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Underdetermination and Evidence

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1. Introduction: Sceptical Underdetermination Arguments

Our beliefs in scientific theories, along with many other beliefs, are inferred from evidence. Let us distinguish then between *evidence* propositions, whose content is the content of our evidence, and *conclusion* propositions, whose content is the content of the beliefs inferred from the evidence. Pre-philosophically we like to think that the conclusion propositions are justified by being inferred from the evidence propositions. Indeed we like to think that such inferences sometimes give us knowledge of the truth of the conclusion propositions.

Amongst philosophers, however, it is widely thought that truth of the conclusion propositions of scientific arguments is in some important sense underdetermined by the evidence propositions. Two sorts of consideration are adduced in support of the underdetermination thesis. The first is a *quantitative* argument. It says that inevitably there is too little evidence to determine a single conclusion proposition. For any set of evidence propositions and any conclusion proposition there will always be incompatible possible conclusion propositions not excluded by the evidence. While extending the set of evidence propositions will exclude some of these incompatible possibilities, no extension that is available to us will exclude all but one. Inductive scepticism typically starts with this sort of consideration. It is said that

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conclusion propositions—universal generalizations—entail infinitely many singular propositions, but our evidence consists only ever of finitely many singular propositions.

The second consideration asserts that there is a *qualitative* difference between the evidence propositions and the potential conclusion propositions, so that the former have no handle on the latter and cannot decide between them. To take an extreme example, if evidence could only consist of mathematical propositions we would think that our evidence underdetermines conclusions concerning organic chemistry. We would think this not because we could never get enough evidence to fix upon a conclusion but because the evidence, however extensive, is all of the wrong kind.

In this chapter I shall address the question of underdetermination as based on the qualitative consideration, and shall assume that a satisfactory answer is available to the quantitative problem. Arguments based on qualitative underdetermination form an important subset of sceptical arguments. Arguments purporting to show that knowledge of the external world is not to be had rest on the assumption that our evidence is comprised of our subjective sense-impressions (or our reports of them), in contrast to our conclusions, which concern the existence of non-subjective, physical objects. However extensive our set of sense-impressions may be, so the argument goes, the content of that set is too restricted in its nature to allow inferred knowledge of physical objects in the external world. There is no rationally licensed inference from evidence about what is internal to conclusions about what is external.

Another sceptical underdetermination argument has the same form but targets inferences with different evidence and conclusion sets. It is an argument characteristic of the sceptical element of empiricism. Many theories of interest concern entities of a kind that cannot be observed—subatomic entities, novel forces, genetic information, and so on. According to this view our evidence is all observational. So theories of these kinds go beyond the evidence in a qualitative sense. For every theory that does so there are other theories that are empirically equivalent—they have the same observational consequences—but which differ with respect to their theoretical claims. Hence our evidence underdetermines theory. The sceptical empiricist argument will be this, or something like it:

- (OBS) all evidence is observational;
- (INF) from observational premises only observational conclusions may be rationally inferred;

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therefore

(SCEP) only observational propositions can be known.

The central aim of this chapter is to show that this argument, and by extension any argument like it, fails. The strategy is as follows. In Sections 2–3 I consider the concept of evidence, endorsing Williamson’s view that all and only knowledge is evidence, ($E = K$). In particular I consider the alternative view that only observational knowledge is evidence. In Section 4 I use this to show that the use of (OBS) in the above argument is question begging. Section 5 is an aside concerning inferences across the observational/non-observational divide which shows the falsity of (INF). In Sections 6–7 I consider repairs to the sceptical argument. In Section 8 I consider the lesson of all this for Bas van Fraassen’s constructive empiricism. Some commentators employ a caricature of constructive empiricism that takes it to employ the same argument. In fact van Fraassen is careful to distance himself from scepticism about theories and to take constructive empiricism to be a view about the aim of science. Nonetheless, I shall argue that van Fraassen *ought* to be committed to the sceptical conclusion, and that constructive empiricism is implicitly committed to the above argument. For the time being we may note that van Fraassen (1980: 18–19) is committed at least to what will be the contended first premise of the argument, that all evidence is observational.

