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Tracking the Real: Through Thick and Thin

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ABSTRACT

In this paper, I examine Azzouni's tracking requirement and its use as a normative constraint on theories about objects which we take as real. I focus on what he calls 'thick epistemic access' and argue that there is a logical–conceptual sense in which thick access to the real *presupposes* thin access to it. Then, I move on to advance an alternative—Sellarsian—way to ontic commitment and show that (a) it is better than Azzouni's, and (b) it can accommodate thick epistemic access as a bonus. Finally, I try to defend the Quinean theoretical virtues against some of Azzouni's objections.

1 Introduction

Azzouni ([2004]) invites us to consider the way we ought to form beliefs about what we take to be real. Beliefs are the products of epistemic processes (e.g., observation or inference), but the processes that Azzouni recommends should meet his 'tracking requirement': they should be 'sensitive to the objects *about which* we claim to be establishing [the truths we are committed to]' ([2004], pp. 371–392). Azzouni ([2004], [1997]) claims that the tracking requirement is met by what he calls 'thick epistemic access' and, in particular, *observation*. Thick epistemic access is defined as follows:

> Any form of epistemic access which is robust, can be refined, enables us to track the object [. . .], and which (certain) properties of the object itself play a role in how we come to know (possibly other) properties of the object is a *thick* form of epistemic access. ([1997], p. 477)

With this kind of access to the real (*thick*), he contrasted what he called 'thin' epistemic access. This is taken to be access to objects and their properties via a *theory*, which is (holistically) confirmed and has the Quinean virtues (simplicity, familiarity, scope, fecundity and success under testing). The friends of thin access would have it that 'if a theory has these virtues, we have good (epistemic) reasons for adopting it, and all the posits that come with it' ([1997], p. 479).[1] But Azzouni is no friend of thin epistemic access. His prime claim is that 'thin epistemic access' fails to secure commitment to the real.

In this paper, I examine critically Azzouni's tracking requirement and its use as a normative constraint on theories about objects which we take as real. In Sections **2** and **3**, I argue there is a logical–conceptual sense in which thick access to the real *presupposes* thin access to it. Then I move on (in Section **4**) to advance an alternative—Sellarsian—way to ontic commitment and show that (a) it is better than Azzouni's, and (b) it can accommodate thick epistemic access as a bonus. Finally, in Section **5**, I try to defend the Quinean theoretical virtues against some of Azzouni's objections.

2 Theoretical irrealism vs holistic realism

Azzouni ([2004]) aims to occupy a middle position between what he calls 'theoretical irrealism' and what one may call *holistic realism*. Holistic realism relies on confirmational holism: in so far as a theory is confirmed by the evidence as a whole, ontic commitment accrues to whatever is posited by this theory—and hence to its unobservable posits. Quine's views are taken to be the *locus classicus* of holistic realism. But, according to Azzouni, far from being realist, this view slides towards 'idealism' ([2004], p. 377). There is no reason, he thinks, to believe that thin epistemic access, via theory-confirmation, meets the tracking requirement. The Quinean virtues of well-confirmed theories fail 'to track the properties of the objects such theories are about' ([2004], p. 378). 'Theoretical irrealism', on the other hand, *denies* the existence of unobservable entities. It bases this denial on the claim that while some epistemic processes (those that rely essentially on observations) track observables and their (observable) properties, other epistemic processes (those that rely essentially on the confirmation of theories) *fail* to track unobservables (if there are any) and their properties. Hence, theoretical irrealism honours the tracking requirement but restricts it to observable entities and the epistemic processes which rely on observation.

Here is where Azzouni himself comes in. In so far as he thinks that confirmational holism does not meet the tracking requirement, he is in full

[1] In his ([2004]), Azzouni avoids the expression 'thin epistemic access' to the real. However, the terminology is useful and I have decided to keep it.

alignment with the theoretical irrealist. Yet, where the theoretical irrealist *stops* at the claim that observable entities are real since they are capable of being thickly tracked, Azzouni argues that the tracking requirement can be met by (instrument-based) epistemic processes that give access to at least *some* unobservables. So Azzouni wants to block theoretical irrealism and defend some form of scientific realism.[2] How is this? As we have seen, Azzouni takes observation to be an exemplar of thick access to the real. He says ([1997], p. 477): 'all observations of something are thick.' But for him, instrumental interactions with 'theoretical objects' also provide thick epistemic access. So, he suggests ([2004], p. 384) that he has a sufficient condition for commitment to the reality of at least *some* of the theoretical entities posited by a scientific theory:

> [. . .] *since* thick epistemic access (. . .) meets the tracking requirement, we can take the theoretical entities (which we have thick epistemic access to) to be real for the same reasons and on exactly the same grounds as we can take observational entities to be real.

