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Underdetermination, Scepticism and Realism[1]

LADISLAV KOREŇ

Department of Philosophy and Social Sciences. Faculty of Arts. University of Hradec Králové
Náměstí Svobody 331. 500 03 Hradec Králové. Czech Republic
ladislav.koren@uhk.cz

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ABSTRACT: This study aims to articulate and compare the structure, presuppositions and implications of two paradigmatic sceptical arguments, i.e. arguments from underdetermination of scientific theories by observational data (*UA*) and Cartesian-style arguments (*CA*) invoking sceptical scenarios of severe cognitive dislocation. Although salient analogies between them may prompt one to think that a unified diagnosis of what is amiss with them is called for, it will be argued that this may be a false hope, if those analogies do not underwrite a complete homology. That said, possible parallels of one promising anti-sceptical exposure of *CA* are pointed out for the case of *UA*, which conspire together to render the problem of underdetermination less threatening than it could at first appear.

KEYWORDS: Evidence – realism – scepticism – underdetermination.

A venerable tradition portrays science as aiming at explaining observable patterns through deeper causal mechanisms that, for all their hiddenness, are "really out there" in the natural world. Scientists, striving to "cut nature at its joints," work hard to amass data via observation or controlled experiments to make it likely that their theories give increasingly more accurate and comprehensive accounts of the ready-made world out there, including, prominently, its hidden joints. What is more, a modest epistemological op-

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timism seems warranted. At least with our mature and impressively successful scientific theories, evidence accumulated over time warrants belief in at least their approximate and partial truth.

This familiar picture of scientific theorizing has come to be called *scientific realism* (henceforth: *SR*). Despite its initial attractions, it has mobilized many able philosophical opponents, as the ongoing controversy over *SR* vividly testifies. Among many objections that have been raised against it, *underdetermination-style arguments* (*UA*) stand out as advancing a particularly serious challenge to its epistemological optimism, on the ground that observational data – no matter how massive – might be equally accommodated by empirically equivalent theories that make incompatible claims about hidden contents and structure of the natural world.[2] In this respect, as many thinkers have noted, there are salient analogies between *UA* and the classical *Cartesian-style sceptical arguments* (*CA*). The latter threaten to undermine even our most common-sense claims to knowledge of the world around us, on the ground that the whole of our experiential evidence – past, present or future – can be accounted for by experientially equivalent hypotheses of severe cognitive dislocation[3] that make incompatible claims about the contents of the outside world (and, in particular, about the causal origin of our sensory experience).

Both *UA* and *CA* aim to establish a radical sceptical thesis on the basis of *prima facie* appealing premises. They have accordingly provoked several attempts to diagnose what is amiss with them but none has commanded wider acceptance. However, I believe it worthwhile to explore in some depth and detail the possibility that suggests itself here, namely, that aforementioned analogies between *UA* and *CA* invite analogous or even structurally unified anti-sceptical diagnoses. To prepare the ground for this issue, I first situate *UA* in its proper dialectical context, explaining its motivation, structure and possible ramifications for the controversy over *SR*. Having reconstructed *CA* as an underdetermination problem of a sort, I then compare both puzzles highlighting their analogies as well as their specifics. On this basis I sketch an anti-sceptical exposure of *CA* that I find promising, but which, owing to the specifics of *UA*, does not straightforwardly carry over to it. This, I argue, should come as no great surprise, because analogies be-

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[2] More precisely, this applies to so-called strong underdetermination arguments, according to the distinctions to be introduced in Section 1.2.

[3] To use an apt term coined by Wright (2002), (2004).

tween *UA* and *CA* do not amount to full homology. That said, I also try to show that instructive parallels of that exposure tactics can be discerned in the case of *UA*, which might conspire together to render the problem of underdetermination less threatening than it could at first appear.

1. Underdetermination problem and the controversy over *SR*

1.1. Sources

The relevant history of *UA* is usually traced back to Pierre Duhem's path-breaking critique of the hypothetico-deductive model of confirmation of scientific hypotheses.[4] According to this model, once a hypothesis *H* is proposed, one may go on to confirm or falsify it by checking its observational consequences concerning occurrence of phenomena under specified circumstances. If a phenomenon does not occur as predicted by *H*, *H* should be taken as falsified, hence rejected and replaced by another hypothesis. If, on the other hand, phenomena occur as *H* predicts, *H* gets (increasingly) confirmed.

But things do not quite work this simple way – or so Duhem argued. First, scientific hypotheses have observational implications only within a larger theoretical context or system – if conjoined with auxiliary hypotheses and background assumptions about initial conditions, experimental setting, instruments, etc. Let us call this enlarged system *H+A*, *A* incorporating whatever relevant auxiliary hypotheses or background assumptions. Second, Duhem pointed out that observational consequences do not – by themselves or together with logical principles – determine confirmation or falsification of the hypothesis *H*. Thus a false observational consequence does not automatically falsify *H*, because it is derived from *H+A* so that what it shows is at most that *H* is false or *A* is false. Accordingly, we may save the data by keeping *H* and making appropriate revisions within *A*, or the other way round. Still, data plus logic alone do not dictate what to do. Second, even if we assume that all so far checked observational consequences of *H* are true, this does not confirm *H* as likely to be true, as there is always a rival hypothesis *H** that, conjoined with its own *A**, accommo-

[4] See Duhem (1914/1954). Also Poincaré's (1905/1952) famous considerations about alternative, yet empirically equivalent physical geometries of space-time are often quoted as an important milestone.

dates all the data equally well, but makes incompatible claims about the underlying reality. So data plus logic alone do not dictate a choice of H over H^*.

