The Underdetermination of Theories and Scientific Realism

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Abstract

The empirical underdetermination of theories is a philosophical problem which until the last century has not seriously troubled actual science. The reason is that confirmation does not depend only on empirical consequences, and we can choose among empirically equivalent theories on the basis of theoretical virtues. True, in current quantum mechanics not even theoretical virtues allow to choose among many competing theories and interpretations, but this is because none of them possess those virtues to a sufficient degree. However, first, we can hope for some advancement in the state of the art: in past history we never witnessed unsolvable scientific problems. Second, even if no further progress came forth, all the most credited competitors agree on a substantial core of theoretical assumptions. Therefore underdetermination does not show that we cannot be realist on unobservable entities in general, but at most that in particular fields our inquiry may encounter some *de facto* limits. Realism is not a claim of omniscience: since the reality we know is subject-independent, gaps or mistakes are always possible in principle.

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 (in short: EE) but contradictory competitors, any one has only $1/\infty$ chances of actually being true.

This is a key argument for antirealists, based on a strong empiricist standpoint, the leeway between observational data and theory. It has been employed by many antirealists of 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, it is disputable whether in the history of science any

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

couple or triplet (let alone larger sets) of theories has been *absolutely* EE, i.e. compatible with *all possible* evidence. By 'possible' one may refer merely to *logical* possibility, or, more naturally, to physical possibility as well. But it may be argued that in past history there have been no couples of theories which were compatible even just with all physically possible evidence, let alone with all logically possible evidence.

Certain couples of contradictory theories (e.g., undulatory vs. corpuscular theory of light, phlogiston vs. oxygen chemistry, etc.) and at least one triplet (Ptolemaic vs. Copernican vs. Tychonian cosmology) have been competing for a more or less long time, because they were *provisionally* EE, i.e. compatible with all the evidence *available at the time*; but later on new incoming evidence allowed to choose one over the other. 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, pp. 454, passim; Stanford 2009a). 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.

Probably at least some theories which were actually discarded might have been kept compatible with the new evidence by suitable adjustments of the auxiliary assumptions and/or background beliefs (even without recourse to outright *ad hoc* changes). According to Duhem (1906) and Quine (1951, § 6) this holds in principle, and something of this kind has been argued even for theories we now believe to be radically wrong, like e.g. phlogiston (Loy 2011). Whether this was always possible or not, however, in actual history at some point it was clearly felt that a defense of those theories against adverse evidence was no longer reasonable or plausible. For instance, the Copernican system prevailed even before the empirical evidence from space travels that today refutes the Ptolemaic and Tychonian systems became available.[2] Even now one could build a very complex non heliocentric system compatible even with observations obtained from space travels, but the exercise is considered obviously futile.

In contrast, the idea of *absolutely* EE theories has been entertained only as a mere theoretical exercise or a thought experiment, like Poincaré's and Reichenbach's opposite systems of non-Euclidean geometry plus ordinary physical laws vs. Euclidean geometry plus non-standard physical laws.

Understandably, therefore, some authors refuse to take seriously the claim that theories are in principle underdetermined by empirical evidence, and argue 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".[3]

However, the antirealist argument is an in-principle argument, so also the realist response should be an in-principle one: even if for all couples of theories T and T' which are EE at time *t* some new evidence *e* could be found at time *t'* confirming T' but not T, it is a logical fact that at *t'* one could construe many more theories equally confirmed by all past evidence plus *e* (Kukla 1996, p. 142).

Besides, even if this in-principle problem would not trouble scientist in practice, realists should be able to explain why this is so, in the face of the logical truth that any body of data is always compatible with infinite theories. As pointed out by Kyle Stanford, there may equal or better alternatives to our theories which we just fail to conceive, and this has often happened in history.[4]

Moreover, the contemporary landscape is somewhat more complex than the historical record. For instance, in quantum mechanics a number of incompatible views are currently held which appear to be experimentally undecidable: we have first of all (a) three alternative formalisms, which

[2] Essentially, it prevailed when it became clear that it was the only one compatible with Newtonian dynamics.
[3] Stanford 2009a; see also Kitcher (1993, 154); Leplin (1997), Achinstein (2002)
[4] Stanford 2006. More on this in the last section below.