That's the big picture. And it is certainly an interesting one. Yet, I think the situation is a lot more complicated. What needs to be stressed is that both the theoretical irrealist and Azzouni take it that the tracking requirement is 'normatively constraining access to the real' ([2004], p. 000). They disagree on exactly what judgements of ontic commitment this requirement licenses. But *if* it turns out that there is a sense in which thick epistemic access to the real requires *thin* epistemic access to it, then Azzouni's attempt to contrast the two kinds of access collapses. And this is what, I think, happens to be the case.

Let me make clear what I will be arguing for. I do not want to deny that there *can* be thick epistemic access to the real. But I do want to stress that (a) Azzouni concedes far too much to the theoretical irrealist because he accepts one of her main presuppositions (or rather: prejudices); and (b) Azzouni (as well as the theoretical irrealist) fails to see that thick epistemic access requires the *confirmation* of relevant theories.

3 On the epistemic authority of observation

Let us concentrate on observation, though what I shall say is also relevant (even more so) to instrument-based thick access. Consider a claim that there is

[2] Perhaps Azzouni intends to defend some form of *entity* realism. In my ([1999], pp. 255–8) I drew a distinction between a *thin* version of entity realism and a *thick* one. The coherence of the thin version, which just posits a bear entity, is questionable as this entity will be a Lockean I-do-not-know-what. Unless it has properties, it cannot be identified—or manipulated. The thick version of entity realism is certainly more interesting. For now, it is not just an entity that is being posited, but an entity with substantive properties in virtue of which it plays its causal role. The result, however, is that the distinction between entity-realism and theory-realism is no longer absolute and robust. Even if thick entity realism does not yet yield realism about *grand* theories, it still shows that commitment to entities and commitment to (at least some) theories about them go hand in hand.

a chair next door. Relative to the evidence I now have, this claim is a hypothesis. It is a hypothesis about an observable entity, to be sure. But it is a hypothesis, nonetheless. So I need to confirm it. Isn't the issue settled by just *looking* at the chair? And isn't this an exemplar of thick epistemic access? Isn't it then otiose (or pedantic) to talk of *confirmation* is this case? Surely, one might say, the relevant epistemic process (i.e., looking) is sensitive to the presence and the properties of the chair and that's the end of the matter.

But how do we get to the end of the matter? Observations rely on theory in two ways. One is the standard way: all observation is theory-laden. But neither I nor Azzouni take issue with this. The *other* way in which observations rely on theory has been made prominent by Sellars' ([1956]) attack on the *Myth of the Given*. This way is *very* relevant to the normative status of observation. Undoubtedly, observation is one of the main processes by which we form beliefs. But an observation cannot have *epistemic authority* unless it can be evaluated as correct or incorrect. This evaluation will, inevitably, depend on theories of many sorts. They will be theories about why direct observation is, generally, a reliable way to find out things about entities like chairs and stones; theories about when an observer is a reliable indicator of her environment, and so on. Theories will not only tell us when the observation has epistemic authority; they will also (if sufficiently developed) *correct* observations. As we move away from rudimentary observations of middle-sized objects, these theories will be more complicated and demanding. They will be theories about how an instrument works, and why it is a reliable indicator of some things. More importantly, they will be theories about how what is observed using the instrument is correlated with (or is caused by) some properties of the unobservable entity that is detected by the instrument. It is beside the point that these theories hardly ever function as *explicit* theories in our everyday undertakings. They are mostly internalised by the observers as they learn to use instruments, to calibrate them, to draw conclusions based on the data they collect using them, and so on. Even so, they are presupposed for the evaluation of observation and hence for its epistemic authority.

In fact, things are even more complicated. For, what is *really* observed in relatively sophisticated scientific contexts (e.g., in the context of an experiment) is not the phenomena themselves but *data*. This distinction between data and phenomena is an all-important one and has been rightly stressed in a seminal paper by Bogen and Woodward ([1988]). Strictly speaking, observations are of data (e.g., temperature readings, or clicks, or pointer readings).[3]

[3] Though true, this is overstated: science can proceed without any observations at all, if by 'observation' we mean something akin to having a (conscious) observer perceiving (or experiencing) the data. As Feyerabend ([1969]) noted along time ago, science without experience *is* possible. For more on this, see Fodor ([1991]).

