Inconceivable Support Relations: Reply to Stanford

Philosophers are drawn to the Atomic Theory like a dog to an old shoe, but my results about realism and anti-realism in *Tracking Truth*, and the distinctive position I carved out on their basis, are independent of the fate of my comments about that historical case. I will defend those comments against Stanford’s objections below, but first I will explain the argument he ignored, because its results undermine not only historically important anti-realist positions, but also the approach via unconceived conceivables that Stanford’s criticisms (and his own pessimistic induction) depend on. My argument is not based on lack of imagination for alternative hypotheses. The issue is claims about equal evidential support that empiricist defenses of epistemological anti-realism, including Stanford’s, must and do appeal to. In Chapter 6 of *Tracking Truth*, I show that the best known probabilistic definitions of evidential support imply that the anti-realist’s key equal-support claims are either false or inconsistent with the view he wants to base on them. There are good reasons to think that it is not even possible to define a probabilistic notion of support that will do the job the anti-realist needs. Due to the role of auxiliary assumptions in determining evidential support relations, the only way of maintaining an anti-realist stance would be to adopt a purely syntactic criterion of confirmation, which most of us regard as a reduction ad absurdum.

It follows from this, as I explain below, that the anti-realist has not defended the claim that we are in principle, or even probably, unable to confirm high-level theories – for this, I say, we must wait and see, and make our best estimates, as I do with Perrin’s evidence for the Atomic hypothesis. I do not endorse the kind of realist claim that says we generally have a right to believe our successful hypotheses that go beyond observables. Some successful hypotheses are better confirmed than others, and others are not at all. However, since the anti-realist’s general skeptical arguments fail, we also do not need such general claims in order to have a right to confidence in those particular hypotheses for which we do have good evidence. The interesting questions that remain are how we do and should make such judgments.

The nub of the issue between the two camps in the epistemological realism-anti-realism debate has always been whether we have a right to believe our scientific theories – in some sense that includes at least some of their claims about unobservable matters – on the basis of our evidence.¹ Answers to this question depend on what our evidence actually is, and on the definition or criteria for evidential support. The former is a factual matter, which can be discussed by making substantive claims in individual cases. But philosophers in this dispute have often also made general assumptions about what our evidence is, such as the empiricist assumption that our evidence includes only observations. The latter, the criteria for what counts as evidential support, is an issue barely even noted in discussions of realism.

¹ This qualification is necessary because of realist strategies that weaken the claim from being about the approximate truth of our successful theories to being about, for example, some structural features of those theories. E.g. Psillos 1999.
and anti-realism, and this issue and its implications are what I have the most to say about.\footnote{The exception to the silence on this question is Richard Boyd’s (1973), whose argument is discussed in \textit{TT} (2005), Ch. 6.}

The kind of support the realist needs is a relation in which evidence gives more reason to believe a hypothesis than we had before, a concept which also allows us to talk about evidence supporting one hypothesis more than another.\footnote{It also allows us to talk about evidence raising the probability of a hypothesis to a level higher than some specified level, but, as will become clear, this notion is not needed to respond to the anti-realisms considered here.} This is also called “incremental confirmation,” which I will frequently shorten to “confirmation.” But the anti-realist needs to make strong assumptions about this relation in order for his arguments to succeed. Since we now have several decades of research on the possible conceptions of this kind of evidential support and the commitments required by the various conceptions, I have used these tools to evaluate the implicit assumptions made in arguments for and against realism, and argued that the anti-realist cannot succeed at all.

Underdetermination arguments and van Fraassen’s arguments for Constructive Empiricism (van Fraassen 1981) appeal to an empiricist epistemology whose main assumption, of course, is that the only evidence we have for learning substantive things about the world comes from experience, or observations. The empiricist moves from there to the claim that a given set of observations equally confirms multiple theories that imply or are consistent with or equally probabilify (are empirically equivalent to) it. The multiple theories to be compared may have different theoretical assertions, as is the aim in underdetermination arguments, or they may be a theory and the claim that that theory is empirically adequate – makes true claims about observables, as in van Fraassen’s view.

However, the assumption that observations are our only type of evidence is not enough to get us to the conclusion that all theories that imply or probabilify, or are consistent with, a given set of observational evidence are equally well confirmed by it. For that we would need another assumption:

\begin{center}
\textbf{EEEI: Empirically equivalent theories are evidentially indistinguishable.}
\end{center}

a claim that empiricists have never defended, and which does not follow from the claim that our only evidence is observations. Notice that the issue here is not whether claims that go beyond observations (e.g., a priori principles, considerations of aesthetics or simplicity) could give us more reason to believe one theory over another that is empirically equivalent to it. That possibility would effectively take our sources of support to include more than observations, and thereby automatically depart from empiricism. In \textit{TT} I am addressing anti-realists on their own terms, so we are assuming that the evidence set includes only observations. The point is that the claim that the evidence set contains only observations does not imply that the set of statements it can differentially confirm contains only observational statements. To assume so would be to assume that if a property is not in the
source then it cannot be in the results. An example of this would be buying without scrutiny Descartes’ argument that God exists because no imperfect being could give him the perfect ideas he has in his mind, or to assume that parents neither of whom has hazel eyes could not give hazel eyes to their children. Evidently, whether a property not in the source could nevertheless be in the product depends on the properties of the process or relation that gets us from the one to the other, in this case evidential support.