are mathematically equivalent: Schrödinger's wave mechanics, Heisenberg's matrix mechanics, Feynman's path integral formalism. Secondly we have (b) various "interpretations" of the standard theory, which differ on how to understand the wave function, the non-commutativity of operators, measurement, the completeness or not of the theory, determinsim or not, etc. They are Bohr and Heisenberg's[5] "Copenhagen" interpretation, De Broglie's "double solution", Wigner's theory of the collapse as produced by conscience, relational quantum mechanics, statistical quantum mechanics, informational interpretations and informational ontologies, modal interpretations, and the coherent histories interpretations.[6] These interpretations are in principle absolutely EE. Thirdly, there are (c) alternative and incompatible formalisms, which introduce different laws or equations: Bohmian mechanics, the objective collapse theories, the many branching universes or branching spacetime theories, temporally symmetric theories, decoherence theories, quantum field theory. Given the stunning empirical success of the standard theory, these alternative theories are designed to yield the same predictions at least concerning what is currently observable. However, they are not in principle EE, and experimental tests discriminating some of them from the standard theory are sought or already attempted.

Furthermore, some contemporary theories of quantum gravity, especially the "string theory", have different consequences, but they can only be tested at very high energies (and very small distances), and many think that such tests will be impossible (accelerators would be needed with dimensions of the scale of the solar system).[7]

Of course, since this contemporary landscape is still so uncertain and debated, it can hardly offer decisive answers on philosophical questions. Yet, scientific realists must consider at least the possibility that at least some of these alternatives prove in the long run to be serious competitors, and they must provide a plausible defense of realism if this turns out to be the case.

2. Confirmation vs. entailment

The most convincing and widely accepted explanation of why underdetermination is not an unsurmountable problem for scientists, nor an obstacle for scientific realism, is that in practice scientists don't buy premise (iii) of the above skeptical argument:

(iii) the only evidence for a theory consists in its testable 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. Therefore "being an empirical consequence of a theory is neither necessary nor sufficient to qualify a statement as providing evidential support for the theory".[8] Let's briefly review point (1) and (2).

2.1 Not all empirical consequences confirm

(1) As shown by Hempel's ravens paradox, not all empirical consequences confirm: the law that all ravens are black is equivalent to the claim that any non-black thing is not a raven; yet, observing a non-black thing which isn't a raven, like some white sodium chloride, does not confirm it. So, a given datum (e.g., that this white powder is sodium chloride) may be consequence of two different theories (e.g., respectively, a certain chemical theory, and the law that all ravens are black) yet it may confirm the former but not the latter.

[5] Although with some differences between these authors.
[6] See also Dieks 2017, p. 303.
[7] I owe this suggestion to Dennis Dieks.
[8] Laudan and Leplin 1991, 466. See also Laudan 1996, ch. 3, Psillos 1999, 169-176.

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 law, holding that *all the planets except Mars* have circular orbits: the fact that Mars' orbit is not circular, though a consequence of my new law, does not confirm it. This shows that confirmation is not a merely logical relation, but an *epistemic* 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 one of its consequences (Psillos 1999, pp. 169-173).[9] For example, data about the behaviour of uranium in the laboratory 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. Now, suppose there is a different geological theory **GT2**, not supported by carbon 14 dating: in that case, 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. An actual example is supplied by Acuña and Dieks (2014, § 6): data about Mercury's perihelion and the bending of light support special relativity although they are not derivable from it, because they support general relativity, which in turn supports special relativity.

3. Confirmation beyond empirical consequences

3.1 Non observational evidence

Once we appreciate that confirming evidence does not coincide with the 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 (Williamson 1997, Bird 2016), and in fact (b) scientists often use non observable facts as evidence for their hypotheses (Bird 2018).