The phenomena (e.g., the melting point of a substance or the path of a planet) are abstracted from the data by means of a number of sophisticated techniques based on a rather substantive set of assumptions and theories. When a phenomenon is established (e.g., that lead melts at 327 degrees centigrade or that Mars describes—approximately—an ellipse), there is no strict observation of the phenomenon itself. Nor can it be said, in a straightforward manner, that the epistemic processes we use to establish (and come to know) this phenomenon track the relevant properties of, say, lead (or of Mars). The datum *327 degrees centigrade* might not even be within the data points that are collected by measurements of temperature.

Typically, what happens is the following. Scientists collect data and use methods such as controlling for possible confounding factors, data-reduction and statistical analysis to evaluate them. They make assumptions about, and try to check, the reliability of the equipment involved in the experiment. They make assumptions about the possible sources of systematic error and try to eliminate them (or correct the data in their light). They also make assumptions about the causes of variation in the data points they collect. Only when all this is in place can there be a reliable *inference* from the data (or a subset of them) to the phenomenon. All this is explained in great detail by Bogen and Woodward ([1988]). The relevant point here is that the establishment of the epistemic authority of what is normally called the observable phenomenon (e.g., the melting point of lead) is a rather complicated process which essentially relies on background theory. If all these background theories somehow fail to be adequate (or well-confirmed, I should say), the observed phenomenon is called into question. Now, this does not imply that before we establish, say, the melting point of lead, we need detailed theories of *why* lead melts at this point. These theories will typically be the product of further theoretical investigation. But it does imply that establishing the reliability (and hence the epistemic authority) of the data as a means to get to stable phenomena relies indispensably on some prior theories. So, observation is not epistemically privileged *per se*. Its epistemic privilege is, in a certain sense, parasitic on the epistemic privilege of *some* theories.

Azzouni might have a reply to all this. In his ([2000]), he argues that the application and empirical testing of theories relies on a body of 'gross regularities' that function *independently* of any scientific theory (in the sense that they cannot, at least in full, be appropriated by any scientific theory). These gross regularities have a life of their own, as it were. Some of them are 'articulated' while others are 'unarticulated': they amount to rather detailed knowledge-how which cannot even be expressed propositionally (*cf.* [2000], pp. 63–8). Azzouni takes observation as well as instrument-based probing to rely heavily on such gross regularities. The most mundane cases concern the middle-sized objects (where the gross regularities concern, e.g., the

accessibility and the relatively stable behaviour of these objects). The most exciting cases concern viruses and sub-atomic particles (where the gross regularities concern, e.g., patterns of behaviour of the particles, or connections between their properties and certain observable behaviours—*cf.* [2000], pp. 116–7). So he might well argue that observation (as well as instrument-based probing) is, after all, epistemically privileged precisely because it relies on such gross regularities which are, by and large, independent of theory.

I am willing to grant, for the sake of argument, these gross regularities. I could easily argue that Azzouni overstates his case for these gross regularities, but nothing hangs on this at present. For I think that Azzouni's appeal to these gross regularities creates more difficulties for his tracking requirement (as a *normative* constraint) than he thinks. The reason is simple. Even if most of the theories mentioned a couple of paragraphs above as part of my claim that observation relies on theory for its normative import are taken to capture gross regularities (which they do not), these theories will be ordinary and, sometimes, elementary *inductive* generalisations. The facts (regularities—gross or not) reported by these generalisations cannot be accessed in a thick way. I do not think any general fact can be accessed in a thick way (in Azzouni's sense). So the commitment to these facts can only be the outcome of *thin* access to them: our beliefs about them have been confirmed, and this gives us reasons to accept as true the generalisations that report them.[4]

By now, the conclusion I want to draw will be obvious: there are no self-authenticating observational epistemic processes. If, then, their epistemic authentication has to come from theories, all these theories have themselves to be authenticated, which, in this context at least, means *confirmed*. If confirmation, even if rudimentary, of all these theories is inescapable before we start taking seriously the idea of an *epistemically authoritative* thick access to the real, then there is a sense in which *the very possibility of thick epistemic access presupposes (conceptually) thin epistemic access*. For, by hypothesis, confirmation gives us only thin access to the real. So thick access rests on a (conceptually and epistemically) prior *thin* access; that is, it rests on commitment to whatever the relevant confirmed theories posit (including relevant regularities).