Summing up, Duhem's holistic thesis says that larger theoretical systems, not single hypotheses, are subject to empirical test. Its corollary then seems to be that observational data together with logic are not enough to determine (a) what item is falsified vis-à-vis recalcitrant data and (b) which of two rival but empirically equivalent theoretical systems is more likely to be true, hence worthy of choice.

A few decades later, Quine contributed by two generalizations of Duhem's claims. First, in his assault on empiricism he argued that the system (theory) to be tested against sensory experience is a whole web of interlinked beliefs including also logical and mathematical claims, so that they too are eventually up for revision if the system faces recalcitrant data (cf. Quine 1951). Second, whereas Duhem had in mind empirical equivalence – hence underdetermination – relative to all available data (already checked observational consequences), Quine radicalized *UA* to mean underdetermination of whole (perhaps even comprehensive) physical theories relative to the totality of all possible data. Importantly, the strong version of *UA* excludes the possibility that future observations could eventually distinguish H and H^*.[5]

It is arguably easier to come up with historical examples of the weak underdetermination. One notorious example put forward by Duhem himself is the transient empirical equivalence of the Ptolemaic and the Copernican system: both were able to accommodate all observed astronomical data at the time of appearance of *De Revolutionibus*. Hence, at the time, available empirical data did not support one system over the other as more likely to be true. We shall have an occasion to see that the situation is more delicate with the strong underdetermination, there being fewer genuinely interesting examples of alternative theories that are both incompatible and empirically

[5] Okasha (2002) argues that the strong version of *UA* is hard to reconcile with Quine's own holism: either a theory T is incomplete/local and then it is always possible that future evidence will distinguish it from currently equivalent rival theories (thus we have at most the weaker Duhemian version of *UA*); or the argument has to be formulated at the level of global (complete) theories – but then we do not quite know what it would be like to have such a theory. See also Hoefer – Rosenberg (1994) who distinguish local and global theories and, accordingly, local and global underdetermination, holding that only the latter is a potential threat.

equivalent relative to all possible data. Let it be said that the clash of rival accounts of quantum mechanics – viz. Copenhagen interpretation versus Bohmian interpretation in terms of hidden parameters – is often cited as a particularly up-to-date example of the strong underdetermination. Also Poincaré-style examples of rival space-time cosmologies based on different geometries seem to supply instructive examples – viz. a space-time theory based on the classical Euclidean geometry and a space-time theory based on the non-classical Riemannian geometry.[6] The idea behind is that all possible predictions about trajectories of physical objects made by one theory can be mimicked by the other: thus, curved trajectories of objects predicted by the space-time theory based on the Riemannian geometry can be mimicked by the space-time theory based on Euclidean geometry if we introduce into the latter theory extra-forces acting on objects as well as rulers. The two alternative theories seem equally underdetermined by all possible data.

1.2. The role of UA in the controversy over SR

It is the strong version of *UA* that will concern me in what follows. For one thing, it seems to be potentially more devastating. For another, it is structurally analogous to the Cartesian-style sceptical argument with which it will be compared in due course.

To reconstruct *UA* in its strong form, a few distinctions are in order.[7]

C belongs to the testable basis of a theory T iff *C* is a claim about phenomena and *T* allows us to infer *C*.

T and T are empirically equivalent theories* iff they have exactly the same testable basis.

T and T are rival empirically equivalent theories* iff they are empirically equivalent theories that make incompatible claims about unobservable items.

T and T are evidentially indistinguishable theories* iff they are equally well supported with respect to all possible evidence so that any possible ob-

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[6] See Poincaré (1905/1952). Reichenbach (1958) formulates a famous generalization of this procedure.

[7] There is already an extensive literature on underdetermination arguments and their relation to the issue of *SR*. For different perspectives on it, I refer the reader to Laudan – Leplin (1991), Earman (1993), Hoefer – Rosenberg (1994), Psillos (1999), Stanford (2001), Devitt (2002), Okasha (2002), Norton (2008).

servation that contributes to confirming (or disconfirming) one (to a given extent) confirms (or disconfirms) equally well the other (to the same extent).

With this terminology in place, it is easy to formulate *UA* as involving the following steps:

1. For any scientific theory T there is an alternative theory T^* such that the two are rival empirically equivalent theories.
2. Empirically equivalent theories are evidentially indistinguishable.
3. So no possibly relevant evidence supports T as more likely to be true than T^*.
4. So no possibly relevant evidence justifies one in holding T to be more likely to be true than not.
5. So one is never justified in holding T to be more likely to be true than not.

