equivalence impossible

In the last decade or so, the prevalence of background assumptions has underwritten arguments *against* underdetermination— principally in discussions following Laudan and Leplin [LL91].[7] Scientists utilize a host of auxiliary assumptions and collateral information in performing experiments, as is readily seen by considering examples like the one in the previous section. Suppose, then, that two theories $L$ and $L'$ make no predictions that would allow us to differentiate between them. This empirical equivalence might be taken to warrant a conclusion that the choice between $L$ and $L'$ is underdetermined. What would the scope of this underdetermination be? It would include our present circumstance, but we may imagine circumstances it would not include. Suppose we learned that $(L \to O)$ and $(L' \to \neg O)$ for some observable phenomenon $O$. The theories would not then be empirically equivalent, and we could responsibly decide between them.

Of course, suitable revision of the rival theories would repeat the underdetermination at the level of theory cum background theory. We would be able to decide between $L$ and $L'$, but the choice between $L\&(L \to O)\&(L' \to \neg O)$ and $L'\&(L \to \neg O)\&(L' \to O)$ would remain underdetermined. Yet why do we believe $(L \to O)$ and $(L' \to \neg O)$? Surely not merely because they would defuse the underdetermination between $L$ and $L'$! Say that we believe them because they are entailed (with some assumptions about initial conditions) by a well-tested and widely-believed background theory $X$. We may attempt to cook up some alternate $X'$ that would enjoy the same empirical support as $X$ but entail $(L \to \neg O)\&(L' \to O)$, but there is no guarantee that this will be possible. The theory $X$ may have systematic connections to the whole body of science, such that any $X'$ sufficiently different to work here would introduce a panoply of anomalies.

Earman objects to Laudan and Leplin's argument in this way: Either the $X$

[6] This is roughly what Kitcher [Kit93, ch. 6] calls the 'compromise model' of scientific change.

[7] Although the argument is not original to Laudan and Leplin, they have pressed it with the greatest vigor. Boyd considers it a "standard rebuttal" to empiricism, but thinks it can be effectively countered by shifting attention to Total Sciences [Boy82, pp. 650–1]. See also Churchland [Chu85, p. 38].

appealed to is a contestable hypotheses like $L$ and $L'$ or it is not. If the former, then the rivals under consideration are no longer $L$ and $L'$— the rivals are instead $(L\&X)$ and $(L'\&X)$. "The result is sidestepped… but that is changing the subject since what counts as the hypothesis has been changed." If the latter, then $X$ is presumed. This would amount to dogmatism, since auxiliaries like $X$ "must go beyond the empirical evidence… and thus their epistemic status will be just as open to question as that of the [hypotheses]." [Ear93, p. 35]

I don't think that there is a sharp point to either horn of this dilemma. First: Even if Earman were right that this changes the subject, that doesn't make it legerdemain. It is perfectly legitimate to substitute a determinable theory choice for an underdetermined one, a tractable problem for an insoluble one.[8] Second: The problem is soluble, because $X$ does not stand in the same need of justification as $L$ or $L'$. The auxiliary goes beyond the evidence and so too is open to question— it is not in principle shielded from scrutiny.[9] What's methodologically crucial is that a community or an individual scientist may hold an accepted background theory fixed for the purpose of some investigation. As Norton [Nor03] argues, we can't understand inductive inference if we don't recognize the rôle of material assumptions.

# 4 The move to Total Science

At this point, many philosophers are tempted to ask about the choice not between theories but between packages of *Total Science*. A Total Science is the collected body of *all* scientific knowledge at a time. Some authors call this a 'total theory,' but this is at best misleading. As I argue below, a Total Science is not a scientific theory in any ordinary sense.

Adding a theory $T$ to a given Total Science $\mathcal{S}$ produces a new, different Total Science. Thinking of theories as sets of propositions, one might think of a Total Science as the union of all the theories known to science and think of the combination as $\mathcal{S} \cup T$. Thinking of theories instead as sets of models, one might think of a Total Science as the intersection of known theories and think of the combination as $\mathcal{S} \cap T$. In order to remain neutral between these and more exotic possibilities, let $(\mathcal{S} \oplus X)$ stand for the resultant Total Science when theory or observation $X$ is added to Total Science $\mathcal{S}$.[10]

Let the initial state of Science prior to any knowledge of $L$, $L'$, or $X$ be given by $\mathcal{S}$. The choice between $(\mathcal{S} \oplus L)$ and $(\mathcal{S} \oplus L')$ is, by assumption, underdetermined. *This* underdetermination cannot be resolved by appealing to $X$. Appeal to $X$ is a *non sequitur*, since $X$ is not part of the Total Science $\mathcal{S}$. If we learned

[8] If you can't stop the rain, get an umbrella— and so on.