Let me summarise. It does not *really* matter whether we have thick or thin epistemic access to whatever is posited by our best theories of the world, provided (a) that what is posited has what it takes to be real, and (b) that our epistemic processes are such that we end up with correct beliefs about

[4] The same argument applies to Azzouni's claim that 'background perceptual regularities' have 'proved immune to epistemic corrosion' ([2004], p. 387). This is certainly correct. But it does *not* follow from this that they get their epistemic authority independently of theory.

these posits, that is, beliefs which resonate with what these posits are and what properties they have. On some occasions, the theory itself (perhaps in conjunction with other theories) may lead us to expect that a certain posit can be tracked by observations and instruments. But if the theory does *not* entail this for a posit, which is nonetheless explanatorily useful, this is *no* reason to suspect this posit. For our beliefs may track it not through observations and instruments but in an alternative way.

4 The myth of the levels

What, then, is this alternative way? I want to motivate a view that challenges the basic assumption shared by theoretical irrealists and Azzouni, viz., that commitment to observables is (should be) grounded on the epistemically privileged status of observation. (Let me remind the reader that Azzouni extends this claim to some unobservables by modelling instrument-based tracking on observations.) I take my cues from Quine and Sellars. Despite their many differences, they both argued for two important theses. First, the theoretical-observational distinction is not ontological but rather methodological. We are not talking here about two distinct senses of 'real', a theoretical existence and a non-theoretical one. There is just one sense of 'real' at play. Second, the theoretical-observational distinction does not mark an ultimate epistemic difference either. It is not that observational claims can be known to be true, whereas theoretical claims cannot. It is not even that epistemic methods which are different in principle are employed for knowing them.

Quine's ([1960], p. 22) master argument for the reality of molecules and the like is that 'they are on a par with the most ordinary physical objects'. As he explains:

> The positing of those extraordinary things [molecules and their extraordinary ilk] is just a vivid analogue of the positing or acknowledging of ordinary things: vivid in that the physicist audibly posits them for recognised reasons, whereas the hypothesis of ordinary things is shrouded in prehistory. (*Ibid.*)

Epistemically speaking, molecules and chairs are also on a par, given that, for Quine, the evidence that we have is couched in terms of 'surface irritations'. As he ([1955], p. 250) puts it:

> If we have evidence for the existence of the bodies of common sense, we have it only in the way in which we may be said to have evidence for the existence of molecules. The positing of either sort of body is good science in so far merely as it helps us to formulate laws—laws whose ultimate evidence lies in the sense data of the past, and whose ultimate vindication lies in anticipation of sense data of the future.

Quine's move, then, is to challenge the theoretical irrealist with a *tu quoque*: if you doubt the reality of molecules, then you should doubt the reality of the bodies of common sense. Note that Quine does not need to appeal to anything like Azzouni's thick epistemic access, for he takes the issue of how we know what there is to be subsumable under the issue of 'the evidence for truth about the world', the 'last arbiter' of which is 'so-called scientific method, however amorphous' ([1960], p. 23). Sometimes a certain process might directly track the properties of a real object, be it observable or unobservable (*cf.* [1955], p. 253). But this is a bonus, as it were. And it is not as if Quine thinks that thick epistemic access is already in place when we form beliefs about common sense bodies. A rudimentary version of scientific method rules there too. He ([1981], p. 20) says quite explicitly that

> Even our primordial objects, bodies, are already theoretical [. . .]. Whether we encounter the same apple the next time around, or only another one like it, is settled if at all by inference from a network of hypotheses that we have internalised little by little in the course of acquiring the non-observational superstructure of our language.

Sellars' approach is interestingly different from Quine's. On the one hand, Sellars ([1956]) resists the temptation that there are sense-data which can play an evidential role. On the other hand, he ([1963]) offers a master argument for commitment to the unobservable entities posited by scientific theories, viz., that they play an ineliminably explanatory role. In order to formulate this argument, he had to resist what he aptly called the 'picture of the levels'. According to this picture, the realm of facts is layered. There is the bottom level of observable entities. Then there is the intermediate level of the observational framework, which consists of empirical generalisations about observable entities. And finally there is yet another (higher) level: the theoretical framework of scientific theories which posits unobservable entities and laws about them. It is part of this picture that while the observational framework is explanatory of observable entities, the theoretical framework enters the picture by explaining the inductively established generalisations of the observational framework. But then, Sellars says, the empiricist will rightly protest that the higher level is dispensable, for all the explanatory work vis-à-vis the bottom level is done by the observational framework and its inductive generalisations. Why, then, posit a higher level in the first place?