Undetermination arguments need to assume EEEI. Such an argument needs to go from the claim that multiple theories imply (or probabilify) all of our observational evidence to the claim that our evidence gives us no more right to believe our theory than to believe those others. The reason Constructive Empiricism needs the assumption depends on how we read van Fraassen’s argument, but EEEI is assumed in both interpretations. Some think the argument is that the observational evidence we have cannot confirm a theory any more than it confirms the claim that the theory is empirically adequate. Since the theory is strictly logically stronger than the claim that it is empirically adequate, our evidence would need to confirm the theory more in order to make it worth the extra risk for us to believe the logically stronger theory rather than merely believing the hypothesis that it is right about all the observables. The role of an equal-support claim is clear.4

Though the foregoing interpretation of his argument is one van Fraassen probably had in mind, I see another argument that makes better sense of his various comments about false courage, and his somewhat odd denial, for epistemology, of the maxim that we might as well hang for a sheep as for a lamb. (van Fraassen 1980, 72) On this version the claim is that no set of observational evidence can disconfirm a theory more than it disconfirms the claim that the theory is empirically adequate. Because for all the evidence (observations) we can gather the punishment for holding a theory that is false would be no greater than the punishment for holding the false claim that it is empirically adequate, the sheep (belief in the theory) is no more valuable than the lamb (belief that the theory is empirically adequate):

… [Since] the extra opinion is not additionally vulnerable, the risk is … illusory, and therefore so is the wealth. It is but empty strutting and posturing, this display of courage not under fire. (van Fraassen 1985, 255, emphasis in original)

4 One might notice that merely showing EEEI false will not be enough to undermine this argument. One must also show that it is false in a particular direction, namely, that it is possible for a set of observational evidence to confirm a theory more than it confirms the theory’s set of observational consequences. Chapter 6 of TT shows how this can occur, even realistically. One might wonder how this direction is possible since H is logically stronger than the claim, CE(H), that H is empirically adequate. Does this not mean that P(e/H)/P(Pe/-H) is always greater than P(e/CE(H))/P(e/-CE(H), which means CE(H) is automatically more supported by e than H is, since –CE(H) eliminates many more possible ways for e to be true than H does? No, it does not, for reasons that are precisely the issue in this paper. Conditional probabilities do not depend merely on the logical relation of e and a hypothesis, but also on substantive assumptions, also called “auxiliaries” following Duhem. The ultimate reason this is a natural possibility is that evidential support is not a syntactical relation, and probabilistic definitions of support abide by this lesson taught to us long ago by Nelson Goodman. Many relevant illustrations of this phenomenon follow in the paper.
This intriguing argument depends on the general claim that a theory cannot be more
disconfirmed than the claim that it is empirically adequate, which is an implication of EEEI. The
derivations in $TT'$ that show EEEI is false on the most adequate measures of confirmation can also be used to show, in particular, that on these measures a set of evidence can disconfirm a theory more than it does the claim that it is empirically adequate. Van Fraassen’s argument will be hard to defend for this reason, and for other reasons discussed below not defensible at all.

I explain below why if we nevertheless grant the use of a less adequate measure, the Constructive Empiricist cannot avoid a skepticism in which observational evidence cannot confirm empirical adequacy claims either, unless he becomes a realist in the specific sense of allowing that we can have reason to believe (have some confidence, and differential confidence in) claims about unobservables. He is left with a choice between realism, incoherence, and a radical skepticism he disavowed. van Fraassen might reply to my claim that he must admit it as possible for us to confirm theories involving unobservables, that he was only arguing that such a commitment is not obligatory and that a lesser one is quite rational. If I am right about the options – realism, incoherence, and radical skepticism – even that weaker conclusion is unfounded, because the commitments desired cannot be coherent.

We should note how audacious the anti-realist’s claim here is, which is the reason he
needs such a general claim as EEEI. He claims that there is no evidence scientists will ever
have that could provide differential support to one theory over another that is empirically
equivalent. One might wonder how we could possibly know that, given the regularity with
which scientists have surprised us by showing something was possible that had been
declared impossible. The anti-realist’s boasted epistemic modesty looks like something
different from this angle. He has a prayer of defending such a bold claim because, and only if, the anti-realist implicitly appeals to general definitions of and assumptions about what
counts as evidence and how evidential support works. Thus it is entirely appropriate and
exactly to the point that we criticize his conclusions through a discussion of the implications
of definitions he does or must espouse.

Before discussing how equal-support claims matter to Stanford’s approach via unconceived conceivables, and thus to his criticism of my comments about the atomic
hypothesis, I will briefly set out the results about EEEI by explaining how this assumption
fares on many of the measures of evidential support we know of. First, there are conceptions
of evidential support according to which EEEI is true, and they go some way toward an
explanation of why the assumption has seemed obvious. Empirically equivalent theories are
evidentially indistinguishable on both the hypothetico-deductive and the enumerative
induction model of confirmation. This is because both take whether a theory implies the
observation statements as the only guide to whether the observation statements support the

5 This is discussed more explicitly in terms of a voluntarist notion of rationality, in (van Fraassen 1989, 175, 179, 320)
theory, and empirically equivalent theories have all the same implications for observables. (Degree of support in between full and none is not definable on this view.) On hypothetico-deductive a hypothesis is confirmed (somewhat) by any evidence set it implies. The enumerative induction view is a more specific version of the same idea, which adds a logical form requirement that the hypothesis is a generalization and the evidence consists of instances of it. These views of evidential support are simple and highly intuitive, and they must be part of the reason EEEI has long been assumed without argument. However, for familiar reasons both views of confirmation are inadequate, to all appearances unsalvageable.

