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The Empirical Underdetermination of Theories and Scientific Realism

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Mario Alai

DiSPeA - Università degli Studi di Urbino Carlo Bo

OUTLINE

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1. The Empirical Underdetermination argument against realism

There is a powerful antirealistic argument from the so called "empirical underdetermination of theories", namely:

(i) from a logical point of view for any body of empirical data, however wide, it is possible to find infinite incompatible theories entailing them;
hence
(ii) at any time there are infinite possible theories having the same class of empirical consequences;
but
(iii) the only evidence for a theory consists in its empirical consequences;
hence
(iv) all those theories enjoy the same confirmation;
therefore,
(v) we cannot have reasons to believe that one of them is true (if any) to the exclusion of the others.

In fact, given the infinity of potential empirically equivalent but contradictory competitors,
any one has $1/\infty$ chances of actually being true, i.e., almost 0 probability.

This is a key argument for antirealists, based on a strong empiricist standpoint: (slack between theory and observational data)
it was employed by many antirealists at all times, among whom
the Hellenistic "empirical" physicians (Celsus 1915: §§ 28-29; Alai 2008: § 3),
some ancient and medieval astronomers (Alai 2008: § 4),
and the various proponents of an instrumentalistic interpretation of Copernicanism.

Realists have replied that "It has never been shown that for any theory there exist *non trivial and minimally plausible* alternatives".[1]
In fact, in the history of science, cases of competition of two empirically equivalent theories have been relatively few. E.g.,
- undulatory vs. corpuscular theory of light;
- standard quantum mechanics vs. Bohmian mechanics;
- non-Euclidean geometry plus ordinary physical laws vs. Euclidean geometry plus non standard physical laws in Poincaré's thought experiment.

The cases of three equivalent competitors are even fewer
(e.g., Ptolemaic vs. Copernican vs. Tychonian cosmology),
and I know of no cases of more than three competitors.

Some authors refused to take seriously the claim that theories are in principle underdetermined by empirical evidence, and argued that
"until we are able to actually construct an empirically equivalent alternative to a given theory,
the bare possibility that such equivalents exist is insufficient to justify suspending belief in the best theories we do have"
(Stanford 2009); see Kitcher (1993, 154); Leplin (1997), Achinstein (2002)).
However, the antirealist argument is an in-principle argument,
so also the realist response should be an in-principle one.

It is a fact that this in-principle problem does not appear to trouble scientist in practice,
but realist should be able to explain why
(given the logical truth that any body of data is always compatible with infinite theories).

[1] Psillos 1999, p. 168. Here of course the stress in on 'non trivial and minimally plausible'.

2. Confirmation vs. entailment

The most convincing and widely accepted explanation why underdetermination is not a live issue for scientists
is that in practice they discount premise (iii) of the above skeptical argument:

(iii) the only evidence for a theory consists in its empirical consequences.

Against this premise it can be pointed out that evidence is not the same as the class of empirical consequences, because
on the one hand, (1) some empirical consequences of a theory do not confirm it, and
on the other hand (2) some of the empirical instances confirming a theory are not
entailed by it:[2]

2.1 Not all empirical consequences confirm

(1) As shown by Hempel's ravens paradox, not all empirical consequences confirm:
the fact that this white powder is sodium chloride
is a logical consequence of the generalization that all ravens are black
(i.e., that nothing which isn't black is a raven), but it does not confirm it.

So, a given datum (e.g., that this white powder is sodium chloride)
may be consequence of two different theories, say,
- a certain chemical theory
- and the claim that all ravens are black,
yet it may confirm the former but not the latter.

- Equally, a theory that has been modified *ad hoc* to accommodate a certain evidence
entails that evidence, but it is not confirmed by it.
For instance, suppose I hold that all planetary orbits are circles, and then,
upon observing that Mars' orbit is elliptical,
I adapt my theory, holding that <u>all the planets except Mars</u> have circular orbits:
the fact that Mars' orbit is not circular, though a consequence of my new belief,
does not confirm it.
This shows that confirmation is not a merely logical relation, but an <u>epistemi</u>c relation: predictive power (i.e., the chronological or at least functional primacy of the theory with respect to the evidence) is crucial to confirmation, and it is not a logical relation.