[9] Going beyond evidence is actually beside the point, since querists may call the evidence itself into question as needed.

[10] Churchland defines a "global theory" as the "global configuration of synaptic weights" in the neural networks of a scientist's brain [Chu89, p. 188]. If we understand this as a Total Science, giving an appropriate construal of '$\oplus$' will be no simple matter— yet there must be some construal, since the scientist's brain certainly adopts *some* global configuration after she makes an observation.

$X$ from some experiment, then we would transition to a different total science. That choice would be between $((\mathcal{S} \oplus X) \oplus L)$ and $((\mathcal{S} \oplus X) \oplus L')$. It would not be underdetermined, true, but that is a different choice between different Total Sciences. Perhaps the choice among $\{(\mathcal{S} \oplus L), (\mathcal{S} \oplus L'), ((\mathcal{S} \oplus X) \oplus L), ((\mathcal{S} \oplus X) \oplus L')\}$ is not underdetermined, but that too is beside the point. On this approach, strong conclusions are drawn from underdetermination that obtains between empirically equivalent Total Sciences.

Several authors appeal to Total Science in this way. Quine said, "The unit of empirical significance is the whole of science" [Qui53, p. 42]. Hoefer and Rosenberg, mindful of Quine, write that "the thesis of underdetermination of theory by evidence is about empirically adequate total science..." and conclude rightly "that Laudan and Leplin's arguments for the defeasibility of empirical equivalence have no application in the context of systems of the world"— that is, in the context of Total Sciences [HR94, pp. 594, 598].[11]

This can be seen as a reformulation of the Duhemian argument, one that cannot be answered merely by an appeal to fallibilism. Even allowing that it is legitimate to appeal to background theories, empirically equivalent Total Sciences cannot be distinguished by some empirical test because all of the available background theories are already included in each Total Science. At the outset, I said that underdetermination concerned the choice among rival *theories*. If we substitute Total Sciences for theories, it's hard to make sense of choices— underdetermined or otherwise.

In the remainder of the paper, I offer several argument to undercut the move from theories to Total Sciences.

**We could never in any meaningful sense choose between Total Sciences.** Querists never face choices between Total Sciences. In actual enquiry, there is some matter in question and other matters presumed. It is possible, of course, that a querist should call some assumption into question. Nevertheless, there is no moment when everything is up for grabs. This means that underdetermination about Total Sciences could have no practical upshot whatsoever. More than that, underdetermination between Total Sciences could not have any part in a methodology we could actually employ. A real method must tell us where to go from *here*. The underdetermination of Total Science for the abstract querist is at too far a remove from enquiry and methodology to hold lessons for concrete querists like us.

**Although our present science is often treated implicitly as a Total Science, it seems likely that actual science doesn't constitute a well-defined Total Science.** It is fair to ask what Total Science would look like. Kukla writes that "a total science is nothing more or less than the conjunction of any 'partial' theory and *all* the auxiliary theories that we deem to be permissible. It does not matter which partial theory we begin with— the end result will be the same" [Kuk96, p. 143, my emphasis]. Yet there is no guarantee that

[11] See also [Kuk98, pp. 63–66].

this conjunction either exists or is well-defined. Philosophers' examples usually take the the form of comparing the usual Total Science with a slight revision of the same; as with the well-worn example of flat space versus curved space with appropriate corrections, we are given single theories as stand-ins for Total Sciences. We are invited to think that a Total Science is something we understand well enough. Since there is a body of scientific knowledge, then its makes sense to think of it collected at a time— right?

Who would we ask and where we would look if we wished to know the state of Total Science? Perhaps Total Science includes only matters about which all scientists agree. Complete consensus is a rare thing, though, so this would count only the tiniest subset of what might plausibly be billed as scientific knowledge. This may miss the point, since underdetermination of can be taken as a lesson about the situation of an individual querist.[12] Even so, it is hard to know how the beliefs of a single scientist could be seen as a Total Science. The scientist may have some sense of which beliefs she believes *qua* scientist and which ones she believes *qua* citizen or consumer, but this will not be a sharp division. Moreover, what she believes *qua* scientist may be inconsistent; she believes the theory of relativity, but also quantum mechanics. It is impossible, I suggest, to sum up her beliefs as a Total Science— if she does not believe even one Total Science, why should it matter if she could not decide between several?