According to the more realistic probabilistic relevance accounts of evidential support, e supports h to some degree (incrementally confirms h) iff

\[ P(h/e) > P(h), \]

and lends disconfirmation to h iff

\[ P(h/e) < P(h), \]

And in cases where \( P(h/e) = P(h) \), no confirmation or disconfirmation is present. This conception of evidence also has the tools to render definitions of the notion of degree of evidential support. The familiar such measures we have, the Likelihood Ratio (LR) measure, Difference measure, and Ratio measure, all agree on the definition above of when confirmation and disconfirmation are present or absent. However, they diverge on the claim EEEI. On the LR and Difference measures the claim EEEI is false. (Roush 2005, Chapter 6) Of the familiar measures this leaves the ratio measure on which e supports h to degree r if and only if:

\[ P(h/e)/P(h) = r \]

Thus, the question whether the degree of confirmation of two hypotheses, h and h’, by e is the same would be:

\[ P(h/e)/P(h) =? P(h'/e)/P(h'), \]

which is equivalent to the question:

\[ P(e/h)/P(e) =? P(e/h')/P(e) \]

The P(e) terms cancel, so we have the question:
P(e/h) =? P(e/h'),

but these are equal by the assumption that h and h’ are empirically equivalent.6 I would guess that it is by implicitly assuming this measure that those like van Fraassen who are sophisticated about probability and its implications for rational confidence had no qualms about EEEI. The ratio measure has serious drawbacks compared to some other measures, especially the Likelihood Ratio measure on which EEEI is false. (See Roush 2005, Chapter 5.) So, being forced to appeal to it puts the anti-realist at a disadvantage. There are more probabilistic ways of defining degree of evidential support, but most we know of are versions of the three probabilistic measures just discussed, which are representative of the range of properties one will have to choose from in the case of probabilistic relevance. Of course, it would be for the anti-realist to show us there are congenial options for him there, and I suggest below a general reason why this will be very hard to do.

From here on I will assume the Ratio measure since that is the one most favorable to my opponents, but even though it gives the anti-realist EEEI his troubles are far from over. van Fraassen’s Constructive Empiricism is not meant to be a radical skepticism about science or life. His strategy for avoiding skepticism is to maintain that though observational evidence cannot confirm or disconfirm a theory in the right way, observational evidence can confirm (in our incremental sense of support) the claim that the theory is empirically adequate, true in all of its implications about observables. Call this latter claim CE(h). EEEI is true on the Ratio measure for all cases, including van Fraassen’s sameness of support claim, where h’ will be CE(h). But the problem with his attempt to avoid radical skepticism is that claims about unobservables are part of the content of that apparently more modest claim about confirmation of an empirical adequacy claim, because they are part of the probabilistic claim of confirmation, even as defined by the Ratio measure. It is often ignored that “P(e/CE(h)) > P(e)” is shorthand for the claim that the probability of e given CE(h) and the probability values for all other statements of the language is greater than the probability of e given the probability values for all other statements of the language.

Any language adequate to science contains of course all sorts of statements about unobservables, and many, many of them are relevant to whether or the degree to which a general claim (hypothesis) probabilizes specific experimental or observational outcomes. The claim that a what a theory invoking forces says about all observables makes probable what I see in these observations depends on the assumption, for example, that an unknown force was not present here, a force which combined with what the theory says about the world, or all observables, would have produced a different observational event here now. (A rigorous version of this example is presented in the book.)

One might protest that those assumptions will be the same for all the terms involved in a claim of comparative confirmation. But this is not to the point. Those claims about

---

6 Sameness of background beliefs was assumed in deriving the falsity of EEEI for all of these measures.
unobservables do not “cancel” out even when they are the same on both sides of a comparison because they are part of the content of a claim of confirmation. What those assumptions are makes a difference in general, even by the Ratio measure, to how much confirmation is present, even when we change them uniformly for every term in the evaluation. The Constructive Empiricist does not think he has reason to make commitments to such contents when they occur in theories. But he must suppose he has the right kind of evidence to make commitments about the unobservables that occur in statements claiming that observations support claims about all observables. The problem is that he can’t have it both ways.

Stanford admits the he must assume any particular experimental or observational situation uncontaminated by “exotics” while “insist[ing] that the “spirit of [the anti-realist’s] position is preserved by refusing to grant us substantive knowledge of facts about particular unobservable entities or processes of the sort she was inclined to deny in the first place.” (Stanford, 2009) But insisting won’t make it so. The anti-realist must provide a principled way of making the distinction between the kind of claim about unobservables that occurs in background assumptions needed for evidential support claims and the kind that occurs in theories, and the mere label “exotic” does not do that work. The label is not to the point anyway since background assumptions essential to an experiment involve claims about ordinary forces as well as exotic ones – claims about Newtonian-type forces present or absent, existing or not existing, and to what level, go into setting the background assumptions for a Newtonian experiment. Claims about which forces exist are obviously claims that could easily figure in a theory. The essential role of such assumptions in evaluating the support of theories was Duhem’s point in drawing attention to auxiliary hypotheses. And, in a probabilistic approach to confirmation a role for auxiliary assumptions is not an independent feature that is added on; it flows immediately from the nature of a probability function.

Vast numbers of auxiliary assumptions containing claims about unobservables, are always required to draw any observational consequences from a theory. This is a fact that the Ratio measure does in general accommodate, since it is not a syntactic measure but appeals to an entire probability function. However, in deleting commitments to those auxiliary assumptions that occur elsewhere in the probability function, the anti-realist would be using a degenerate case of the Ratio measure and the only thing left for evaluating whether there is confirmation of hypothesis by evidence would be the logical form of those two claims. No one, including van Fraassen, believes confirmation is a syntactic relation determined by mere logical form.