2.2 Not all confirming instances are consequences

(2) Confirmation is not the same as entailment also because
hypotheses can be confirmed by instances which are not their consequences:
for instance, as noticed by Psillos,
suppose that datum ***d*** confirms theory $\mathbf{T_1}$, and $\mathbf{T_1}$ supports theory $\mathbf{T_2}$.
Thus datum ***d*** indirectly confirms theory $\mathbf{T_2}$, even if it is not its consequence.[3]
For example, data about the behaviour of uranium in the laboratory

[2] See Laudan 1996, ch. 3, Laudan and Leplin 1991, Laudan and Leplin 1993. See also Psillos 1999, 169-176
[3] Psillos 1999, p. 169-173.

can be a consequence of our theory of atomic decay, and confirm it.
In turn this theory, allowing us to date fossils by carbon 14,
can confirm a particular geological theory **GT1.**
Therefore, laboratory data about uranium indirectly support our geological theory **GT1**,
although they do not in any way follow from it.

Thus, there might be a different geological theory **GT2**,
such that neither **GT2** nor **GT1** entail our laboratory data on uranium,
yet **GT1** is better confirmed than **GT2**.
Hence, empirically equivalent theories may be differently confirmed.

It might be replied that just as, by the principle of underdetermination,
for any set of data entailed by a theory, with enough ingenuity one could find another theory entailing the same data,
so also for any set of data confirming T_1, with enough ingenuity one could find a theory confirmed by them.
But this is precisely what nobody has shown so far,
since entailment is a straightforward logical matter,
and building a ***d***-entailing theory is a mechanical process,
while confirmation is a much more complex matter.

3. Confirmation beyond empirical consequences

3.1 Non observational evidence

Once we appreciate that confirmation is not the same as empirical consequences,
the question becomes:
where does the confirming evidence exceeding empirical consequences come from?
And what kind of evidence is missing when empirical consequences do not confirm?

It has rightly been pointed out that
(a) anything we know can be used as evidence for an hypothesis, not just empirical data (T. Williamson 1997, Alexander Bird 2016), and
(b) in fact scientists often use non observational facts as evidence for their hypotheses (Bird 2018).
Yet, while these claims may be true,
they risk to be question-begging as explanations of how we overcome underdetermination, because
-(a) since we are fallible, how can we distinguish whether we actually know that X,
hence we can use X as evidence,
or we simply *think* we know that X?
Or how can we distinguish whether something is a fact, or we only think it is a fact?
Moreover,
-(b) using non observational facts as evidence presupposes scientific realism
(i.e. the beliefs unobservable facts can be justified), hence it cannot support it.

3.2 Theoretical virtues and ampliative methods

We get closer to the heart of the matter
by pointing out that scientists are not blocked by underdetermination because they don't look just for theories that "save the phenomena".
Rather, they seek theories that, in addition,

- ***explain*** the phenomena
- **unify** them into a systematic account, and the more phenomena they account for (i.e. the **stronger** they are) the better;
- are **fecund**, i.e. they do not simply account for known phenomena, but make new testable predictions, so enlarging the body of known data;[4]
- are **plausible**, because they employ mechanisms which are already known to work in other contexts;
- are **consistent** with the rest of accepted theories and beliefs;
- are **consilient** with them, and support and/or are supported by them.
- are *ceteris paribu*s s**imple**, offering a **unified** account, without ***ad hoc*** clauses - and in any other possible sense.

So, although scientists *could* imagine many alternative theories compatible with one and the same body of data, they just don't consider them unless they have these theoretical virtues, and it has not been shown that in most cases more than one or few theories fulfill these requirements. Moreover, in most cases just one theory (and exceptionally only 2 or 3) fulfill these requirements.

Just in the same way, although logically speaking any set of points in a Cartesian plane is compatible with infinite curves,
in guessing the next points, one always chooses the smoothest and simplest possible curve.

In Bayesian terms, different hypotheses have different prior probabilities,
so even if confirmed by the same evidences, they get different posterior probabilities.[5]

Thus, for right or for wrong,
scientists don't buy premise (iii) of the Empirical Underdetermination argument,
(that the only evidence for a theory consists in its empirical consequences).

Compliance with known data is certainly good evidence for the empirical adequacy of theories, but the very principle of empirical underdetermination shows it does not supply sufficient evidence for their *truth.*

So, the scarcity of competitor theories actually considered by scientists shows that they don't look just for theories which "save the phenomena", but for *tru*e theories.