**Total Sciences could not be compared to evidence, so they could neither be determined nor underdetermined by data.** Underdetermination of Total Science is often said to obtain between empirically adequate and equivalent Total Sciences. We can think of these wholly adequate Total Sciences as sciences of the end times, sciences which have answered all empirical questions as adequately as questions can be answered. Thinking in these terms makes it irrelevant that present actual science doesn't constitute a Total Science,[13] but it exacerbates the disconnect between underdetermination and methodology. Hoefer and Rosenberg concede that "we can never be in a position to know a purportedly empirically adequate total theory is in fact a total theory or empirically adequate. But this epistemological truism does not undercut the conceptual point that two empirically adequate total theories would be nondefeasibly underdetermined by the evidence" [HR94, p. 595].[14] This conceptual point that some Total Sciences may be "nondefeasibly underdetermined" loses its force if no Total Sciences could ever be defeasibly determined.

The appeal to Total Science precludes the invocation of background theories by packaging the background theories in the rival Total Sciences. Yet since

[12] It is tempting to say that there is no principled epistemic difference between underdetermination for the individual and for the community, but Longino argues that underdetermination is resolved by *socially accepted* background assumptions [Lon90]. Given my purposes here, I don't think I need for this to be one way or the other.

[13] Since we must still imagine some future actual science constituting a Total Science, the previous objection is not entirely vitiated. Will some future state of science be so different from its present state?

[14] They attribute this "truism" to personal correspondence with Leplin.

observations, data, and the outcome of our experiments are are also part of our Total Science, rival Total Sciences could never be compared against the evidence— there is no evidence outside of the Total Sciences against which they could be compared! Underdetermination of Total Sciences entered our discussion as a radical form of *the underdetermination of theory by data*, but it seems reasonable to think that only things that can be compared to data can be underdetermined by data.

**For there to be a well-defined Total Science, science must be unified in an inmplausibly strong sense.** Kukla treats underdetermination as an issue within the context of debates over realism. He deploys the notion of Total Science and concludes:

> Realists will undoubtedly wish to attack the notion of a total science. Admittedly, this notion has been severely underanalyzed by both friends and foes of [underdetermination]. But it remains to be seen whether its obscurities affect the role it plays in the argument for [underdetermination]. The prima facie case has been stated. The burden of proof is on realists to show why the total sciences version of the underdetermination argument fails. [Kuk98, p. 66]

Yet if philosophers have not overtly analyzed 'Total Science', they have said things that speak to the issue. For the last two decades at least, a growing number of philosophers have argued for a "picture of science as radically fractured and disunified" [Dup96, p. 101].[15] And these philosophers do not argue without precedent. Hacking points out that William Whewell, who coined the world 'scientist' as a moniker for a querist into nature, acknowledged the plurality of the sciences with titles like *The History of the Inductive Sciences* and *The Philosophy of the Inductive Sciences, Founded upon Their History* [Hac96, p. 37]. Yet the case of Whewell is not so clear; in the latter of these works, he writes that the aim of Science is to bring matters under general propositions "so as to form a large and systematic whole" [Whe89, p. 104]. Whewell figures in the long history of thinking of the sciences as a plurality, but also in the similarly venerable tradition of thinking that the sciences might make a Total Science. Without attempting to decide the matter for once and all, let me only cast serious doubt on Kukla's assessment of where the burden of proof lies. Hacking writes that, "The unity of science is rooted in an overarching metaphysical thought that expresses not a thesis but a sentiment. Since it is not exactly a doctrine, it lacks straightforward expression" [Hac96, p. 44]. It is only when we are in the grip of that sentiment that Total Science seems a plausible enough thing to constitute even a *prima facie* case.

Collecting the various sciences into one Total Science would require not just unity, but unity in a rather strong sense. The view that science is utterly fractured and disunified is not the only alternative to such monolithic unity. It

[15] On the disunity of science, see also: Dupré [Dup83], Galison and Stump [GS96], and Cartwright [Car99].

may be that special sciences are *integrated*, in the sense that they mesh together where their domains overlap [Far00], but that they are not so unified that they can be collected altogether as a Total Science.

**Underdetermination seems to suppose that there are two or more maximally good Total Sciences, but there is no guarantee that epistemic virtue provides that kind of ordering relation.** Even the unity of science would not necessarily entail the existence of a final, fully adequate Total Science. As Paul Churchland suggests: "Just as there is no largest positive integer, it may be that there is no best theory. It may be that, for any theory whatsoever, there is always an even better theory, and so *ad infinitum*" [Chu85, p. 46]. If possible states of science stand in an ordering relation such that there is no supremum, then there would be no final sciences— *a fortiori* not the two or more required for underdetermination.

## 5 Conclusion

The rôle of background theories in science ameliorates more than it aggravates problems of underdetermination. Duhemian considerations only amount to pernicious underdetermination if we demand that scientific theory choice be timeless and noncontextual. When we acknowledge that ampliative inference is fallible and contextual, the bugbear is beaten back. Underdetermination slinks away defeated and will not rally even when the banner of Total Sciences is raised.

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