Simple and abstract as *UA* is, its ramifications for *SR* are potentially far-reaching. To appreciate this, we should note that *SR*, as commonly conceived, involves a mixture of essential ingredients backing up the claim that scientific theories aim to provide approximately true descriptions of the objective sort that are apt to be warranted by empirical evidence:

Semantic commitment: Sentences comprising theories – including those apparently about unobservable items – are to be taken semantically at face value, that is, as purporting to refer to and describe what they *prima facie* appear to refer to and describe (truly or falsely).

Ontological commitment: There is some stuff out there for them to be approximately true descriptions of – including entities, structures, etc. underlying phenomena and their regularities.

Independence commitment: That stuff *is what it is* quite independently of any linguistic, conceptual or epistemic means that can be employed in positing, classifying, describing or confirming them.

Epistemological commitment: Empirical evidence can place us in a favourable position to hold (at least our best current) scientific theories to be (at least nearly and partially) true descriptions of the world – in both its observable and non-observable parts.

What *UA* urges on us is that on a realistic construal of a scientific theory *T* – as concerned with a largely unobservable and ready-made world out there – there is a gap between the two that cannot be bridged by empirical evidence, however varied and massive. Clearly, observational data do not entail theories, there being always room for alternative theories equally accommodating all the data in terms of incompatible claims about hidden structures. But, if *UA* goes through, the data do not determine scientific theories inductively either – by lending them at least a reasonable degree of support – since (a) empirically equivalent theories T and T* are held to be evidentially indistinguishable but (b), being incompatible, only one of them can hope to be true to the facts.. Thus, if *SR* with all its essential ingredients holds, then, epistemically speaking, the hidden natural world out there seems once and for all lost!

UA, I submit, can be seen as an improvement upon more traditional and controversial arguments informed by the positivist stricture to the effect that what transcends experience/observation is beyond the province of knowledge: i.e. to claim anything about unobservable items is to outstrip the bounds of evidence that only licenses warranted claims about the world out there. In response to arguments of this calibre, friends of *SR* would do well to challenge the positivist stricture behind them – *only what is observable or reducible to observable is knowable*. Even if is it granted that scientific hypotheses and theories are evidentially controlled by the data of observation and experiment, this reasonably looking empiricist regulative does not entail the positivist stricture. For why, realists ask, shouldn't theories positing unobservable items be liable to empirical test indirectly, that is, via what they (together with auxiliary hypotheses and background assumptions) imply about phenomena? Why, then, shouldn't we regard the fact that some theories fare excellently in predicting and explaining phenomena – indeed, much better than alternative theories – as giving considerable, if indirect, empirical support also to their claims about unobservable items?[8]

[8] The "No miracle" argument – sometimes dubbed *the master argument for SR* – makes this point dramatically: wouldn't it be a most remarkable coincidence – indeed, a mystery – if our best current theories, successfully predicting and explaining regularities of phenomena in terms of hidden structures/mechanisms, were not onto something as regards the hidden nature of the world? See Putnam (1978) and Boyd (1983) for influential defences of the claim that only *SR* does not make the apparent success of science one big mystery left without any explanation.

So, without a more powerful line of argument against the possibility of warranted claims about unobservables, realists may feel relatively safe. Yet, *UA* promises to give its opponents precisely such a principled argument, because it directly challenges the crux of *SR* – the claim that (our mature and impressively) successful scientific theories are likely to be true (nearly and partially).

Although *UA* directly attacks the epistemological commitment of *SR,* it could be used in drawing significant metaphysical or semantic conclusions. Indeed, scientific scepticism wholeheartedly embracing its negative conclusion has been a rare reaction to *UA*.[9] Thus, constructivists, projectivists or internal realists would argue that realities must be theory-dependent after all. Differences aside, their goal is to close the gap between theories and reality by evidentially constraining truth-conditions or urging some form of conceptual or epistemic dependence of realities on theoretical frameworks employed to capture them.[10] Others would contend that sentences apparently about unobservables should not be interpreted literally but, as instrumentalists would have us say, as computational means (perhaps useful fictions) in the service of correlating phenomena (input-data with output-predictions). Either way, the problem of underdetermination can be mitigated, though on pain of incurring theoretical commitments which are far from unproblematic.

There are other "non-realist" strategies, to be sure. Since this study is not primarily concerned to review them, suffice it to say, for this moment, that the friends of *SR* have several lines of response available to them to

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[9] A notable exception, however, is Van Fraassen's *constructive empiricism* (1980) to which I shall shortly return later.

[10] Here we may assign Kuhn's (1962/1996) *paradigm-style* account of the structure of scientific theory change with its corollary of conceptual incommensurability of scientific frameworks employing different classificatory and epistemic styles (based on different *exemplars* establishing the range of problems to be solved and the methods of solving them). On this view, realities of concern to scientists are always immanent to scientific frameworks – different frameworks coping with different realities. Though in many respects very different, Putnam's *internal realism* (1981) may also be reckoned here. Positions of this sort need not deny that scientific theories are concerned with observable as well as unobservable aspects of the world and that they aim to give us, in a way, accurate accounts of both. What they in different ways question is theory-independence of realities studied by science, unconditioned by conceptual and epistemic means that constitute theories or theoretical frameworks.

show that, while the embarrassing conclusion of *UA* compromises some of its premises, it does not reduce *SR* to absurdity, if *SR* does not license the culprit premises. I will mention some of those responses later, after having compared *UA* with *CA*. But let me now turn to the classical Cartesian-style sceptical argument and its subversive potential.