[9] In a similar way, Laudan and Leplin (1991, 461-462) argue that if a theory *T* entails two logically independent theoretical hypotheses H_1 and H_2, and if in turn these hypotheses entail the classes of observational consequences E_1 and E_2, respectively, then the truth of any member of E_1 will support H_1 and *also* H_2,even though H_2 does not entail any statement in E_1. Against this argument Samir Okasha (1997) raises a problem already noticed by Hempel (1945, 103-104): the argument assumes the principle (AS): "any statement (abductively) supports any statement by which it is entailed". It is thanks to (AS) that E_1 supports H_1 and H_1 supports *T* (which in turn supports H_2). But (AS) has the absurd consequence that every statement confirms any other one. In fact, take any couple of arbitrary statements *P* and *Q*. By IS *P* confirms (*P*&*Q*) since (*P*&*Q*) entails *P*. But (*P*&*Q*) confirms *Q*, because it entails it. So, by transitivity *P* confirms *Q*. Therefore (AS) must be rejected, and with it Laudan and Leplin's argument (Okasha 1997, 253). Now, why (AS) doesn't work is clear already from Occam's razor: any evidence supports only the hypotheses which is strictly needed to explain it, to the exclusion of any redundant hypothesis. Equally, therefore, (AS) must be rephrased as (AS'): "Any statement supports any statement which is strictly needed to entail it, to the exclusion of any redundancy". Now, (AS') blocks the derivation of *Q* from *P* (because (*P*&*Q*) is not strictly needed to entail *P*, since *P* itself is enough); yet, (AS') is enough to show that there may be some evidence E_1 which supports H_2 even if it is not entailed by it, as argued by Laudan and Leplin. On this issue see also Acuña and Dieks (2014, § 4.2.2).

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 **P**, hence we can use **P** as evidence, or we simply *think* we know that **P**? And (b) how can we distinguish whether something is a fact, or we only *think* it is a fact? Moreover, using non observable facts as evidence presupposes that non observable facts can be known, i.e., it presupposes scientific realism, 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 and *unify* them into a systematic account,
- are *strong*, i.e. explain and unify the largest possible number of phenomena;
- are *fecund*, i.e. do not simply account for known phenomena, but make new testable predictions, so enlarging the body of known data;[10]
- are *plausible*, because they employ mechanisms which are already known to work in other contexts;[11]
- are *consistent* with the rest of accepted theories and background and metaphysical beliefs;[12]
- are *consilient* with them, and support and/or are supported by them.
- are *ceteris paribus simple*, without *ad hoc* clauses - and in any other possible way.

Thus, 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 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, looking for a law 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.[13]

Of course, the concepts of these virtues are somewhat vague, admitting of different possible characterizations, so that there may be borderline cases and ambiguous cases. For instance, there may be different criteria of simplicity, so that theory A may be simpler from one point of view, and theory B from another point of view; explanation is an interest-relative and context-relative, so that theory A may be explanatorily better from one point of view and theory B from another; etc. But there are also many typical, clear-cut and univocal cases, which most of the times can decide between otherwise EE theories. It can be argued that since theoretical virtues are vague, ambiguous, subject- and context-dependent, any choice based on them cannot be objective and epistemically compelling, and their importance is pragmatic, not epistemic (Acuña, Dieks 2014 § 3; Dieks 2017, p. 298). But there is no reason to deny that they have epistemic value at least in clear-cut and univocal cases; true, in the other cases they may leave the choice undecided, but the history of science shows that these cases are few.

[10] As shown in the literature, a phenomenon counts as novel even if it was known beforehand, but the theorist didn't use it in building the theory that predicts it (Alai 2014b).

[11] For instance Boyd (1973, pp. 7-8) holds that Poincaré's theory of Euclidean space plus distorting forces is less plausible than its rival because such distorting forces, which are not generated by matter or by motion are unlike any forces we know of.

[12] For instance, Acuña and Dieks (2014, § 6) notice that although Lorentz' electron theory was EE to Einstein's relativity theory, it was abandoned as quantum mechanics became accepted, around 1911, because it was incompatible with it.

[13] Fano 2005, p. 166; Psillos 1999, p. 163.