Moreover, they use the abovementioned theoretical virtues
(explanatory and unificatory power, fecundity, plausibility, simplicity, etc.) as criteria of theory choice because they believe that those virtues are good evidence for truth;

Besides, they believe this because they assume that nature is simple, rationally explainable, and that our ampliative methods, plausibility standards, background theories and beliefs are by and large correct.

Further, the stunning predictive and explanatory success of science shows that these assumptions are largely correct.

In a nutshell, *theoretical virtues* have confirmatory power.[6]

In fact, they are the only possible basis for all our ampliative inferences: even the mere inductive projection of a property from the particular case to the general one presupposes the

[4] As shown in the literature, a phenomenon counts as novel even if it was known beforehand, but the theorist was able to account for it without using it in building the theory (Alai 2014b).
[5] Fano 2005, p.166; Psillos 1999, p. 163.
[6] Glymour (1980), Kosso (1992), Psillos (1999), 171-176.

uniformity of nature, and follows the rule of simplicity. The same holds for analogical reasoning, while abductive reasoning exploits the explanatory power of hypotheses.

If theorists just looked for theories that are empirically adequate, or "save the phenomena", as suggested by antirealists, they wouldn't care for theoretical virtues, and entertain with equal seriousness all kinds of weird theories, as long as they are compatible with the available data.

It might be replied that empirical adequacy (i.e. consistency with all possible observations) is not a property which can be directly recognized at any particular time; so perhaps scientists seek theoretical virtues because they take them to be evidence of empirical adequacy. However, while it is clear which link there is between virtues and truth for realists, which relation there might be between virtues and empirical adequacy is not clear at all. For realists, if nature is simple, simple theories are true; if accepted theories are mostly true, coherence with them indicates truth; if phenomena are explainable, then no theory can be true unless it explains them; etc. But how about empirical adequacy? Of course, if we have evidence that T is true, we also have evidence that it is empirically adequate. On the opposite the only evidence that a theory is empirically adequate even if not true is its mere consistency with past observations, but of course many non empirically equivalent theories can be compatible with past observations (Alai 2014a, §6).

Scientific realists simply agree with scientists,
both on the idea that we should look for truth,
and on what are reliable indicators of truth.

On the other hand, the scientists' behavior should appear irrational and unexplainable to the antirealists who hold premise (iii):
from their point of view, at any time scientists should entertain with equal seriousness all kind of weird theories, as long as they are compatible with the available data.
No wonder therefore that the few historical cases of underdetermination have been solved in due time.

4. Underdetermination *even* by theoretical virtues

Antirealists objected that in certain cases not even theoretical virtues could discriminate between competing theories. For instance, they couldn't discriminate between:

4.1 Trivial permutations

- a theory $\mathbf{T_1}$ and a theory $\mathbf{T_2}$ cooked up by commuting everywhere two terms of $\mathbf{T_1}$.
But obviously this would be just the same theory, expressed in two different languages.[7]

4.2 Instrumentalist reductions

Also, Kukla held that any theory **T** will be equally supported as
- **T'**: the claim that **T** has true observable consequences, but it is false;
and **T** will be equally supported as
- **T"** : the claim that the world behaves in the way **B** described by **T** when observed and in a different way **B'** when not observed;
- etc. (Kukla 1996)
Other similar alternatives are mentioned by Fine, van Fraassen, Lyons, Stanford, etc. (Alai 2014a, § 2).

[7] Stanford 2009, foot note 9, lists a number of works discussing the question of when, and to what extent, empirically equivalent theories can be considered *just* different formulations of one and the same theory: Glymour (1970, 1977, 1980, 2013) Sklar (1982), while more recent work includes Halvorson (2012, 2013).

However, first, as noticed by Stanford (2001), this would reduce the specific problem of scientific underdetermination to the metaphysical skepticism of Cartesian Evil Demon.

Secondly, and more importantly, these alternatives are much worse than their standard counterparts from the point of view of theoretical virtues.
For instance,
- while **T** explains its empirical consequences, **T'** (= the theory that T' is empirically right but false) does not.
- **T"** is more complex and less confirmed than **T**, because it postulates two different behaviors of the World (**B** and **B'**), of which only **B** is supported by our evidence.
- etc.
- etc.