2. *CA* as an underdetermination argument

It is well known – even well worn – that Descartes bequeathed to us a sceptical problem,[11] whose ghost is still with us today, in spite of the fact that he thought to have disposed of it. In its restricted form, targeting only empirical claims about the world out there, the issue arises if we take it that at least some of our best common-sense or scientific claims – e.g. claims about medium-sized objects observable by unaided senses under optimal conditions – are certain or at least likely to be true. However, the fact that we confidently take the world to be a certain way does not yet guarantee that the world really is that way. So what basis, if any, can we have for placing such confidence in our most cherished empirical claims? Sensory experience we take ourselves to have made with the outside world seems to be, *prima facie* at least, the most plausible candidate. However, if experience consists of a stream of appearances – internal phenomena – that might just as well occur when we are being subject to a severe cognitive dislocation (e.g. devious delusion or compact dream), it does not seem to lend a reasonable degree of support to our empirical claims – still less an assurance that the world out there is, by and large, the way we take it to be. As Descartes pointed out, apparently far-fetched claims about the contents and structure of the world can accommodate the totality of given appearances – even the totality of all past, present and future appearances – which are nevertheless incompatible with the claims that we take to be true.

It suggests itself to reconstruct *CA* as a radical underdetermination problem: underdetermination of empirical claims, including all claims about observable items, by the data of experience-based evidence, no matter how varied and massive. And here is a simple receipt to this effect. First, take your best common-sense and scientific claims about the world – including those about the aetiology of your sensory experiences – as belonging to an

[11] For an original statement of the problem see Descartes (1640/1996).

empirical theory/system *B* that depends for its support, if any, on the data of experience-based evidence *E*. Then confront *B* with whatever sceptical theory *S* that is specifically devised to fit all experience-based data that you might ever have while making incompatible claims about the world (if only because it makes incompatible claims about the causal origin of experiential data). Descartes' *Evil Demon Hypothesis* might serve the purpose, as well as the more up-to-date *Brain-in-Vat Hypothesis*. Finally, ask if you have a sufficient evidential basis to favour *B* over *S*.

Now, the would-be sceptic expects the answer to be in the negative and he has a simple argument at hand. *E*, recall, is supposed to consists of all internal phenomena – items of the sort *It appears to me as if p* or *I am appeared to as if p* – that might evidentially bear on *B*.[12] We then choose as *S* some suitable sceptical hypothesis (theory) that allows us to expect all phenomena belonging to *E* – in fact, *S* typically provides an alternative account of their aetiology – but is incompatible with *B* regarding many external phenomena (including, prominently, the causal origin of internal phenomena). *CA*-style underdetermination argument is then simple:

1. *B* has a sceptical rival-incompatible hypothesis *S* that is equivalent with it with respect to *E*.
2. If *B* and *S* are equivalent with respect to *E*, then they are equally well supported by *E*.
3. So *E* does not support *B* as more likely to be true than the incompatible *S*.
4. So *E* does not justify one in taking *B* as more likely to be true than not.
5. So one is never justified in taking *B* as more likely to be true than not.

Given, further, that *E* should include all relevant pieces of evidence that might ever bear on *B* – viz. all past, present or future appearances – the argument, if sound, shows that no evidence can ever justify *B*. Since, then, *B* is arbitrary, the upshot is that no empirical claim or theory is justified – all being on par in this respect. This is certainly a radical conclusion that is hard to swallow.[13]

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[12] Note that such items make no references to the outside world and have no implications about it: *It appears to me as if p* does not imply *p*.

[13] It may be argued that the underdetermination reconstruction of *CA* is more fundamental than alternative formulations of it – viz. the so-called argument from ignorance –

3. Comparison

We have seen that strong underdetermination arguments pose a challenge to scientific realism, on the ground that virtually any empirical theory T may be confronted with a rival theory T^* that allows us to infer exactly the same claims about phenomena while being committed to incompatible claims about unobservable structures and mechanisms underlying phenomena. Assuming, furthermore, that the only relevant evidence for (or against) a scientific theory comes ultimately from observational confirming or disconfirming of its implied claims about occurrence of phenomena, T and T^* should be evidentially on a par, equally likely to be true in the light of all possibly relevant empirical evidence. If so, the presumption that such evidence could justify our acceptance of scientific theories as (nearly and partially) true descriptions of the nature of the world out there is undermined. Hence *SR* seems to be in serious trouble.