7 In this sense it is not a formal notion of confirmation, in the syntactic way we know is inadequate, despite its being possible to express it symbolically in the shorthand of “P(h/e)/P(h).” None of the probabilistic notions of confirmation discussed here are formal in the syntactic sense, since they use the whole probability function, not just the form of e and h, to determine how much confirmation is present, i.e., to determine the value of P(h/e)/P(e), a point which is frequently misunderstood. See, for example, Norton (2003).
One might object that a confirmation-theoretic treatment of van Fraassen’s argument for anti-realism is not probative because it assumes a syntactic view of theories.\(^8\) He assumes a semantic view, according to which a theory is true if one of its (fitted) models correctly represents the world, and empirically adequate if one of its (fitted) models is isomorphic to all possible appearances of the kind in question. But this difference does not save Constructive Empiricism. It provides a new definition of empirical equivalence – a given set of observables in any fitted model of one of the theories will be isomorphic to those observables in some fitted model of the other theory – but this is not a defense of the claim that empirical equivalence yields evidential indistinguishability (EEEI). For this, one needs to claim that whether either or both theories has a model isomorphic to the data (or a model with a greater degree of isomorphism) is the only relation between theories and data that can make the one theory epistemically preferable to the other. van Fraassen does not argue for this. To see that this claim is far from trivial, and that doubting it is quite sensible without ceasing to be an empiricist, recall the analogous claim of the hypothetico-deductivist, who said that the only relation between theory and data that affects the support of the theory by the data is implication. We know how that worked out. There is also an irony in the fact that the semantic view of theories leads us to a purely syntactic criterion (isomorphism) for whether a theory is favored by data.\(^9\)

The framework for judging theories via comparison of (degree of) isomorphism has a further relevant limitation. van Fraassen needs to convince us that believing merely that a theory, \(h\), is empirically adequate, \(CE(h)\), is better than, or equally rational as, believing that it is true. Even if we grant – purely for the sake of argument – that if two theories have models of the data that are isomorphic to each other then the data can’t give us epistemic reason to prefer the one to the other, i.e., grant EEEI, the choiceworthiness of the claim \(CE(h)\) cannot be made out on the only scheme we have been given for evaluating claims that go beyond actual observations (as \(CE(h)\) does). We could see how the argument goes if van Fraassen could defend the claim that \(h\) and \(CE(h)\) are empirically equivalent – nothing tells for one over the other so choose the weaker one. But \(h\) and \(CE(h)\) are not empirically equivalent according to his own definition. The claim that a theory is empirically adequate is not a theory and doesn’t have models about which we could ask whether the observables are isomorphic in them – \(CE(h)\) is a claim that \(h\) has a model that stands in a relation of isomorphism to certain observables. The question whether \(CE(h)\) is empirically equivalent to \(h\) is not well-formed. This is not a trifling matter of syntax either, since it means that contrary to appearances we haven’t been given any constraints for evaluating whether and

\(^8\), and van Fraassen does (1981, 78) make this objection to an earlier version of my complaint that was made by Richard Boyd (1973)

\(^9\) Given what we know about the inadequacy of syntax to capture evidential relations between theory and evidence in other representations of the issue, I suspect that the models-isomorphism view sweeps the role of substantive assumptions under the rug in some way or other. In other words, I suspect they are there but rendered invisible. If they are there, and if they contain assumptions about unobservables, then van Fraassen is stuck on the other fork of the dilemma I am discussing.
why it is preferable to believe h or CE(h). This makes room for mischief, which representing
the argument using probability avoids since it treats every theory (h) and statement (even
CE(h)) as having a probability and so being constrained to stand in coherence relations to
every other statement; probability also allows us to represent the concepts of greater, less,
and equal support explicitly in a way that applies to all claims in the argument, and so, to be
constrained by the implications of the concepts we adopt.

There are clear reasons to think that the problem making out equal support claims
will confront the anti-realist even if he invents a (probabilistic) measure of confirmation we
do not yet know about. This is because the empiricist’s difficulty in making equal support
claims comes from having to avoid commitments about unobservables, and every
probabilistic way of defining the relation between evidence (observables) and theory
(involving unobservables) that is needed for drawing out observable predictions will either
involve such substantive claims or be syntactical. The substantive linking claims will have
to include claims about unobservables, in order to making the observables relevant to the
unobservables. But if the linking claims involve claims about unobservables, then so does
any claim of confirmation, including the supposedly modest claim that observations support
claims of empirical adequacy. Thus the only refuge of Constructive Empiricism (and, as we
will see, Stanford’s pessimistic induction) would be to take commitments to substantive
linking claims to be irrelevant to the confirmation relation and look merely to the syntactic
form of e and h to find the confirmational relation between them. Note that the fact that the
hypothesis to be confirmed for the anti-realist, CE(h), is logically weaker than what the
realist would have needed to confirm, h, cannot be any kind of salvation here. The problem
here is not about the content of the hypotheses to be confirmed or not but about the content
of the claim of evidential support or equal evidential support.

I come to Stanford’s pessimistic induction over the history of science to say how the
equal support claim is relevant to his argument. His induction is designed to show that we
have reason to withhold confidence in our theories because we have reason to think that
there are unconceived conceivable theories that are equally supported by our evidence. It is
easy to be convinced that it is logically possible that there are trivially different theories that
imply the same as ours do about observables, but to his credit Stanford acknowledges that
that should not impress us too much. Instead he claims that there is good reason to think
there are full-blooded theories out there that are equally supported by our evidence, and they
are probably within the realm of what we could discover if we were further along. He builds
a case that such alternatives probably exist for us by arguing that there were cases in the past
in which we know a theory had conceivable rivals its proponents did not conceive but that
were equally supported by the evidence the proponents of the first theory had. We know this
because from our historical vantage point we can see that those rivals unconceived at the
time were later conceived.