Another problem is that different theoretical virtues may recommend different theories: if theory T has virtue X and theory T' has virtue Y, we may be uncertain which one to choose (see Acuña, Dieks 2014 § 3). Actually, however, scientists want a theory to have (to a reasonable extent) *all* of those virtues, for they assume that a true and informative theory would have all of them. In fact, whenever in the past centuries they entertained two EE theories, they eventually resolved to choose one only when they were satisfied that it did satisfy (to a reasonable extent) all of those requirements; besides, they always came to a decision in this way. Even today, it may be argued that the persisting coexistence of so many alternatives in quantum mechanics is due precisely to the fact that none of them fulfills enough of those requisites. I shall come back on this below.

Thus, for right or for wrong, scientists don't believe premise (iii) of the underderdetermination argument, that the only evidence for a theory consists in its testable empirical consequences. Compliance with data is certainly good evidence for the *empirical adequacy* of theories, but the very phenomenon of underdetermination shows that it is not 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 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, uniform, orderly and rational, 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 (2014a, pp. 58-59). No wonder therefore that all the historical cases of underdetermination have been solved in due time. Scientific realists simply agree with scientists, both on the idea that we should look for truth, and on what are reliable indicators of truth.

In a nutshell, theoretical virtues have confirmatory power.[14] 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 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 empirically adequate theories, as suggested by antirealists, they wouldn't care for theoretical virtues, and would entertain with equal seriousness all kinds of weird theories, as long as they are compatible with the available data. Thus, their actual behavior should appear irrational and unexplainable to the antirealists who hold premise (iii) of the underderdetermination argument.

It might be replied that empirical adequacy (i.e. consistency with *all* possible observations) is not a property that 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 the link between theoretical virtues and truth is rather clear, which relation there could be between theoretical 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, new theories must be coherent and consilient with them in order to be true; 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 a theory 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 consistency with past observations (although of course many non empirically equivalent theories can be compatible with past observations) (Alai 2014a: § 6). If we are not looking for truth, no theoretical virtue is required: the easiest way to save the phenomena is to write the conjunction of all one's observations, i.e. a theory with no simplicity and no capacity to unify or explain.

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[14] Glymour (1980), Kosso (1992), Psillos (1999), 171-176.

4. Ways out of underdetermination in the past and now

This explains why in the past so few EE theories have been considered as viable competitors, and how, when two or more such EE theories have been entertained, scientists have been able to decide nonetheless. To begin with, some were only *provisionally* EE, and the tie was broken when new evidence came in. In other cases, however, scientists were able to choose even before new decisive empirical data became available (as when the Copernican system was finally accepted).

As explained by Duhem (1906) and Quine (1951), in principle any theory can be salvaged from contradictory evidence by adjusting auxiliary assumptions or modifying background beliefs; in practice, however, at some point scientists give up these defensive moves (as it happened with the phlogiston theory) when they realize that a theory, even if able to "save the phenomena", is too weak from the point of view of theoretical virtues: in fact, it could be kept compatible with the prima facie contradictory data only at the cost of worsening its explicative and systematic power, plausibility, consilience with other theories, simplicity, etc. (Lakatos 1968).

This also suggests how we should look at currently underdetermined theories. For one thing some of the competitors in quantum mechanics are alternative formalisms, with different laws, which are only *provisionally* EE, and we may expect that some crucial experiment in the future decide among them.[15] Equally, we may hope that some tests become available in the future for the different predictions of theories of quantum gravity.

Granted, it might also happen that such tests are forever barred to us because of the too high energies needed or other technical problems. Besides, for some competitors in the field of quantum mechanics no experimental discrimination is humanly foreseeable,[16] and the different ontological "interpretations" of quantum mechanics have been designed to yield in principle the same empirical predictions of the standard theory, hence are *absolutely* EE to it. In these cases we should look at theoretical virtues. For instance, Richard Dawid (2013) proposes that theories which are beyond the current possibility of empirical tests are evaluated through meta-inductions on the form of successful theories and on the basis of theoretical virtues, etc.