2. Evidence

Let us look at the first kind of assumption, that our evidence is only of a certain kind. We might adduce objections to particular views of what evidence is. So for example we might complain against a positivist conception of evidence as (reports of) sense-impressions, that our everyday concept of evidence allows us to include facts very remote from sense-impressions as evidence. The iridium spikes in certain geological strata are taken as evidence for the meteor theory of dinosaur extinction. Even if false, that theory is not refuted by pointing out that iridium spikes are not sense-impressions (and so, allegedly, not evidence).

But of greater interest are more principled arguments for—and against—the idea that evidence is always of some certain kind. It may be held that we must know what our evidence is and that only sense-impressions allow evidence to fulfil this role. But *nothing* is such that if it is X we must know that it

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is X (Williamson 1996). Something may be X but so close to not being X that we are unable to distinguish it from a nearby case of not-X. In such circumstances, that thing would be X but we could not know it is X, thanks to the principle of safety. (Safety requires that for p to be known, p cannot be false in very nearby cases (Williamson 2000: 123–30).) So, in particular, sense-impressions are not special, in that we always know what our sense-impressions are. And, in general, the requirement that we know what our evidence is cannot be fulfilled, whatever evidence is taken to be.

If we reject the thought that we must know what our evidence is we are giving up a certain sort of foundationalism, the kind that says that our epistemic foundations must be self-intimating. But if we give up on the latter, there is no reason to think of our evidence as being of a certain *kind*. What then ought we say about our evidence? Timothy Williamson (1997; 2000: 184–208) holds that our evidence is what we know.

I shall not consider Williamson’s argument here. Instead I shall articulate an independent motivation for the equation and also sketch an argument against a particular counter-proposal. The independent motivation concerns our concept of knowledge. The proposal is that evidence propositions are all and only those that can be used as premises in a knowledge-generating inference. This captures the idea that we argue *from* evidence to a conclusion, evidence is where we start our cognitive journey. That, of course, is consistent with the thought that the evidence itself had to be argued for and is the conclusion of some prior argument. But that argument had better be sufficiently good for its conclusion to be evidence for some further proposition. Furthermore, the proposal explains why we value evidence—because it can give us knowledge (when used in conjunction with a good enough argument). Evidence is more valuable than mere belief. The proposal can be broken down into two elements:

- (EI) evidence must be capable of being the premise of a knowledge-producing inference;
- (IE) if a proposition can be the premise of a knowledge-producing inference, then that proposition is amongst one’s evidence.

(EI) shows that this conception requires all evidence to be knowledge. If some proposition is not known but plays a non-redundant role in an inference then the conclusion of that inference cannot be knowledge either. And (IE) shows that all knowledge is evidence. Let p be some proposition that is known, from

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which we infer some q by a simple process of inference of known reliability. Hence q will be known. No assumption was made about p other than that p is known. So all propositions that are known can support knowledge-producing inferences, and so by (IE) all propositions that are known are evidence.

As regards (EI) there will be those who think that something less than knowledge will be sufficient for evidence, even that mere belief will be. But that fails to explain why inconsistency with one's evidence is bad news for a theory in a way that inconsistency with one's beliefs *simpliciter* is not. Indeed the fact that inconsistency with evidence is sufficient to falsify a theory shows that an evidence proposition must be true. People may employ their (false) beliefs as evidence, as the basis for their inferences. But to use a belief as evidence does not make it evidence. Since the status of evidence is not self-intimating, one should expect people to be mistaken on occasion about what their evidence is. Austin (1962: 116) points out that 'any kind of statement could state evidence for any other kind, if the circumstances were appropriate.' A fortiori any statement can state evidence, in appropriate circumstances. While this does rule out limitations on evidence to certain kinds of contents (for example, sense-experience, observations, and so on), it does seem to point to a contextual, pragmatic conception of evidence. It is then a short step to the thought that in a context where all parties in a dispute agree in their belief in proposition p but disagree as to whether q , then, in that context, p is part of their evidence but q is not. I have already pointed out that to use p as evidence does not make p evidence. At the same time the correct observation that q is not evidence does not require a pragmatic conception of evidence. Since some parties do not believe q they do not therefore know q ; hence q is not part of the group's shared knowledge, and is thus not part of their shared evidence. In so far as the dispute is a collective enterprise to resolve some question so that all parties come to knowledge of the conclusion, that dispute must start from shared evidence not just the evidence that some have. To put the matter another way, the argument I present to you must start from *your* evidence, not simply for the pragmatic purpose of convincing you but also for the epistemic purpose of bringing you into a state of knowledge concerning the conclusion. It is true that evidence varies from context to context, but that is not because the pragmatics of argument and investigation differ according to context; it is because what we know differs. (A hermit scientist can have evidence despite having no interlocutors; but not everything he believes is necessarily amongst his evidence.)