A similarly dramatic role has been imputed to the Cartesian argument, invoking experientially undetectable scenarios of massive delusion encapsulated in sceptical hypotheses. Obviously, *CA* is more radical in its intended scope and consequences. *UA* allows us to know a lot about external facts/events observable by unaided senses, but nothing about hidden entities and structures underlying phenomena. *CA*, however, threatens to deprave us even of external phenomena. Thus, e*xternal phenomena* are to be distinguished from *internal phenomena*, only the latter being immediate data of experience – *appearances* – the presence and character of which we are in a privileged position to recognize. The crux of *CA* is the claim that empirical claims about the contents and causal structure of the world – claims about external phenomena in particular – depend for evidential support ultimately on appearances. Unfortunately, sceptical hypotheses are devised so as to accommodate all our appearances – past, present or future. Thus the totality of evidence supposed to be ultimately relevant for empirical claims – one supervening on appearances – does not favour empirical claims/theories over rival sceptical hypotheses, both being equally likely to be true in the light of it. If so, the initial presumption that our best systems of empirical

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that hinge on the premise that we cannot exclude (know to be false) *S* (and on the premise that we know *P*, only if we can exclude or know *S* to be false). If this premise is not self-evident – and many would maintain it is not – then we should argue it. And, it would seem, underdetermination of *P* by *E* would have to be invoked to justify it. Cf. Bruecker (1994) and Okasha (2003).

claims about the world out there are by and large true is undermined. Hence the very idea of empirical knowledge of the world seems to be in serious trouble.

This analogy between *UA* and *CA* can be pushed further. Crucial in *CA* is their tendency to construe even claims about things and events that we would normally call (in agreement with *UA*) "observable" as if they were on a par with epistemically derivative "theoretical" claims, inheriting their degree of evidential support, if any, by inference from *the Given* – consisting of appearances as immediate data of experience. To put it slightly differently, external phenomena are placed in the troublesome position that unobservable items occupy in *UA*, on the ground that they are beyond immediate, hence safe reach of our cognitive powers. And the dramatic challenge posed by *CA* is that the evidence of *the Given* does not favour even our best common-sense or scientific empirical claims over alternative sceptical claims, because the latter claims are designed to accommodate – indeed, to account for – *the Given*.

In light of this, perhaps the most salient analogy between *CA* and *UA* is that they presuppose a foundationalist picture of evidence based on a sort of *data/theory* dichotomy and its corollary that theory-like items can be justified – if at all – only by inference from data-like items (or non-inferential reports of them), that is, from the epistemically prior experiential and observational evidence respectively (cf. Okasha 2003).

	Data	Theory
CA	Internal phenomena	Empirical: about the observable or unobservable world
UA	External phenomena	Scientific: about the unobservable world

More precisely, both arguments seem to rest on the following assumptions that work together to license sceptical conclusions:[14]

> *Inferential justification of theory-claims*: *T/B* – in particular, theoretical claims – can be justified only via a rationally compelling inference from more ultimate evidential claims.

[14] A diagnosis along these lines is offered also by Bird (2007) and Okasha (2002).

Restricted evidence: observational/experiential data-claims provide ultimate evidence for *T*/*B*.

No rationally compelling inference: rationally compelling inference can be either deductive or inductive,[15] but, no matter how massive the observational/experiential basis of data-claims for *T*/*B*:

T/*B* are not inferable from it deductively – viz. the existence of inconsistent yet empirically equivalent theories *T**/*S* that is asserted by the 1st premise of *UA*/*CA* (call this *deductive underdetermination*).

T/*B* are not inferable from the basis inductively – viz. the existence of inconsistent yet evidentially indistinguishable theories *T**/*S* that follows from the 1st and 2nd premise of *UA*/*CA* (call this *inductive-ampliative underdetermination*).

If this comparison is on the right track, one may expect that a careful analysis of one sceptical case will in turn illuminate the other. Indeed, the closely parallel structure of the two arguments may prompt one to think that there must be a closely parallel diagnosis of what – if anything – is amiss with them. This, though, may turn out to be a false hope. Granted that *UA* and *CA* are structurally parallel, this does not preclude a possibility of a promising anti-sceptical strategy vis-à-vis *CA* that might not be all that promising vis-à-vis *UA* and *vice versa*. Clearly, there is one salient difference between *CA* and *UA*: the latter typically take *phenomena* to be *intersubjective matters* accessible to observation, while the former construe *phenomena* as *subjective appearances* accessible via reflection or introspection, with no implications about the outside world – whether observable or not. It may be that precisely this difference between the two cases will prove crucial at the end of the day.

4. Exposing *CA*: not the epistemic game we play!

My favourite tactics with respect to *CA* is a genre of what Alex Byrne recently classified as "expose the sceptic" strategy, as opposed to "convince

[15] Inductive inferences are understood here in the broad sense of ampliative inferences, whose conclusions, unlike those of deductively valid inferences, state more than is contained in the premises.

the sceptic" strategy.[16] In general, this tactics hinges on the idea that we should not try – in vain – to convince the sceptic via non-question-begging arguments *by his lights*. Rather, the onus is on our imaginary sceptical opponent, who could win the game with us only if it turns out that we are committed to buy his premises as resting on plausible considerations *by our lights*. We then try to show that this is not the case: the rules of the game that the sceptic wants us to play – and inevitably lose – are not something we are committed to, not being derived from the commitments inherent in *our* discursive-epistemic practices, in which *our* epistemic standards are at home. This strategy, though firmly based in ordinary practices, does not just reassert – *à la* G.E. Moore (cf. Moore's famous 1939/1959) – the claims of common-sense as comparatively far more certain than the premises of *CA*. It proceeds by showing that the foundationalist picture driving *CA* is not something we are committed to. There are two main considerations that I want to briefly mention in support of this claim.