The essential role of equal support claims in Stanford’s argument occurs in the step
where he characterizes those theories he finds in the past. The first theory, T₁, had a set of
supporting evidence, E, and unbeknownst to the proponents of the first theory there existed at least one theory, T₂, discovered later, that was equally supported by that earlier evidence E. This claim is essential to his case because if the earlier scientists’ evidence set did not equally support T₂, then Stanford would have given us no reason to think our evidence set equally supports an alternative theory we have not conceived. It would only indicate that there may always be evidence around the corner that will threaten our theory. But we knew that we never have complete evidence. This is the basic problem of induction, and the new pessimistic argument needs to be more than that if we are to take it seriously.

It should be obvious from the foregoing that there is a serious question what Stanford can mean by “equal support.” He speaks of threatening rivals as theories that enjoy the same empirical success, making adequate explanations and the same predictions of the empirical phenomena found in the earlier period as the theory of the earlier scientists does, and concludes without further ado that earlier and later theories are equally supported. If so, then he assumes that the rival theories T₁ and T₂ implying or probabilifying to the same degree all the statements within the evidence set E is sufficient to conclude that the theories are equally supported by the evidence set E. This is the assumption EEEI.

Stanford needs EEEI in order to make the claim that our predecessors’ evidence often supported conceivable theories they had not dreamt of as much as it did their own theories, so he will be restricted to the Ratio measure (or a purely syntactical account). And in needing equal support claims at all, he faces the same dilemma van Fraassen did. If we can evaluate equal support claims, which involve reference to unobservables, then we have no reason to think we cannot also incrementally confirm theories via empirical evidence. (That theories make claims about unobservables cannot be an objection.) If it is not possible or allowable to evaluate support claims for theories, then it is not possible to make responsible equal support claims about, in particular, theories of the past, and Stanford has not made an argument at all. He cannot claim confirmation of empirical hypotheses over all observables either – all support claims refer to unobservables – and his pessimistic view reduces to skepticism. The other alternative is for Stanford to give us a new account of what evidential support is that preserves EEEI, (avoids syntacticism), allows for claims about support without appealing to claims about unobservables, and avoids skepticism. What will not be sufficient is to base a claim that two theories are equally supported on intuitive judgments, given the now obvious role of general assumptions about the nature of evidential

---

10 One might think to escape my arguments by claiming that the later theory is typically and recurrently more confirmed by that very same earlier evidence set than the earlier theory was. It would be for Stanford to show that. However, even if he made it intuitively plausible, intuitions don’t settle things at the stage we have reached. The difficulty with new evidence and new auxiliaries that I discuss below makes it very hard to see how these comparisons of more and less over the same evidence set can even be made in the case of theory and evidence development over time, much less made without appeal to assumptions about unobservables.

11 One might notice that we are here talking about a finite evidence set rather than the set of all possible claims about observables. This is also an instance of EEEI, just one where the theories have the same consequences over a smaller empirical evidence set. Nothing has been assumed in my arguments about how small or large the set is and possibility versus actuality has not played an essential role.
support in disputes between realists and anti-realists. Stanford claims that his argument is confirmation-theory neutral. (2006, 21) The road to hell is paved with good intentions.

In response to questions about auxiliary hypotheses, Stanford makes a move that may seem to insulate his view from the problems just explained. Acknowledging that auxiliary assumptions are claims which themselves often involve unobservables, and that new auxiliaries are often needed in order to see the later theory as equally confirmed by the earlier evidence, he says that the claims he means to say are equally supported by the earlier set of evidence are the first theory combined with its auxiliary assumptions, and the second theory combined with its (often previously unconceived) auxiliary assumptions. (Stanford 2006, 20) Wherever claims about unobservables are found in the commitments of the later scientists, they were also unconceived by the earlier scientists, obviously conceivable, and, taken together with the later theory, equally supported by the set of earlier evidence.

Sly boots, for it seems that he has divided the set of statements of the language into a set E of evidence, a set T_1 of theoretical statements, a set T_2 of theoretical statements, and the remainder a set A_1 of auxiliaries, and a set A_2 of auxiliaries. This appears to enable him to claim that E equally confirms T_1.A_1 and T_2.A_2 (conjunctions of theories and their auxiliaries) via the Ratio measure without making a claim about unobservable entities, thus:

\[
P(T_1.A_1/E)/P(T_1.A_1) = P(T_2.A_2/E)/P(T_2.A_2)
\]

For consider where the background assumptions about unobservables occur whose values figured above in determining whether evidence makes a difference to hypothesis, here whether E makes a difference to the probability of T_1.A_1? All relevant claims about unobservables occur in either theories or auxiliaries, and those have now been incorporated into that which is claimed to be confirmed. This apparently avoids having claims about unobservables be part of the content of the claim of equal support.

However, this move has made the notion of evidential support that Stanford must use a syntactic relation. This is because now that all auxiliaries are part of that which is to be confirmed (or not), there are no substantive relevant claims involved in linking the contents of the confirmed and the confirming, T_1.A_1 and E respectively. Thus, the only connection that could determine whether E made a difference to the probability of T_1.A_1 would be a purely syntactical relationship between T_1.A_1 and E. (E.g., instance and generalization relations, and other relations of logical form.) The problem with this, as noted above, is that everything we have learned in the past 50 years tells us that evidential support is not a syntactic relation. If this is the kind of support Stanford means when he claims two theories have equal support, the stick he is shaking at us is a thin reed. Stanford himself insists that evidential support is not a syntactic relation, and that his argument does not depend on an atavistic view of confirmation. But insisting does not make it so.