But if theoretical virtues can be employed to break empirical underdetermination, why no clear and widely accepted choice has been made in either quantum gravity or quantum mechanics? As I suggested above, this is probably because scientists are not happy with a theory unless it fulfills all or most of the theoretical requirements to a reasonable extent. Of course, when just one alternative is available, then it is used, *pour faute de mieux*, even if not completely satisfactory from this point of view (in fact, even if not completely satisfactory from the *empirical* viewpoint). For instance, the Ptolemaic system performed rather poorly from the point of view of explanatory power and simplicity, but it was used for centuries because nothing even distantly comparable to it was available. Equally, Newtonian mechanics was used for two centuries in spite of its recognized empirical failure in describing Mercury's perihelion. But when more alternatives are available, each of them unsatisfactory in different ways but to a similar degree, then a decision may be withdrawn, exactly as it would be if all of them were equally fully satisfactory.

This seems to be precisely the case with the alternative interpretations of quantum mechanics, and this is what has fueled unending discussions in the foundations of quantum mechanics. For instance, Bohm's theory postulates an implausible and otherwise unsupported instant dependence of everything on everything else (Dieks 2017, p. 309). The many world interpretation contrasts with our very basic belief about the uniqueness of the universe, and it is otherwise unsupported; the causation of the wave collapse by the conscience is at odds with the firm belief of moderns that the spirit cannot directly act on unanimated matter, and it is otherwise unsupported; the Copenhagen

[15] E.g., see Dieks 2017, p. 302.
[16] See ibi, p. 308.

interpretation avoids all these shortcomings, but it provides no picture of the unobservable mechanisms and no explanation of the empirical regularities; etc.

"Summing up ... no argument has been presented that singles out one of the various interpretations of quantum mechanics as objectively better than the others" (Dieks 2017, 311). Are we therefore stuck in underdetermination, and does contemporary microphysics mark the triumph of the EU argument?

One might suggest that since these alternatives are just different *interpretations* of the same theory,[17] this is not a case of underdetermination of different theories. But since they make different and incompatible factual claims about the ontology of the microphysical world, they should rather be considered as wholly different theories (Dieks 2017, p. 308). The difference among these theories may be discounted by hardcore positivists, but clearly not by scientific realists, who believe science should provide a true picture of the unobservable structures and ontologies.

Rather, realists can answer that the serious theoretical shortcomings of all these theories indicate that, in spite of their compatibility with available evidence, none of them is true, and we should continue our search for a better theory. After all, the historical record is that all theories which in the past did not have all the required theoretical virtues have been replaced in due course.

But what if we never find a better theory, i.e., one with sufficient theoretical virtues? What if we never progress in experimental techniques enough to test currently untestable but in principle testable theories? Good scientific methodology requires that we proceed as better theories and more powerful experimental techniques could always be found; besides, so far we have encountered no *insoluble* problems in the history of science. Still, this might no longer be the case in the future.

But even in that unfortunate case, we should simply conclude that as a matter of fact our quest for knowledge encounters insuperable limits in the quantum world; this however is quite different from saying that we cannot gather enough justification for *any* theory on *any* subject, as claimed by the empirical underdetermination argument. Further, this would not make realism impossible even in microphysics itself: in fact, physicists ordinarily discuss of unobservable objects, like elementary particles, fields, strings, membranes, etc., and such discourse is not understood instrumentalisticlly, as a mere *façon de parler*, but as referring to actual entities (Dieks 2017, 311-312).

This is because, in spite of their persisting divergences on the precise nature of the microphysical world, the different theories and interpretations are constrained by the empirical findings, which limit the possible options. As a result, for instance, any admissible formalism must in some way reflect non-locality and contextuality. This has been shown by the "no-go theorems" of Von Neumann (1932) Bell (1964) and Kochen and Specker (1967) (Dieks 2017, 312). As remarked by Cordero, the most credited competitors agree on many substantial claims: standard quantum mechanics, Bohmian mechanics, the spontaneous collapse theories, and the many decoherent worlds theories,

> all associate the quantum state with a peculiar physical field, all include the Schroedinger equation centrally in the dynamics, all endorse a strong form of ontic-structural nonseparability, and all agree on geometrical relations between sub-systems (internal molecular shapes, atomic and quark structure, etc.) (Cordero 2011, S307).