First, *our* conception of relevant empirical evidence differs in crucial respects from the alleged neutral phenomenal evidence with no implications about the world out there. As Wittgenstein, Austin, or McDowell taught us, apart from dubious and scepticism-infected requirements such as *infallibility of evidence,* or considerations such as *argument from illusion*, it is not clear why we should buy from the sceptic the phenomenal conception of evidence for our empirical claims (see Austin 1962; Wittgenstein 1969; McDowell 1994).

In fact, (A) what would normally count as evidence for empirical claims is hardly confined to records of internal appearances, which, anyway, are rarely cited as evidence. This holds for everyday life as well as rigorous practices of science, where evidence is, paradigmatically, intersubjectively checkable. Consequently, empirical evidence, as normally conceived of, is ex-

[16] See Byrne (2004). Byrne himself appeals to McDowell (1994). In my view, this line of response to sceptical challenges was pursued by Wittgenstein (1969) and Austin (1962). More recently, except of Byrne, Leite (2009) offers an interesting version of this tactics. In his already classic discussion of scepticism, Williams (1996) urges a rather similar approach: basically, to expose sceptical arguments as resting on theoretical commitments that are, despite initial appearances, far from being intuitive and uncontroversial (he identifies what he calls *epistemological realism* as a crucial and highly controversial sceptical commitment). Clearly, if sceptical commitments are not *our* commitments, we are free to ignore them. A careful study of sceptical arguments from underdetermination is Vogel (2004), who also usefully classifies possible responses to them.

pressible in claims whose content is far from neutral about the world out there. Intersubjectivity also supplies the only "neutrality" that is needed.

(B) Furthermore, evidence is typically asked only for claims deemed unobvious, directly inaccessible or unavailable in a context of inquiry, whereas pieces of evidence are supposed to be directly or unproblematically available in the context – at least compared to what it is expected to be evidence for. Thus, under normal conditions, spontaneous perceptual judgements about medium-sized items in our vicinity are not taken as standing in need of evidence – rather, they serve as possible pieces of evidence for other claims – because we do not take what we so perceive as inaccessible or unavailable to us (see, for instance, McDowell 1994). Also evidence is typically not asked for common-sense certainties like "I am a human being", "I have hands" or "The world has existed many years past", whose role in our belief-systems is more accurately described as that of Wittgenstein's metaphorical hinges around which all questioning, doubting and reason-giving turns, since they set standards of what in a given context tests what.

(C) Empirical evidence does not have to be - and usually is not - infallible or incorrigible. This, of course, hangs in closely with the previous two points, which allowed evidence to expand well beyond the allegedly safe confines of subjective experience. Perceptual judgments may provide perfect evidence by ordinary standards, even though they are fallible and corrigible.[17]

(D) Finally, what normally qualifies as empirical evidence is so heterogeneous and context-dependent that it is simply hopeless to try to reconstruct semantically determined kinds of claims, whose ordering would faithfully reflect invariant evidential dependencies among them.[18] For instance, from the perspective of our ordinary epistemic practices, there is no *a priori* reason to think that claims of type *A* (e.g. direct reports of sensory experience) are essentially non-inferential, that is, potential pieces of evidence upon which claims of type *B* inferentially (e.g. reports about the world) de-

[17] Moore (1925/1959), (1939/1959) claims that such truisms are immune to sceptical doubt. Wittgenstein (1969) develops this idea in greater detail and depth, though he distances himself from Moore's approach in many respects. I explain what I take to be a promising Wittgensteinian position in some detail in Koreň (2013). A useful survey article about evidence is Kelly (2008).

[18] The idea that there is an invariant and objective order of epistemic dependencies between semantically determined classes of claims/beliefs is what Williams (1996) calls *epistemological realism*.

pend, for which, however, distinct evidence is never (in no context) required. As Austin aptly put it: "*Any* kind of statement could state evidence for *any* other kind, if the circumstances were appropriate." Thus:

> I may say, for instance, "The pillar is bulgy" on the ground that it looks bulgy; but equally I might say, in different circumstances, "That pillar looks bulgy"— on the ground that I've just built it, and I *built* it bulgy. (Austin 1962, 116)

All these features characterizing empirical evidence as we normally conceive of it – and possibly others that I have not mentioned – are at odds with the properties attributed to phenomenal evidence, which is held to be infallible, non-inferential and world-neutral, among other things.

The second consideration, then, is closely connected to the first. It is hard to deny that empirical claims or theories are, in general, not entailed by available empirical evidence, if only because they go beyond it in universal generalizations. It is less clear, however, that we should be worried about the possibility of empirical theories being underdetermined by *our* empirical evidence. For one thing, outside philosophical circles, a reasonable attitude would be to say that only claims suggesting specific and contextually relevant possibilities of error – not *ad hoc* hypotheses devised by philosophers for the purpose of accommodating appearances – should be given hearing when we compare and evaluate epistemic standing of our empirical claims and theories. For another, once we feel free to set aside the phenomenal conception of evidence as something we are not compelled to buy, allowing empirical evidence to contain good many "outward-looking" or "external" claims, sceptical hypotheses, even if they are allowed to enter comparison, would be excluded as incompatible with the more liberal empirical evidence.