That is enough to end the story, but another problem with Stanford’s pessimistic induction illustrates how complicated comparative evidential support really is, and how well
probabilistic analysis respects that if we use it properly. A further problem for Stanford’s argument arises from the fact that the division of the statements of a language among the sets $T_1$, $A_1$, $T_2$, $A_2$, and $E$, cannot be exhaustive of the relevant statements of the language. Set aside the fact that there are other theories than $T_1$ and $T_2$. What concerns me now is that the language in which we compare $T_1.A_1$ to $T_2.A_2$ must contain other evidence statements than those in $E$, which was the set of evidence possessed by the earlier scientists. Those defending the later view typically have more evidence than their predecessors, and some of that evidence shows, or appears to show, the new theory superior because that theory explains or predicts more. Also typically, although some theories and auxiliaries often go unconsidered, there are at least some of those additional evidence statements of the later scientists that are part of the language of the earlier scientists and about which the earlier scientists made assumptions. One reason people can be surprised at the outcomes of later experiments, even when the experiment itself had not been conceived in the earlier time, is that prior to the experiment people had made different assumptions about what would result, that they may not even have known they had. Light, like any other wave, should have shown interference fringes when two sources from different reference frames intersected. No one needed actually to have conceived the Michelson-Morley experiment for that to have been the most reasonable assumption about how light behaves in the situation described. It was strongly suggested by all the evidence 19th century physicists had that said light is a wave, and the evidence that other wave phenomena – sound, water waves – had such properties.

Intuitively it seems fair to say that the pre-Michelson Morley evidence confirms the conjunction of the Special Theory of Relativity, $T_2$, with auxiliaries, $A_2$, that included the claim that there is no ether, as much as it does Classical Electromagnetic Theory, $T_1$, with auxiliaries, $A_1$, that included the claim that there is an ether. After all, Special Relativity combined with denial of the ether and with other auxiliaries predicts the earlier evidence roughly equally as well as the old theory plus auxiliaries did, right? But intuitive as this is, it is not so. The new theory plus auxiliaries only so predict or probabilify the earlier evidence set if the new non-interference evidence is taken into account.

To see this, let not-$E'$ be the evidence, from the Michelson-Morley type experiments, that consisted of observed non-interference when two light beams from sources with different velocities intersected. Not-$E'$ is part of the later evidence that supports $T_2.A_2$ over $T_1.A_1$, and the supporters of $T_1.A_1$ assumed $E'$; otherwise people would not have been surprised at the later evidence. The earlier scientists assumed $E'$, the later saw not-$E'$. Let us chase down where $E'$ and not-$E'$ could have been in a claim that $E$, the earlier evidence, equally supports $T_1.A_1$ and $T_2.A_2$. Where should we imagine $E'$ to sit in this evaluation? It is among the beliefs of the earlier scientists but is $E'$ part of the earlier evidence? It is not fair to assume so since though the earlier people made assumptions about it they did not have the

---

12 These theories present a problem too, for the same reason developed in this paragraph for additional evidence statements.
observations. Let us consider the possibility, though, for my opponent’s sake. If E’ were part of E, then A2.T2 could not plausibly be as supported by E as A1.T1 was, because T2.A2 includes a rather consequential assumption – there is no ether -- that is highly improbable by the lights of E (in virtue of E’s including E’), given assumptions that both earlier and later scientists made. If there are interference fringes, then there is no reason to deny that there is a medium for the waves.

The only other alternative that will put E’ among the earlier scientists’ beliefs is to call it part of the set represented by A1, the earlier auxiliary assumptions, which seems the more reasonable option. Where is not-E’ in the evaluation, the evidence that only the later scientists have? This is not part of the evidence we measure the later theory against in the equal support comparison in question because that comparison is done over the earlier evidence. Not-E’ cannot be part of the auxiliary set A2 either, since it is a claim about observables, it is discovered evidence, and it is not an assumption to the later scientists. So, since not-E’ cannot have yet entered either in the condition or in the conditioned of the term “P(T2.A2/E)” it must be that E’, not not-E’, is being assumed among the members of A2. However, that automatically makes A2.T2 less supported by the evidence E than A1.T1 was. In light of other assumptions that both the earlier and later scientists made, the conjunction in A2 of the claim that there is no ether with E’ has an extremely low probability. Thus, P(A2.T2/E) is very low in the comparison, since E’ is part of the background assumptions. The earlier and later scientists would agree that if you have the relevant interference, E’, then you have no reason to deny that there is a medium, the ether, and still all the other reasons to believe it (the evidence about other types of waves). The later theory plus auxiliary is not equally confirmed by the earlier evidence unless we take the later evidence into account.

Someone claiming equal support here has nothing sound he can do with E’, the earlier assumption, and not-E’, the later, new evidence. Moreover, this case does not involve odd circumstances. The situation is one where in order for the newer theory-plus-auxiliaries set to save the phenomena even as adequately as the old combination T.A did, we need to take into account the new evidence that disconfirms a background assumption of the earlier scientists that is broadly incompatible with a needed new auxiliary assumption. There are examples of this phenomenon in the cases Stanford considers. Darwin’s assumption that variation is an anomaly whereas heredity is the rule does not sit comfortably with Mendelian genetics, where getting aa from two Aa parents happens by the same rule as getting Aa from them. It is irrelevant whether this assumption was or was not the reason Darwin failed to conceive of a common cause view of heredity. Grant that it was not. What matters is that the assumption occupies the position of the background assumption E’ in the problem just discussed. The earlier (Darwin-period) evidence cannot equally support Darwin’s view and the new Mendelian view, because in that comparison E’ must be in the background assumptions of both support claims -- evidence unseating E’ only comes later -- and E’ is not compatible with Mendel’s genetics. Whatever evidence there came to be against E’ that
allows Mendelian genetics to probabilify Darwin’s evidence is not part of the evidence over which we are being asked to compare the support of the two theories, when the claim is made that the earlier evidence equally supports the earlier and later theory-plus-auxiliary combinations.