Sellars' diagnosis is that this picture rests on a myth, viz., *the myth of the levels*. A Sellarsian realist should unveil and reject this myth. Sellars' argument against the myth of the levels is that the unobservables posited by a theory explain *directly* why the individual observable entities behave the way they do and obey the empirical laws they do (to the extent that they do obey such laws). So he resists the idea that the theoretical framework has as its prime

function to explain the empirical generalisations of the observational framework. As he ([1963], p. 121) graphically states:

> Roughly, it is because a gas is—in some sense of 'is'—a cloud of molecules which are behaving in certain theoretically defined ways, that it obeys the empirical Boyle-Charles law.[5]

Sellars claimes that unobservable entities are indispensable because they also explain why observational generalisations are occasionally violated; why, that is, some observable entities do not behave they way they should, had their behaviour been governed by the observational generalisation. A good way to state Sellars' point is by noting that the observational generalisations will be either false or *ceteris paribus*. Think, for instance, of the generalisation that everything that is left unsupported falls to the ground. What about balloons then? Now if these generalisations are false, the theory which is made of them will be *false* too, and hence it will have to be replaced by a truer theory (one, for instance, which explains why most objects fall to the ground but balloons do not). If, on the other hand, these generalisations are *ceteris paribus* true, then again it is theories of the theoretical framework (and their concomitant positing of unobservables) that explain the circumstances under which they hold strictly and the circumstances under which they fail to hold. In this case too, the theory that is made of *ceteris paribus* generalisations will have to be replaced by another truer theory that explains their limitations.[6]

Sellars ([1963], p. 122) takes it that in so far as theories 'establish their character as indispensable elements of scientific explanation', they also establish themselves 'as knowledge about what *really* exists'. Sellars' thought is quite complex. But, to a good approximation, what he has in mind is that, ultimately, scientific explanation proceeds via the *theoretical identifications* of observable entities with unobservables. The latter not only explain the observable behaviour of some observable entities; they really *are* the things which we thought of as independently existing observable entities. There isn't a table *and* a swarm of molecules. There is just a swarm of molecules. It is not puzzling, then, that we should be committed to unobservables. That is the only thing we *can* be committed to if we want to explain, and come to have *true* beliefs (and not just *ceteris paribus* observational generalisations) about, the entities which populate the observational framework.

A very perceptive reader for this journal levelled a crucial objection at this part of my argument. S/he noted that my use of 'commitment' is ambiguous. And s/he added: 'To make the argument go through, it needs to be understood

[5] The sense of 'is' that Sellars has in mind here is explained in his ([1956], pp. 314ff.). It is the 'is' of theoretical identification.

[6] This point is also made vividly by Hempel ([1988]).

in a *de re* sense. But it is very doubtful that ontological commitment is a *de re* propositional attitude! It seems, rather, to be a *de dicto* attitude, which would nullify the argument here made by the author, since substitutivity of identicals cannot be assumed.' By way of reply, I should note that there is certainly a sense in which 'commitment' to an entity could be understood as a propositional attitude: when, for instance, we read 'S is committed to x' as 'S believes that there are x'. (See van Fraassen [2000], pp. 1657–8.) Yet I am not sure that it is *useful* to understand 'commitment' to an entity as a propositional attitude. And if it is not, I am no longer sure why commitment to an entity could not be *de re*. The relevant literature is immense and interesting (and I thank the reader for making me look into it in some detail). But there is a way for my argument to proceed that *seems* unobjectionable. As I found out, I prefer to follow Michael Jubien's ([1972]) formulation of ontic commitment: T *assumes* x, where T is a theory and x is *something else* (e.g., an entity) and the ontic commitment is a relation between a theory and this something else. Now there are quite a few problems with this proposal. But as Jubien ([1972], pp. 381–2) has shown, there is a clear case in which the substitutivity of identicals *can* be assumed, so long as the theory *itself* provides the ground for the substitution. Suppose that T assumes a. If $a = b$ is part of T (e.g., a theorem of T), then it *can* be inferred that T assumes b. It should be clear that this fits Sellars' proposal (and my argument) perfectly, for the required theoretical identifications are part of the theory. To press the example above, since it is the theory itself that identifies tables with swarms of particles, if the theory assumes swarms of particles, it also assumes tables.[7]