5. Some responses to *UA* and instructive parallels

The upshot of the exposure-tactics as applied to *CA* is that the phenomenal conception of evidence is not something we are compelled to identify with. The situation with *UA* is a more delicate matter, though, if only because it is difficult to abandon completely the empiricist regulative that scientific theories should be evidentially controlled by observational data. Anyway, given that *UA* construes *phenomena* as external, hence intersubjectively available, the exposure-tactics does not carry over to *UA*. Nevertheless,

a few instructive parallels could occur to us when we start to think about possible responses to *UA*. In this way, I think, a careful discussion of one case can shed light also on the other.

Perhaps the most straightforward reaction to *UA* is that of the *sceptic* (or *agnostic*) who concurs with its negative conclusion and reads the whole argument as effectively compromising not just the epistemological commitment of *SR* but indirectly also the realist tenet behind it, namely that scientific theorizing aims to give a true account of the largely unobservable world. If that is the aim, the sceptic goes on, we are never warranted in holding it to be achieved – or so *UA* teaches us. Yet, we may embrace a different (in this sense, anti-realist) view of the aim of scientific theorizing, which does not seem to have the embarrassing consequence that we can never have epistemic warrant to believe that the aim has been achieved. According to the most influential development of this approach – Van Fraassen's *constructive empiricism* (cf. Van Fraassen 1980) – a reasonable aim of science is to produce *empirically adequate* theories that allow us to infer only true claims about phenomena and their regularities. Accordingly, in *accepting* scientific theories we need not – fortunately – hold them to be true *tout court*. We only need to hold them to be empirically adequate – true, that is, in what they claim about phenomena. And we may be warranted in such modest claims, because they do not transcend phenomena.[19]

Whatever its merits, this agnostic strategy with respect to unobservable facts has not been widely followed. Still, it has an insight in that we can profitably read *UA* as an attempt to reveal a tension between realism and empiricism. Incidentally, it is not without interest that Descartes' methodological scepticism can be interpreted in a similar way – namely as challenging a view of knowledge based on empiricism – be it a naive empiricism of common sense or a more elaborate empiricism animating the post-Aristotelian philosophical and scientific tradition.[20] Empiricist approaches to science emphasize the key role of observational evidence in testing scientific hypotheses and theories. However, if sound, *UA* shows that a seemingly innocuous version of empiricism gives rise to scepticism about realistically interpreted theories as not likely to be true in the light of total observational

[19] Following the lead of Duhem and Poincaré, one may add to this line that a choice of theory is not determined solely by the data and logic, but also by extra-empirical virtues and pragmatic, conventional or social factors.

[20] For an influential interpretation of Descartes in this spirit see Frankfurt (1970).

evidence. That would be pretty bad news to many realists who accept an empiricist approach to scientific knowledge.

The situation, though, is not hopeless. In fact, realists are likely to deny the force of *UA* and its devastating potential vis-à-vis realism, while hoping to retain a version of the empiricist regulative. And they could either question the premise that any theory T has a rival empirical theory T^* or the premise that empirically equivalent theories are equally well supported by total evidence. Neither strategy is without appeal.

One may argue that except for a few interesting or genuine cases of empirically equivalent theories, most other cases cited in expositions of *UA* are just contrived *ad hoc* rivals of no interest to everyday scientific practice, some of them even generated by sterile algorithms of the sort: *T^* is like T in what it says about phenomena but incompatible with T in what it says about unobservable parts of the world*. Furthermore, if interesting cases of underdetermination apply to partial theories T and T^*, their future incorporation (or failure to be incorporated) into a more comprehensive and successful theory could render them distinguishable after all: only one of them may be successfully incorporated into the larger theory, or if both are, the resulting systems might produce different sets of predicted phenomena so that T and T^* won't count as empirically equivalent (cf. Laudan – Leplin 1991; Hoefer – Rosenberg 1994; Okasha 2002). Here we face only the weak underdetermination problem that is not so threatening. Since the strong underdetermination problem arises only at the level of total (or comprehensive) scientific systems accommodating all phenomena that might ever occur in their intended domain – or, even more ambitiously, at the level of complete scientific systems of the world – the first question to ask is what interesting examples there are of such total scientific systems, and the second question is whether one could come up with a plausible pair of empirically equivalent total systems that would not involve *ad hoc* alternatives such as *Evil Demon* or its algorithmic likes. One problem then is that it is hard to provide even one plausible candidate for a comprehensive theory, not to speak of two (cf. Hoefer – Rosenberg 1994; Okasha 2002). Another problem is that it might not even be clear enough that alternative systems are really *rivals* rather than notational variants (cf. Norton 2008). At any rate, if we set aside merely *ad hoc* rival theories that scientists would not dream of taking seriously, there is little reason to think that every theory T has a rival T^* which is empirically equivalent in the sense required for the strong underdetermination problem to arise.

Here, then, is the first parallel with the diagnosis urged by the exposure-tactics applied to *CA*, which also allows us to dismiss *ad hoc* sceptical alternatives as irrelevant from the point of view of our ordinary epistemic practices.