Finally, to comment on Stanford’s criticism of my remarks about the Atomic hypothesis, it should be obvious that nothing in my position so far depends on my comments about this hypothesis. And beyond exposing anti-realist views as roughly speaking incoherent, my realist tendencies do not involve general claims that we have actually confirmed our successful theories, so my task in evaluating when we have and have not will end up being independent, and going case-by-case. I do judge, independently, that we have reached at least a little beyond an observable-unobservable line (assuming there is one), but that too has no need for claims about atoms, since it is shown by pregnancy tests and tests for medical conditions.

I also think, independently, that we do have significant evidence for the modest atomic hypothesis. One of the reasons I chose this example is that it illustrates clear improvements over time in our methods for ruling out alternatives. The methods Perrin used show, for example, the ability to rule out alternative hypotheses in groups without conceiving of the possibilities individually. This was a giant advance over his predecessors who could only rule out possible explanations of the Brownian motion individually and seriatum. Such methodological advances are a phenomenon to set against pessimistic inductions, and since 77 I have come to think this point is powerful for blocking all such inductions, which are arguments requiring more significant assumptions than most have recognized.13 I do not suppose or need to suppose that our methods have improved, or will improve, all the way to letting us rule out all possible alternatives, a claim that Stanford repeatedly says a realist must defend. For example:

We would seem to have little reason to believe that we are presently at the end of an exhaustive search of it [the space of alternative possibilities] and have finally reached the point at which serious unconceived possibilities no longer pose any real danger to our theoretical science in a given domain.” (Stanford 2006, 133. See also 131.)

He assumes that we would need to be at such an end in order for our evidence to give us a right to believe our theories. This is a puzzling and undefended assumption that appears to presuppose an infallibilist epistemology.

It is easy to show we will not rule out all possible alternatives, since we will never have all possible evidence. But if that is why we are supposed not to have a right to be confident in our theories, then the issue is one of basic inductive skepticism, and if so then

13 See my “Optimism about the Pessimistic Induction,” 2009.
we could have described the problem without any discussion of the history of science. All that a reasonable realism needs is the failure of pessimistic arguments that attempt to undermine, usually in some general way, our right to believe, to whatever degree and in whatever way is appropriate (e.g. perhaps structures rather than entities), the theories our current evidence supports. Being justified in believing p does not imply p.

It would be silly to deny that there are alternative theories that we have not conceived of. I haven’t done so or needed to. I claim that the anti-realist has not given grounds to object to confidence in the modest atomic hypothesis that is informed by the evidence. I have taken the stand that Perrin’s evidence lent significant confirmation to the atomic hypothesis, a claim that Arthur Fine seems to accept. (1991, 88) By this I do not of course imply that one is rationally obligated to believe the hypothesis. (Thus I do not employ Miller’s schema about truisms.) It is not irrational to disagree with the conservative assumptions I made, or to suspend belief. Besides, the number I come up with as the probability of the atomic hypothesis after Perrin’s evidence is only at least .6, which far from obligating full belief does not even warrant it. Stanford seems to have missed the fact that in that calculation I do not assume there are no alternative explanations of the Brownian motion than Perrin’s. In the numbers I assume only that the probability that some theory incompatible with the modest atomic hypothesis probabilifies Perrin’s evidence is below .5. (That is, that the probability of the evidence given the negation of the hypothesis, P(e/-h), the catch-all term, is below .5.) Thereby I allow for as many as you like plausible alternative hypotheses incompatible with the modest atomic hypothesis and for their being positively relevant, even highly so, to the Perrin evidence. All such an alternative needs to be allotted is a non-zero probability in order for it to be susceptible to later evidence increasing its probability as high as you like (not 1), and I have allowed it to be much, much higher than non-zero.

Stanford apparently does not think these accommodations of our epistemic imperfections and incompleteness are enough. He points out that there were actual alternative explanations in Perrin’s time that were not ruled out by Perrin’s evidence. But of course, the existence of alternative ideas of the submicroscopic world cannot threaten the support Perrin’s evidence lends to the modest atomic hypothesis unless we are also being offered an equal support claim. That is, Stanford must be claiming that the reason we should not take Perrin’s evidence as seriously supporting the modest atomic hypothesis is that his evidence equally supports a number of other real alternative views. Stanford’s words confirm this:

That is, the recognition of serious scientific alternatives to the atomic hypothesis that also confer a high likelihood on Perrin’s evidence threaten to turn the razor-
thin gap Roush recognizes… into a yawning chasm instead. (page 7, my emphasis)

But equal support claims cannot be made breezily anymore, as we have seen. The problem with that gambit here is that no one has given any reason to believe that these alternative ideas of electrostatic forces and brute randomness probabilify Perrin’s evidence, or even that they could be made to. Since Stanford’s *general* argument that our predecessors’ theories stood in equal support relations to theories they were unaware of didn’t work, he will have to convince us of the equal support claim in these particular cases.