To sum up. We start, perhaps, unreflectively, with the idea that the observational framework consists of a domain of *sui generis* objects, to which we have perceptual access. But then we come to realise that we have been held hostage to the myth of the levels. And we come to accept that the so-called observable entities are, in some sense, 'constructs'. From all this, it does *not* follow that we cannot have observational access to the so-called theoretical entities. Quite on the contrary. If the theoretical identifications hold, then such access is guaranteed. Indeed, Sellars' attack on the myth of the given was based, partly, on his claim that theoretical concepts and theoretical terms can gain a 'reporting role'. Nor, of course, does it follow that all so-called theoretical entities will be identified with observables. Still, their indispensable role in explaining the selected phenomena of which the theory is a theory is enough to make us say that they have what it takes to be real.

There is certainly a sense in which Sellars can honour Azzouni's call for thick access to the real. Indeed, Sellars insists that when one learns a scientific

[7] There is a residual problem: a theory cannot assume anything that does not exist. Equivalently, in the locution 'T assumes a', 'a' should designate an entity. I am not sure how damaging this objection might be to the argument offered in the text.

theory, one learns to 'tell by looking' that certain theoretical states of affairs obtain ([1977], p. 183). He takes it that 'a theoretical framework can achieve first-class status only if a proper subset of its expressions acquire a direct role in observation' (*Ibid.*). But it is not as if this is a *criterion* of ontic commitment.[8] Rather it is more of a methodological criterion which links the theoretical framework to intersubjective perceptual responses. Yet there is also a sense in which the Sellarsian approach shows the limits of thick access. For, to say the least, there may well be further theoretical identifications between different theoretical entities, which are ontically committing, because relevantly explanatory, without directly satisfying the tracking requirement.

What follows from the Sellarsian approach is that good theories go hand in hand with existence claims and knowledge of them.[9] And 'good theories', as Sellars ([1963], pp. 117–8) suggests, are those theories which explain directly the behaviour of observable entities by positing unobservables. What, for Sellars (*Ibid.*), makes these theories *good* is that they license an inference from some observational premises to a theoretical conclusion, e.g., that molecules exist. This inference is certainly an explanatory inference; something like an *inference to the best explanation*. Sellars' important spin on this inference is that he weds it to the denial of the myth of the levels: either we have no explanation of what we take to be observable entities and their behaviour (i.e., we just have a *ceteris paribus* inductive systematisation of them) or we have to accept that there are unobservables.

I prefer Sellars' account to Quine's for two reasons. First, Sellars' account yields a more robust realism than Quine's, especially if we see Quinean realism in the light of his overall philosophical views concerning the immanence of truth, ontological relativity and the like. Second, Sellars' account makes more explicit than Quine's that ontological commitment and explanatory indispensability go hand in hand. But we should not lose sight of their important connections, for they both urge us to accept the reality of unobservable entities based on the role that these unobservables play in explaining observable entities and phenomena.

Following Sellars (and Quine), we have an alternative picture of how the real is tracked. Put in a slogan form: the real is tracked via good theories (in Sellars' sense). In fact, we seem to have a better picture than Azzouni's. For

[8] Azzouni ([2004], p. 384) argues that he does *not* take thick epistemic access as a *criterion* for what is real. He takes it to offer only 'a sufficient condition for the existence of something'. But this cuts no philosophical ice. Of course, if something does *not* exist we cannot gain 'thick epistemic access' to it (nor, I should add, thin access, or any access whatever). The hard problem for Azzouni is to explain what kind of access we might gain to things we are committed to (e.g., new types of viruses, as he says) but to which we do not have thick epistemic access (in his sense). If we are committed to them via *confirmed theories*, then his argument against confirmational holism would be considerably weakened.

[9] A detailed defence of this point is given by Churchland ([1979]).

we have a way to argue that what is posited has what it takes to be real. The Sellarsian argument can be seen as supplying an independent argument to support the claim that what our epistemic processes track *is* the real. Besides, the Sellarsian argument reveals the root problem with theoretical irrealism, for the latter rests on the myth of the levels. The Sellarsian argument is precisely that the theoretical irrealist is left with no ontic commitments at all, if she eschews commitments to unobservables, as the supposed observational framework is a *false* theory. Not only is there nothing deeper (like sense contents) for the observational framework to be explanatory of, but also 'there *really* are no such things as the physical objects and processes of the common sense framework' ([1977], p. 174).[10] It is also interesting that the Sellarsian argument can even accommodate Azzouni's call for thick epistemic access—but as a *bonus*, or, perhaps, as a *double check*.