4.3 Mathematically intertranslatable theories

Philosophers have suggested various examples of empirically equivalent theories (exceeding the three abovementioned ones) which apparently cannot be decided even by theoretical virtues, because they are mathematically intertranslatable.
Such examples include:

1. Newtonian mechanics (based on force) vs. Lagrangian or Hamiltonian mechanics (based on a principle of minimal action);[8]

2. theories introducing fields vs. theories using action at distance with retarded potentials;[9]

3. Heisenberg's matrix mechanics vs. Schrödinger's wave mechanics;[10]

4. Newton's cosmology, assuming that the entire universe is at rest,
vs. a theory with the same laws of motion and gravitational attraction,
but assuming that the universe is moving with some constant velocity in some given direction (van Fraassen 1980);

5. Newtonian mechanics with its gravitational field vs. GTR's curvature of spacetime;[11]

6. different cosmological models of the GTR assuming different global topological features which are empirically undistinguishable inside the light cones of even idealized eternal observers.[12]

One might suggest that these couples of theories are just *different formulations of the same theory*,[13] hence they are not mutually incompatible, and can be contemporarily true.

But this answer is not easily available to scientific realists:
perhaps they can accept the suggestion that there is simply no fact of the matter whether the universe is at absolute rest or not, hence the alternatives of case **4.** are really the same theory;

but realists should hold that there is fact of the matter about whether quantum waves are real or not; whether there are fields and no action at a distance, or vice-versa;
whether spacetime is flat or curve; which are its topological features; etc.

However, realists can deal with these case by three (not mutually exclusive) strategies:

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[8] Putnam 1978a, p.153, 1981, pp. 81-82.

[9] Putnam 1978a, p.153, 1981, pp. 81-82.

[10] Putnam 1978b, p. 555; Friedman 1983, pp.165 ff.; Fano 2005, p. 166.

[11] Earman 1993. Instead, for John Norton (2008) this example simply involves two notational variants of a single theory (see footnote **14** above); see also Stanford 2009.

[12] Earman 1993; see also Stanford 2009.

[13] Again, see footnote **14** above)

(I) suggest that while these theories are mathematically intertranslatable, they make divergent ontological claims, which in the future may become empirically testable.

Because of changes in experimental methods, instruments, and auxiliary assumptions,
any two theories which are empirically equivalent at a given time may no longer be so at later times (Laudan and Leplin 1991, Stanford 2009).

In general, scientists may be unable to choose between two or more empirically equivalent rivals for a certain time, but obviously
realists are not committed to a realistic interpretation of theories in those cases.
Only when scientists have finally been able to chose one theory,
realists insist that we have compelling reasons to believe that it is at least partially true.

(II) A second possible realist strategy is insisting that in spite of the empirical *and* mathematical equivalence of certain theories,
we can choose between their different ontologies by theoretical virtues and plausibility criteria.
For instance, Hamiltonian mechanics has actually been preferred to Newtonian mechanics and matrix mechanics to wave mechanics.

(III) Finally, realists may simply grant that,
although there is a fact of the matter about those different ontologies,
we (or at least, the theories at hand) are simply unable to tell anything about them:

as suggested by Stanford (2001, 2009),
what such theories diverge about is a "surplus content",
something beyond their proper scope, which they have no warrant to assume,
and which we need not believe in order to take them seriously.

Realism is not belief in omniscience:
while antirealists sometimes declare meaningless what cannot be detected,
the core intuition of realism is that truth is independent of the subject,
to the point that it might (although it need not) be unknowable even in principle.

In particular, this third strategy is adopted by **structural realists**,
who believe that we should exclusively focus on the mathematical structures of reality,
as opposed to entities and ontologies,
because structures are all that we can possibly know (as held by epistemic structural realism) (Poincaré 1902, Worrall 1989, etc.), or even all that really exists in nature (ontic structural realism) (Ladyman & Ross, French, Ladyman 2011, etc.).

From this point of view, therefore,
when alternative theories attribute to their subject matter the same structure,
their difference may be discounted and no underdetermination arises (French 2011).

In general structural realism may not be a satisfactory solution to the problem of empirical underdetermination,
because one and the same body of data may be always be accounted for by theories attributing not only different ontologies, but also different structures (Holger Lyre (2011).[14]

But this difficulty does not arise for mathematically untertranslatable theories,
for [15] they describe the same structure.