Thus, while we currently ignore the truth on the questions on which they diverge, and perhaps we will ignore it forever, there are also matters about which we have well founded beliefs and probably even knowledge. Realism is not belief in omniscience. The idea that there might be something unknowable is absurd for idealists, who identify reality with the content of thought. But realists hold that we can get knowledge of a *subject independent* reality (observable and unobservable alike). Therefore, gaps or mistakes are always possible in principle.

[17] Stanford 2009a, footnote 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), Halvorson (2012, 2013).

5. Underdetermination *even* by theoretical virtues

Antirealists have objected that in certain cases not even theoretical virtues could discriminate between competing theories. Let's examine four kinds of cases.

5.1 Trivial permutations

First, theoretical virtues couldn't discriminate between 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.[18] So, this undecidability is not a problem for scientific realism.

5.2 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;
- **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 (Kukla 1993, 1996).

Further similar alternatives are mentioned by Fine, van Fraassen, Lyons, Stanford, etc. (Alai 2014a, § 2). However, as noticed by Stanford (2001), this would reduce the specific problem of scientific underdetermination to the metaphysical skepticism of Cartesian evil demon. Moreover, 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'** (i.e., the theory that **T'** is empirically right but false) does not. In general, no theory stripped of all its theoretical claims can explain anything: "the hypothesis that it is raining explains why the streets are wet—but 'The phenomena are as if it were raining' does not" (Musgrave 2006-2007, Alai 2014a, § 2). Moreover, an instrumentalized version of **T**, like **T'**, cannot be a rival to **T**, because it is a logical consequence of **T** (Laudan and Leplin 1991, 456-457).

As for **T"**, it 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. Laudan and Leplin (1993) argue that the claim by which **T"** differs from **T** (i.e., that the world behaves differently when not observed) is superfluous (since it has no empirical consequences), implausible (since it implies a violation to the uniformity of nature we have never observed) and untestable.

Thus, Kukla's algorithms fail to produce theories that are equivalent not just empirically, but also from the point of view of theoretical virtues. This failure does not refute the general point that in principle for any theory there may be thousands of EE theories. It doesn't even refute the *possibility* that *some* of those alternative theories are undetermined also by theoretical virtues.[19] However, Kukla's failure deprives of any support the claim that for *any* theory there are empirically *and* theoretically equivalent theories.[20]

5.3 Mathematically intertranslatable theories

Philosophers have suggested various examples of EE theories 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) (Putnam 1978a, p.153, 1981, pp. 81-82);

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[18] See footnote 17.

[19] One argument showing this *possibility* is offered by Bangu (2006, 273-274). But this is not a necessity, and it needs not hold for *all* cases. On this see also the discussion in Acuña and Dieks (2014, § 4.2.3). I agree with Acuña and Dieks (2014, § 5) that the existence of particular couples of theories empirically and theoretically underdetermined is possible.

[20] On this point see also the discussion by Acuña and Dieks (2014, § 4.1.2).

2. theories introducing fields vs. theories using action at distance with retarded potentials (ibid.);
3. Heisenberg's matrix mechanics vs. Schrödinger's wave mechanics and Feynman's path integral formulation;[21]
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;[22]
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.[23]

These couples of theories are more closely related than the alternative quantum theories considered above, which needed not be mathematically intertranslatable. Thus, one could suggest with stronger grounds that these alternatives are just *different formulations of the same theory*, therefore they are not mutually incompatible, but can be contemporarily true. Again, however, 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 item 4. of this list 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.

Still, realists can deal with these cases by three (not mutually exclusive) strategies:

(I) they can argue that these theories are mathematically intertranslatable only because their diverging ontological claims are not fully expressed by their formalisms nor reflected in their empirical predictions. But perhaps one they will be developed so to express them, and become empirically decidable. For instance, as long as only the geometrical formalisms of the Ptolemaic and Copernican systems were considered, they were mathematically intertranslatable. But soon it became possible to attribute to their diverging claims a physical meaning (for instance by specifying their different dynamics), and so also deriving different empirical predictions from them.

(II) Further, realists may insist 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 is currently preferred 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, 2009a), 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. As remarked earlier, granting that we may be *de facto* unable to know the truth about some particular subject is not just *compatible* with realism, but entailed by it.