5 Quinean virtues

Before I conclude, let me quickly address a couple of points that Azzouni raises (and which I may be said to have missed).

First, Azzouni ([2004], p. 378) rightly worries about the epistemic credentials of the Quinean virtues. He discusses five of the virtues that Quine has put forward: simplicity, familiarity, scope, fecundity, and success under testing. It is certainly legitimate to wonder whence these virtues acquire their supposed epistemic force. And it is equally legitimate to argue that, since confirmation depends on them, if they are found lacking in epistemic force, confirmation will not carry much epistemic force either. I am quite willing to grant that these virtues enter the Quinean picture as a *deus ex machina*. This, of course, is not to imply that they lack epistemic force. Success under testing surely has some such force. Consider how Quine ([1955], p. 247) introduces 'familiarity' as a virtue: 'the already familiar laws of motion are made to serve where independent laws would otherwise have been needed.' It is not hard to see that a way to read this virtue is that familiarity increases the degree of confirmation of a theory since the so-called familiar laws are already well-confirmed ones. Or consider 'fecundity': 'successful further extensions of the theory are expedited'. Here again, the connection with an increase in the degree of confirmation is not far away. But to say that a factor x has epistemic force for a belief (or theory) y is to say that x raises the probability of y's being true. So there is a sense at least in which the Quinean virtues have epistemic force. This last thought might remove the sting from Azzouni's ([2004], p. 378) claim (a joke?)

[10] This may be misleading. As Sellars explains, these objects do not exist *as they are conceived* by the observational framework.

that the list of virtues might be augmented to include, for instance, 'crass political manipulation'. The latter does not have, even *prima facie*, epistemic force in the sense explained above.

The point remains, however, that the Quinean virtues need more defence. And this point is equally forceful against the Sellarsian view I have been defending since, at least *prima facie*, talk of explanatory theories and talk of virtuous theories go hand in hand. But there is a sense in which the Sellarsian view can escape Azzouni's pressure on the virtues. It is this. To say of a theory T that it offers an explanation of certain observable entities, of their law-like behaviour and of their deviations from this, is to tell a story in terms of the theory's unobservable posits and their properties what these observable entities are and how their law-like behaviour comes about. This (causal-nomological) story is an explanatory theory irrespective of the virtues that it might have. To put it differently, it is the positing of the molecules (with their properties and their causal role) that does the explaining and *not* the virtues of the theory of molecules. The virtues can and do enter the picture in an important but indirect way: as parts of a *theory* of how the first-order explanatory stories told by scientific theories are (should be) appraised. Rock-bottom, as it were, is empirical success, and especially what may be called novel empirical success, i.e., confirmed novel predictions. But it may happen that these are not enough to single out one first-order story because more than one tie with respect to empirical success. It is for this reason that we need to introduce a *theory* of theory-appraisal, whose parts are the virtues. But if we see the problem in this light, then it is open to us to treat this theory for what it is: a *theory*. We then need evidence, that is, empirical evidence, for the virtues that will go into the theory. This evidence can only come from the past record of first-order explanatory stories. Put succinctly, the question we should try to answer is this: what kinds of virtues were possessed by scientific theories which were successful (in the strict sense of also yielding novel predictions) as explanations of certain observable entities, of their law-like behaviour and of their deviations from such behaviour ? It hardly needs stressing that we are far from having such a theory. But, I think, such a theory can legitimise the virtues.[11]

The second point that needs to be touched on is this. I am in broad sympathy with Azzouni's critique of confirmational *holism* (and of its concomitant *holistic realism*, if there is such thing). I think, to be sure, that confirmational holism makes coherentism much more plausible as a theory of justification: it engenders and guarantees some friction with the world, and hence it renders justification not a simple function of explanatory coherence and the Quinean virtues. But in so doing, it can lead (and has led) to excesses. It is absurd, I

[11] I have tried to offer an account of this theory in my ([1999], Ch. 8). I analyse and defend Inference to the Best Explanation in my ([2002]).

think, to say that when a theory is put to the test, the whole science is put to the test. And it may well be problematic to say that when a theory is put to the test the laws of logic and the mathematics are also tested. But an important insight of confirmational holism *vis-à-vis* theoretical assertions is that they are confirmable no less than (*prima facie*) observational ones. As I have argued elsewhere (*cf.* [1999], pp. 125–7), however, the insight that confirmation can go all the way up to the higher levels of theory does not entail the conclusion that *all* theoretical assertions are confirmed, and confirmed to the same degree.