Furthermore, one may challenge as inadequate the positivist view of the epistemic structure of scientific theories implicated in the definitions of observational basis and empirical equivalence of scientific theories. Arguably the crucial aspect of this view is the dichotomy of observable and unobservable items itself reflected in the dichotomy of observational and theoretical claims. Now, many critics argue that there is no *a priori* demarcation criterion – and actual scientific practice does not suggest any – separating observable from unobservable items. Admittedly, some items are detectable by unaided senses, while others we can detect only with aid of more or less sophisticated instruments – starting with glasses and ending with quantum microscopes.[21] Yet scientists would not hesitate to say of the latter that they are observable – though a good deal of background theory is implicated in detection-procedures – provided that detection-procedures have been established as reliable.[22] Also, *what we observe/perceive* seems influenced or shaped to some extent by what we expect from the world around us, where collateral expectations may well be more of a "theoretical" than "observational" sort.[23] Now, if on the right track, observations of this type suggest that the dichotomy of observational and theoretical distorts rather than illuminates actual scientific practice.

This points to the second parallel with the diagnosis of *CA* in the spirit of the exposure-tactics: intrusion of *prima facie* theoretical elements into the evidential basis of empirical/scientific theories undermines the foundationalist schism shared by *CA* and *UA*: viz. that there is a theory independent evidential basis of non-inferential data-claims (about experiences and observations respectively) upon which theoretical/empirical claims asymmetrically and inferentially depend for their support. Now, once the observation/theory dichotomy is discredited, a potentially much more extensive and contextually flexible evidential basis is available to scientists – compared to the austere positivist basis confined to purely observational claims – pos-

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[21] This is the basis of a famous argument by Maxwell (1962).

[22] I can only recommend Hacking (1983) for many fascinating examples.

[23] Two classic works – both emphasizing *theory-ladenness* of observation– are Hanson (1958) and Kuhn (1962/1996).

sibly including assumptions about the existence of underlying causes or about existence and reliability of (say, causal) explanations,[24] or even theoretical analogues of *hinge-propositions* exempt from empirical test.[25] If we put this together with the reservations already expressed, the threat of underdetermination of theories by data becomes even more moot.

The last and related point that I want to mention is that opponents of *UA* may complain that it rests on an overly simplified conception of empirical support for scientific theories/hypotheses – basically, on a simple version of the hypothetico-deductive model attacked already by Duhem. Once we recognize this inadequacy, it is doubtful that, in general, empirically equivalent theories are equally well supported by the totality of possible evidence, even if we grant the premise stating the existence of empirically equivalent rivals. Some critics would say that *theoretic-explanatory virtues* such as simplicity and systematicity might evidentially distinguish empirically equivalent theories, provided there are any genuine and interesting examples of such theories. Others would point out that we do not even need to appeal to such 'super-empirical' virtues in order to show that the same data might support empirically equivalent theories to a different degree. Various theories of inductive support or confirmation can be invoked for this purpose (cf. Norton 2008), prominent among them being the *Bayesian* confirmation theory that emphasizes the possible impact of different prior probabilities assigned to rival theories/hypotheses in addition to updating of probabilities conditional of new pieces of evidence (though notorious problems concerning arbitrariness of prior probabilities may diminish its appeal). Eventually, one may want to differentiate between genuine prediction and mere accommodation of available data. Thus, hypotheses explicitly devised to accommodate the data are not supported by them – certainly not to the same degree as when an independently motivated hypothesis turns out to predict them.

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[24] See a useful discussion of this point in Bird (2007).

[25] Lakatos (1970) seems to come close to recognizing something like *theoretical hinge propositions* when he talks about research programmes having „hard core" of unrevisable principles. Similar ideas, of course, have been present already in pragmatist and conventionalist approaches. Poincaré (1905/1952) is particularly interesting in this respect, as he claims that originally experientially based truths might gradually harden into principles. Whether or not such propositions are allowed to feature in the empirical evidential basis, they might be held to structure it in the sense that they help to determine what to test in the light of what pieces of evidence. This has parallels in Wittgenstein's category of hinge propositions (Wittgenstein 1969).

6. Conclusion

There are no doubt other options. Suffice it to say, by way of conclusion, that the very last line of response to *UA* available to realists has no analogy with the exposure-tactics applied to *CA*, though nothing in the latter precludes similar factors to play a role in governing and determining empirical theories. But the absence of analogy here is not so surprising, since this specific strategy vis-à-vis *UA* usually aims to show how we can hope to block the threat of underdetermination of theories by data, *even if* we accept that data are purely observational. As a matter of fact, analogous anti-sceptical strategies have been proposed for *CA* but they are of a completely different kind than the exposure-tactics urged in this study. And their prospects – in my view – are not so bright. One of them, for instance, attempts to show that even if we grant the foundationalist conception of phenomenal evidence, our best empirical theories might still be reasonably taken as approximating the truth about the world out there, because they provide the best explanation of patterns of phenomenal evidence (cf. Vogel 1990). This, obviously, is not the way of my favourite exposure-tactics, which challenges the very idea of purely phenomenal nature of ultimate empirical evidence that arguably underwrites the foundationalist schism.

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