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 realists (Poincaré 1902, Worrall 1989, etc.), or even *all that really exists*—as claimed by *ontic* structural realists (Ladyman and Ross 2007, 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).

[21] Putnam 1978b, p. 555; Friedman 1983, pp.165 ff.; Fano 2005, p. 166.
[22] Earman 1993. Instead, for John Norton (2008) this example simply involves two notational variants of a single theory (see footnote 17 above); see also Stanford 2009a.
[23] Earman 1993; see also Stanford 2009a.

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).[24] But this difficulty does not arise for mathematically untertranslatable theories, for (in some important sense) they describe the same structure. 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.

5.4 Same content in different schemes

Again, it may be remarked that theoretical virtues cannot discriminate between theories which Goodman and Putnam call "equivalent descriptions", for they are not just mathematically intertranslatable, but have also an equivalent theoretical content. The following are ways of getting *equivalent descriptions*: describing reality alternatively by speaking of rabbits or of rabbit-stages; conceptualizing rain as an object or as a process (Quine 1958); using as primitives both objects and mereological sums, or just objects; using lines and points or just lines;[25] describing spacetime events by different simultaneity concepts; assuming that the Earth is motionless and the Sun rotates around it, or that the Sun is motionless and the Earth rotates around it, or that both move in space, etc. (Goodman 1978, ch.VII).

But the impossibility of choosing in such cases is not a problem, because equivalent descriptions are simply descriptions of the same systems couched in different conceptual schemes, or in different coordinate systems, or frames of reference: they just express the same content in a different form, that is why they are mutually compatible (Alai 1994, ch. 3).[26]

6. 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 evidence *and* theoretical considerations). However, he describes a form of provisional underdetermination (i.e., underdetermination by currently available evidence) which we face all the time, and is equally or more threatening for realists. In fact, *at any time t* there are possible alternatives to the currently accepted theories which we cannot even imagine: because of the limits of our mental capacities and current epistemic conditions, numberless possible alternatives "exceed our grasp". Besides, many of these unimagined alternatives are EE to the actually entertained theories, and some of them are also on a par with them from the point of view of theoretical virtues. Therefore underdetermination is a constant phenomenon: even when we actually entertain just one theory, it is empirically and theoretically underdetermined with respect to its virtual alternatives.

It might seem that this underdetermination is not an unsolvable problem and a dramatic predicament, since it is "transient":[27] no doubt, future research will bring in new evidence, by which many of the theories which are empirically and theoretically equivalent at time *t* will be ruled out at a later moment *t'*. But the trouble with this transient underdetermination is its recurring character: no matter how much new evidence comes in and how many actual or virtual competitors are ruled

[24] Lyre also thinks that actual of underdetermination are too few to support the general underdetermination thesis, but again, the question is why.
[25] Putnam 1978b: 130-133; 1987: 32-33; etc. See Alai 1994, ch. 3.
[26] 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 (Alai 1994, ch. 4).
[27] As it is called by Sklar 1975, 1981.

out at the later time *t'*, so many others will remain compatible with all the empirical and theoretical constraints at *t'*, and so on for any later time. This is a direct consequence of the logical point that the possible theories compatible with any given body of data are infinite.

Stanford's historic examples make his case very convincing: 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 gained better support than its predecessor (Stanford 2009a). Moreover, there are similar and well known examples in electromagnetism, chemistry, cytology, etc. (Stanford 2009b, 263-265). Therefore, not only underdetermined alternatives are always there, but apparently the true one is always among those overlooked by the scientists.

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. Yet, whenever only one theory **T** is actually accepted, our awareness that we don't know all the relevant data results in the lack of certainty that **T** is true; but 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 practical certainty that **T** is false.

Thus, in Stanford's treatment underdetermination converges with the other main antirealistic argument, the infamous pessimistic historical meta-induction, of which his examples remind: 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 precisely by the two main strategies used against the pessimistic induction (see Alai 2017a, § 1):

(1) noticing that 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 (Fahrbach 2011, Cordero, 2017a, 2017b). It can hardly be disputed that all this allows us to conceive better and better theories and to rule out more and more theories which do not satisfy either some empirical or theoretical constraints. Therefore, the number of unconceived but epistemically reasonable theories 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.