[14] Lyre also thinks that actual of underdetermination are too few to support the general underdetermination thesis, but again, the question is why.
[15] in some important sense

Also, typical scientific realists may not be satisfied with structural realism,
because they are committed to a realist interpretation of entities and ontologies.
But again, it seems that in *this* particular case
realism may be better defended by humility than by presumption,
i.e. by conceding that the subject escapes our knowledge, at least for the time being.

4.4 Same content in different schemes

Again, philosophers as Goodman and Putnam remarked that theoretical virtues cannot discriminate between theories which are not just *mathematically* equivalent,
but have also an equivalent theoretical content,
since they are simply descriptions of the same systems couched in different conceptual schemes,[16]
or in different coordinate systems, or frames of reference.
For instance, this happens with theories describing spacetime events by different simultaneity concepts;
or with a theory assuming that the Earth is motionless and the Sun rotates around it,
one stating that the Sun is motionless and the Earth rotates around it
and another holding that both move in space, etc. (Goodman 1978, ch.VII).

But the impossibility of choosing in such cases is not a problem,
because these theories are mutually compatible, and can be true all at once:
hence, they just express the same content in a different form.[17]
Davidson attacked the scheme-content distinction,
but I have argued that his criticism fails, or at any rate it cannot prevent this solution to the problem of equivalent descriptions.[18]

5. Transient but recurrent underdetermination

Kyle Stanford (2001, 2006)
agrees with realists that no successful argument has been presented for the general underdetermination of theories by *all the possible* empirical evidence
(nor, *a fortiori*, for their underdetermination by empirical *and* theoretical evidence).

However, he argued that there is a form of underdetermination
that is more threatening and historically actual,[19]
namely underdetermination by currently available evidence.

It might seem that this underdetermination is not new and dramatic, since it is transient:
we all expect that future research will bring in new evidence,
by which we will be able to answer many of today's questions.

What makes this transient underdetermination troubling is its recurring character:
that is, we may assume that at any time
there are possible alternatives to the currently accepted theories whuch we cannot even imagine:
because of the limits of our imagination, mental capacities and current epistemic conditions,
numberless possible alternatives "exceed our grasps".

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[16] E.g., the same things can be said by talking of rabbits, or of rabbit-stages, of rain as an object or as a process, etc. (Quine 1968); by using as primitives both objects and mereological sums or just objects, lines and points or just lines (Putnam 1978b: 130-133; 1987: 32-33); etc. See Alai 1994, ch. 3.
[17] See also Alai 1994 : ch. 3.
[18] Alai, 1994, ch. IV.
[19] Sklar,1975, 1981.

Moreover, Stanford argues that these alternatives are underdetermined
both by all the currently available data and by theoretical virtues.

Granted, in due time some of these possible alternatives will be ruled out by newly emerging evidence, but many others will remain viable.

Stanford case's is made very convincing by his historic examples:

when Aristotelian mechanics was accepted, it was extremely well supported by empirical evidence and theoretical considerations;

but it was also just impossible to conceive something like Cartesian mechanics,

which centuries later became much better supported and superseded it.

Yet, in Descartes' time it was impossible even to imagine Newtonian mechanics,
and then Relativistic mechanics,
each of which in turn became much better supported of its predecessor…

and there are similar and equally well known examples in the life sciences, chemistry, etc.

Therefore, not only underdetermined alternatives are always there,
but apparently the true one is always among those overlooked by the scientists.

There is an important difference
between the classical underdetermination "by all possible evidence"
and this "transient but recurrent" underdetermination:
the **former** implies a radical scientific skepticism,
holding that even if we were at the ideal limit of inquiry, when all the possible data are in, we wouldn't know the truth.

Instead **the latter** implies the weaker skeptical claim that we won't know the truth
until we are at the ideal limit of inquiry.

However, since we are very far from—and we probably will never get at—the ideal limit of inquiry, this makes little difference.

It might seem that Stanford's transient underdetermination is not very different from the epistemological platitude that we cannot ever be certain of the truth of our theories,
because we don't know all the relevant data.
But,

if as usual we think of the currently accepted theory **T** as practically the only game in town, then our awareness that we don't know all the relevant data results in the lack of certainty that **T** is true;

Instead, if we think of **T** as just one among a host of unconceived and possibly better alternatives, as Stanford suggests,
the same awareness results in the almost certainty that **T** is false.