That there is no reason to believe this becomes clear if we reflect on the fact that Perrin’s evidence is detailed, quantitative, and precise. The brute randomness idea (with no underlying entities) was not equally supported by Perrin’s evidence, because this bare claim has no resources to imply or even probabilify the kind of very specific evidence Perrin had. Similarly for the broad idea that the random walk might be due to electrostatic forces. This is plausible as a starting point, but what is it about that idea that should lead us to expect Avogadro’s number exactly? The fact that these were serious ideas is accommodated in my assuming no more than that the atomic hypothesis was minimally more able to probabilify Perrin’s evidence than its negation was. We have thus been given no reason to suppose what Stanford admits he needs: that these alternative views “also confer a high likelihood on Perrin’s evidence.” Those ideas were not, and as far as I know have not been, developed clearly enough to do that. I don’t know that they cannot be, but I don’t need to show that.

I need not go further here to defend my level of confidence in Perrin’s hypothesis, but the case does illustrate well the points above about auxiliary hypotheses. The modest atomic hypothesis has a potent advantage over the vague alternative hypotheses we know existed because it was essential in defining the statistical models of possible types of causation of the Brownian motion, the crucial new form of inference that Perrin introduced that was used to produce and render significant the data that confirmed the randomness of the motion. (Note that this does not mean he assumed the truth of the atomic hypothesis in order to carry out his experiment.) Thus in virtue of Perrin’s work it is clear in a very detailed way *how* the atomic hypothesis of entities moving at random would explain the data that confirmed random motion. No one has shown how the other vague possible hypotheses could play that role, or even an analogous role, in producing the outcomes of those experiments (which data then strongly supported randomness), so they have not been shown to explain those data. There is also no reason to expect that they could play the role that the modest atomic hypothesis did, for the way the models work, and work with experimental design, is specific and depends on a host of particular auxiliaries. Typically, as argued in the paper, and explained above, auxiliaries will *have* to be different for the alternative

---

14 The razor-thin attribution is a misreading. Though Stanford thinks I can’t possibly have meant it, I used the term “close” to compare the content of two hypotheses, which is why the word “close” qualified the word “equivalence.” My claim may of course be wrong, but I think at this point that is no longer relevant.
hypothesis to be supported or related to the evidence. So, we can’t just casually claim that
the randomness result from Perrin’s experiment equal supports or is equally explained by
other hypotheses without saying how, again because of how evidence and explanation
actually work, both formally and historically.

One might try protesting that it is enough to shatter our right to believe if these
alternatives could possibly explain or probabilify Perrin’s detailed evidence. But it is not,
because the real possibility of an alternative explanation of the evidence is well
accommodated by assigning the catch-all term any non-zero number, even epsilon, and I
allow it to be up to .5. So, as far as anything our anti-realist has shown, and assuming no
more than that Perrin’s evidence claims have a .6 probability of being true, Perrin’s evidence
ends up giving the right to a 60% degree of belief in the modest atomic hypothesis.\textsuperscript{15}

Ludicrous, one might protest, to think that you have any right to assign any number
at all to the probability of the evidence on the catch-all. Let us run with this idea. It could be
thought to be ludicrous because we have not conceived of everything in that possibility
space of alternatives. But the existence of Perrin’s methods shows that conception and
conceivability are not per se relevant to whether we have ruled alternatives out. It could be
thought crazy to assign numbers because this commits us to claims about unobservables. But
we have seen that even on the measure most favorable to the anti-realist, if we have no right
to make commitments about unobservable matters then we cannot make commitments to
equal support claims, or even commitments that observations support claims about
observables. My presuming to assign probabilities to these assumptions might also be
thought outrageous because we never have enough evidence to know about everything that
is in that possibility space. But we never have enough evidence to exhaust the possibility
that we are wrong, even about whether there are tables in front of us; requiring that in order
for our epistemic claims to be successful assumes infallibilism, and leads to skepticism.

It could be objected, and often has been, that what is unjustified is the assignment of
exact numbers. It’s not the way scientists actually think about evidence and we don’t have
the information for that. But what is important in these assignments is not specific numbers
or scientists entertaining thoughts about them, but ordinal relationships – where some
probability and evidence claims stand relative to others, in fact or in the rational confidence
of scientists – and the numbers are simply tags for keeping track of that. Does Stanford think
there is no fact of the matter about whether the evidence he has marshaled supports his
views more than it does mine? If that is what he thinks, then I am not sure what he is doing.
Precision is also quite irrelevant to the issues here, since the effects of auxiliary assumptions
on evidential support claims can be as dramatic as you like, flipping a claim that one theory
is much more supported than another to a very strong claim in favor of the other theory. The
denial that equal support claims can be made out is not a denial of their precision.

\textsuperscript{15} I employ here a non-standard way of using the information, and Bayes’ Theorem, to calculate a posterior
probability. This “Leverage Method” is discussed in Chapter 5 of \textit{TT}, but the argument can be made with the
usual way of applying Bayes’ Theorem.
In writing his book, and introducing his strategy, Stanford thereby concedes that it is for the anti-realist to make a case here. We have evidence for our theories and it is for him to tell us why we shouldn’t proportion our beliefs to it as best we can.\textsuperscript{16} Stanford’s particular argument for doubt, which is the same in his criticism of my remarks about Atomic Theory as in his overall pessimistic induction, depends on a barrage of equal support claims which have not been adequately defended, and look to be indefensible except via an unacceptable syntactic notion of evidential support. Evidential support claims, and in particular equal support claims, are about unobservables as well as observables, and you can’t step in the same evidence twice. Skepticism makes us feel smart, and maybe even virtuous, but rationality is not on its side.

References


\textsuperscript{16} Besides, due to the role of auxiliaries, the pessimist can only make a dialectical argument that starts with the realist’s confidence in the truth of current theories, for only on such an assumption can we be confident that our predecessors were actually wrong.