Now, Azzouni devotes his Section II to a challenge of this last conclusion. To be sure, he turns his fire against Philip Kitcher's relevant views. But both Kitcher and I have argued for two theses. First, there is a plausible explanatory argument (the no-miracles argument) that takes one from the fact that theories are successful to the claim that they are approximately true. Second, in buying into this argument, realists need not accept that all theoretical constituents of a theory are confirmed and can be deemed (approximately) true. Kitcher and I draw the line between the 'good' and the 'bad' parts of successful theories differently but we both agree that confirmation is selective and that the theoretical constituents that are confirmed are those that essentially contributed to the successes of a theory.[12] Azzouni has two arguments against this line of thought, both of which are well-known.

The first [(2004), p. 380] is this: 'A set of falsehoods, however, can have true implications (implication isn't falsity-preserving). If, therefore, there are scientific theories composed entirely of falsehoods, but with true (empirically exploitable) implications, then, at least in these cases, attempts to divide the true from the false this way will fail.' This is true, but irrelevant. Realists argue for a rigorous notion of empirical success, which includes *novel* predictions. Given this, it is very unlikely, realists argue, that *all* of the theoretical premises that are employed in the derivation of novel predictions are false. So Azzouni (or anyone else for that matter) should show that there are such theories which cannot be divided into true and false parts.

The second of Azzouni's arguments is this. In typical cases, 'successful theories are evaluated against a background of possible theories where ontological possibilities are rich; and so there is good reason to distrust the inferences from the success of these theories (and the instruments these theories are about) to their truth' ([2004], p. 382). And he adds: '"success to truth" inferences only make sense when the ontological options excluded are meagre' ([*Ibid.*], p. 382). It is not hard to see that Azzouni's point is none other than the old chestnut of the underdetermination of theories by evidence. I do not intend to repeat here the usual strategies that realists follow to block this

[12] For more on the differences between Kitcher and myself, see my ([1999], pp. 111–2).

argument (see my [1999], Ch. 8). My reply, instead, is minimal. Azzouni employs the foregoing argument in order to promote his own thick epistemic access. Presumably, in ordinary observation (where observable entities are involved) as well as in (some) instrument-based tracking, the ontological options are not rich. But who is to decide this? Relative to a sceptical scenario, the ontological options are wildly rich, even when it comes to ordinary observation.[13] Azzouni is surely right when he says that in typical cases of observations (of observables) 'we are severely constrained in the ontological options it is reasonable to even consider' ([*Ibid.*], p. 381). But the emphasis in this claim should surely be on the word 'reasonable'. If, as indeed happens, what options it is reasonable to consider when it comes to observables depends on several background theories and assumptions (e.g., that there must be some physical cause of the extinction of the dinosaurs), then it is surely open to someone to argue that analogous background theories and assumptions make it reasonable to restrict the ontological options when it comes to theoretical inferences from success to truth. In any case, the way Azzouni describes things does not show that theoretical inference from success to truth is unjustified or irrational. It only shows what we already knew, viz., that it is more risky.

6 Conclusion

Let me close by summing up my argument. I have not tried to discredit thick epistemic access as an *external constraint* on our theorising about the world. But I have focused on showing its limitations as a way to cash in Azzouni's *normative constraint* to the real, viz., his tracking requirement. Briefly put, my argument has been that (a) thick epistemic access to the real requires *thin* epistemic access to it, if it is to be taken as epistemically authoritative; and (b) the real is tracked via good (i.e., explanatory in Sellars' sense) theories. That is, the real is not tracked just by observations and instruments. Azzouni is right to warn us against the *excesses* of confirmational holism. But in elevating the tracking requirement to a *normative* constraint to our access to the real, he might throw away the baby with the bath water.

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[13] Azzouni ([2004], p. 386) sets aside the issue of global scepticism. It should be noted, however, that I am raising the spectre of scepticism only to make vivid a point that I take to be obvious, viz., that what ontological options are taken as live (and hence whether the ontological options are rich or poor) depends on certain assumptions that are being made and on the relevant context. Other, less sceptical, stories could bring this point home.

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