Thus, Stanford's underdetermination has the same consequences as
the infamous pessimistic historical meta-induction, which his examples immediately elicit:
just as all past theories turned out to be false,
so also current ones are probably false, and future ones will be false as well.

However, this conclusion can be resisted by two main strategies,
similar to those used against the pessimistic induction (see Alai 2017):

First, undeniably science progresses in a number of very concrete and measurable ways: available data increase,
instruments and methodologies are improved,

the quantity of researchers, publications and resources
grows all the time.
Moreover, this progress has become faster and faster in the last decades.[20]

It can hardly be disputed that all this allows us to conceive better and better theories and to rule out more and more wrong ones.
Therefore, the number of unconceived but epistemically reasonable theories[21] today is certainly smaller than in the past, and it will be even smaller in the future.

Hence the probability that our theories are radically wrong diminishes all the time. They might still be false, and we don't have an absolute measure of that probability, but one cannot simply and straightforwardly infer from the failures of the past to analogous failures of today or tomorrow.

Second, Stanford's pessimism (like the pessimistic induction) can be resisted by Kitcher's (1993) and Psillos' (1999) deployment realist strategy: many of the best theories at each time, although later recognized as false, included some true claims, which are preserved in successor theories: it must be so, otherwise the *novel* predictions issued by those theories would be a miracle (Alai 2014b).

Moreover, Cordero (2017a, 2017b), Peters (2014), Votsis (2011) and others have discussed criteria by which we can confidently identify some true components of false theories.

To be precise, I argued (Alai, 2017) that
we can confidently identify claims that are at least partially true,
and this of course is compatible with subsequently discovering that
such claims also include some false content,
so they are false *tout court* and call for replacement by more completely true claims.

Still, for these reasons, Stanford's underdetermination (just like the pessimistic induction) does not show that we are unable to achieve theoretical truth;
it simply shows that

- we haven't reached the whole truth, yet;
- possibly we won't reach it (not before the ideal limit of inquiry, at least);
- we don't know "how far" we are from it now (assuming talk of distance makes sense here);
- and quite possibly there will still be radical scientific changes in future science.

Antirealist objections to the "theoretical virtues" solution:

I have claimed that theoretical virtues can to a large extent solve the underdetermination problem. But antirealist might reply by a number of objections:

6. Holistic Extension of Underdetermination

6.1 Wrong background beliefs

Key theoretical virtues are compatibility and consilience of new hypotheses with accepted theories and beliefs;
but it may be objected that compatibility and consilience are evidence of truth

[20] Fahrbach; Cordero
[21] That is, if we consider not all the logically possible competitors, but those with any epistemic plausibility Otherwise, the number is always the same: infinite

only if we assume that accepted theories and beliefs *themselves* are *largely true*.
But what if they were largely false?
In fact, as far as we know, the entire system of all our beliefs might have many global alternatives, all compatible with the same data, and we'd never know which one is "true".

But first, as argued by Quine, Goodman, Davidson, Dennett, and Putnam,[22]
if an entire system of beliefs is coherent and empirically adequate,
it cannot be *completely and radically* false:
for unless there is a sufficient quantity of truths in it,
the system can make no sense at all, so it cannot even have any mistakes.

The difficulty is rather building such a coherent system.
But when we discard an otherwise empirically adequate theory
because it is inconsistent with other theories,
we are progressing toward that end.[23]

It might be countered that even if Quine-Davidson-Dennett-Putnam' s argument is correct, it only shows that the largest part of our beliefs must be true.
But that part might consist of empirical beliefs only,
so the theoretical beliefs might be largely false.

Still, the consistency with other theories entails at least the consistency with a much larger body of data (in fact, in the long run, with *all* the data),
and this excludes a much larger number of possible competitors.

Moreover, when scientists employ the criterion of coherence,
they have predictive success!
The same happens when they employ all the other ampliative plausibility criteria
(like explanatory and unifying power, simplicity etc.).

This shows that all these criteria are reliable indicators of truth,
so suggesting that our background theoretical beliefs,
which have been reached through those criteria, in fact are largely true;

therefore, coherence and consilience of an hypotheses with accepted theoretical beliefs is an indicator of truth-conducive.