(2) In addition, 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 identify some true components of false theories. To be precise, I argued (Alai 2017b) that we can 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.

Therefore Stanford's underdetermination (just like the pessimistic induction) does not show that we are unable to achieve theoretical truth; it simply shows that (a) we haven't reached the *whole* truth, yet; (b) possibly we won't ever reach it (perhaps we *would* reach it only at the ideal limit of inquiry, if we could get there); (c) we don't know "how far" we are from it now (assuming talk of distance makes sense here); and (d) quite possibly there will still be radical scientific changes in future science.

7. Objections and replies

7.1 The holistic extension of underdetermination

I have claimed that theoretical virtues can to a large extent solve the problem of underdetermination. But antirealist might reply by a number of objections, the first of which extends underdetermination holistically by advancing two possible scenarios.

7.11 First possible scenario: 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 only if we assume that accepted theories and beliefs *themselves* are *largely true*. If they are largely false, then compatible and consilient theories may be equally 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".

However, following Wittgentsein (1969), Quine (1960, p. 59), Davidson (1974, p. 19), and Dennett (1978, p. 20), it can be replied that 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.[28] Building such a coherent system may be difficult, but when we discard an otherwise empirically adequate theory because of its inconsistency with other theories, we are progressing toward that end.[29]

Antirealists might counter that even if this reply is correct, it only shows that the largest part of our beliefs must be true. But that part might consist exclusively of empirical beliefs, 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, theories selected because of their consistency with accepted beliefs and theories (and of the other theoretical virtues) are often predictively successful. This shows that these virtues are reliable indicators of truth; further, since our background theoretical beliefs have been selected by the same criteria, probably they are largely true, and theories consistent with them have good chances of being true as well.

7.12 Second possible scenario: alternative ampliative principles

The second possible scenario, again in the footsteps of Quine, is that our ampliative inferential principles, based on the abovementioned theoretical virtues, may be questioned: for instance, Quine would suggest that any of them may be given up in order to defend a 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 (Stanford, 2009a).

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 reasons to assume that there cannot be better principles. For instance, Reichenbach (1935) argued that if any inductive method can lead us to the truth, the straight rule of induction does.[30] Besides, even from a Quinean holistic 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.

[28] See also Grandy 1973, Stich 1991, ch 2.
[29] See also Dorato 2007, p. 196.
[30] Problems have been raised with his argument, but now we have an even better grasp of the truth-conducivity of inductive methods.

7.2 What if nature is not **simple, uniform, orderly, rational,** *and our ampliative methods fail?*

It might be objected that theoretical virtues can be taken as indicators of truth only if nature is simple, uniform, orderly, 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.[31]

In other words: how do we know that theoretical virtues are relevant to truth? According to McAllister we know by meta-induction, because scientists select as theoretical virtues those features which characterized past successful theories (McAllister 1989, 39, Dawid, 2013). Of course there is a problem, viz. that "the meta-induction at issue is strictly *Humean*, in the sense that it is based upon mere past correlation, without any guarantee of empirical success *now*" (Acuña, Dieks 2014 § 3). But again, raising *this* problem would be to reduce the problem of scientific realism to the old Humean puzzle of induction. Instead, they are two separate problems: we may take induction as reliable, and still have the realism problem: in fact, many are scientific antirealist even while accepting induction as a valid inference method. Conversely, if induction is rejected we get a generalized skepticism, non only on unobservables, but on observables as well. Therefore the realism debate, the Humean problem should be bracketed, at least for the sake of discussion.

After all, as noticed by Peirce (1931-1935), 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. with respect to the objection we are considering here this means that since experience so far tells us that nature *is* simple, uniform, orderly, and rationally explainable, we have no reasons to doubt that, hence no reasons to doubt that theoretical virtues are indicators of truth.

7.3 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, there is always at least another one which, by our lights, we should rank as equally plausible (Laudan 1996, 33-53), thus ending with a *stalemate* (Lipton 1991, 159-160).

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 (inconclusive, 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; but this shows 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 (Lipton 1991, pp.159 ff.).

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[31] See Lipton 1991, ch. 9.

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