6.2 Alternative ampliative principles

Another possible objection, in the footsteps of Quine, is that our own ampliative principles, which encompass what I have called 'Theoretical Virtues', may be questioned:
for instance, Quine would suggest that any of them may be questioned in order to defend an hypothesis in the face of recalcitrant evidence;
therefore, theories which are ruled out by our ampliative principles
might become quite acceptable competitors when coupled with different principles (Sanford, 2009)

However, we have no reasons to assume that there might be different ampliative principles which are better, i.e., more truth-conducive than ours.
In fact, we have reaons to assume that there cannot be better principles (e.g., Reichenbach on straight induction)

Besides, even from a Quinean point of view,
it would be practically never be wise to dump some of these *very* general and *wide scope* principles, just to save any particular hypothesis from contradictory evidence.

[22] See . Stich, 1991.
[23] See also Dorato 2007, p.196.

7. Humean scepticism: what if nature is not simple, consilient and rational and our ampliative methods fail?

It might be objected that theoretical virtues can be taken to confirm theories
only if nature is simple, consilient, and rationally explainable;
but nature might not be like this!

This is just another way to put Hume's point that we don't have a non-circular proof of validity for our ampliative inferential methods.

However, as noticed by Peirce, the only chance we have of knowing anything beyond what is presently observed, is that our ampliative inferential patterns work;
so, if we are seeking knowledge, we have no choice but trusting them.[24]
In fact, usually empiricists accept ampliative inferences from what is observed to what is not observed, as long as it is observable.
But as I have argued elsewhere (Alai 2010), while the difference between being observed and not observed has an epistemic relevance,
the difference between being observable and not observable has at most a pragmatic relevance.
In other words, inferential reliability is a logical character, not depending on the subject matter;
therefore it is not affected by the different properties which make some entities capable of being observed by human senses and other not capable of being so observed.
Hence, ampliative inference patterns should be considered as reliable even when applied to entities not observable by human senses.[25]

For instance, suppose we have seen that an asteroid produces on the ground a hole large 20 times its diameter.
So, if astronauts land on a distant planet and find a hole of 20 meters diameter,
they may abductively infer that it had been caused by an asteroid of 1 m.
If they find a hole of 1 m. diameter, they may infer that it had been caused by an asteroid of 5 cm., and so on.
But if they find a hole large 0,2 mm (which is just barely observable),
they may infer that it has been caused by a asteroid of 0,01 mm. Now this conclusion might prove true or false (since the inference is an ampliative one), but the inference is at least prima facie reliable, although that asteroid would not be observable by the naked eye.
In a similar way, van Leehuwenoek was able to describe and estimate the dimensions of various microorganisms by a very simple optic microscope,
whose reliability he knew because he used it to check the quality of the textile fabrics which he traded.

8. The stalemate argument

Perhaps the underdetermination argument could be employed
even without subscribing to the discouraging generalized Humean scepticism:
one might grant that we have *some* inductive powers,
i.e., that we can reliably rank theories as more or less plausible,
but hold that for any theory we would rank above all its competitors,

[24] See Peirce, "Three logical sentiments"

[25] They might reply that methods reliable for knowing about observable facts are not reliable for knowing about unobservable fact, but I have argued that this suggestion is untenable: Alai, 2010. In fact, van Fraassen,in order to avoid crediting ampliative methods as applied to unobservables, refuses to credit them even as applied to boservables…

there is always at least another one which, by our lights, we should rank as equally plausible,[26] thus ending with a *stalemate*.[27]

However, it would be already good news if we could always restrict our candidates to just two,
for then the chosen theory would have 0,5 probability of being true:
that would count as a (non conclusive, perhaps non-compelling) reason to believe it.

More importantly, as noticed by Lipton, we rank our theories for plausibility
in the light of what we already know about the world,
and of criteria which are themselves influenced by our substantive factual beliefs.

Hence, our ranking could not be reliable (as the stalemate argument grants)
unless a large majority of our background theories were themselves (approximately) true: thus one must also grant that we have not only comparative inductive powers
(i.e., powers to reliably rank theories among themselves),
but also *absolute* inductive powers
(i.e., powers to come up with the true theory in a large number of cases);
hence, this *limited* version of Humean scepticism is not coherent.[28]

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[26] Laudan 1996, 33-53.

[27] Lipton 1991, p.159-160

[28] Lipton 1991, pp.159 ff.

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