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In any case, the thought that something *less* than knowledge can be evidence must weaken the power of a sceptical, qualitative underdetermination argument. The more kinds of proposition that can be evidence, the less room there is for saying that our evidence is not of a kind that supports the inferences in question. In a society of realist scientists various theoretical propositions might be among the shared beliefs. If that were sufficient for evidence, then the sceptical underdetermination argument we saw above will not be sound, since the premise (OBS) that all evidence is observational will be false.

A greater challenge is therefore presented by the possibility that knowledge is *insufficient* for evidence, and hence that (IE) is false.

3. Evidence as Observational Knowledge

A counter-proposal against (IE) is that evidence is the non-inferential knowledge, back to which a chain of (knowledge-producing) inferences may be traced. Evidence stands at the beginning of a chain of inferences; the intermediate propositions are not themselves evidence. To this it might be added that all non-inferential knowledge is observational, whence we reach the conclusion that all evidence is observational.¹

Both parts of this claim are false. To start with the latter half, not all non-inferential knowledge is observational. Non-inferential a priori knowledge provides an obvious counterexample. Innate knowledge might provide another. We might well doubt whether all non-inferential knowledge is observational, even if we restrict our attention to a posteriori knowledge. Take testimony for example. Knowledge that we have as a result of testimony is not observational. Nor is it typically inferred knowledge. However, let us suppose that knowledge from testimony is inferred. There are accounts, such as Hume's, that regard knowledge from testimony as inferred from beliefs about the reliability of the source and about the contents of their utterances. Such accounts of testimony are implausible, since one can gain knowledge from testimony without having beliefs in the reliability of the source, let alone knowledge of such reliability (see Coady 1973 and 1992). Innate knowledge provides a whole class of knowledge that is both non-inferred and non-observational. One sort of important non-inferential knowledge may be

¹ Cf. Maher's view that evidence is knowledge given directly by experience (Maher 1996).

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quasi-theoretical (it is probably a particular instance of innate knowledge). For example, knowledge that certain events have a cause or explanation may be non-inferential. I see a broken window. *Which* is the explanation of the broken window may well be inferred from prior experience. But knowing *that* there is an explanation is not inferred. And even if it is, it could not always be. What would it be inferred from? If it were inferred from prior experience that broken windows in the past have always had explanations, then it is being assumed that at least some of the prior occasions on which it was known that the broken window has an explanation were not themselves inferred in this way. (This is significant, since such knowledge may be just what is required in order for knowledge to be generated by Inference to the Best Explanation (IBE). Van Fraassen (1980: 21) states that ‘the realist will need his special extra premise that every universal regularity in nature need an explanation, before the rule [of IBE] will make realists of us all.’ However, it might be sufficient that we know that some specific regularities or even singular events have explanations for IBE to make us at least partial realists.) Furthermore, building a case against ($E = K$) on a distinction between inferential and non-inferential beliefs is to erect an argument on shifting sands. For the distinction is not a fixed one. A neophyte’s knowledge might be inferred but once she has become an expert no process of inference need take place.

To return to the first part of the counter-proposal, that evidence is non-inferential knowledge. The fallibility of memory illustrates the objection to this claim. The starting points of our inferences can be forgotten without impugning the knowledge inferred from them. Take any case where a chain of several inferences leads to a proposition, p , that is thereby known. Let the non-inferred knowledge from which the chain started be forgotten before the (inferred) end of the chain is reached. The initial non-inferred knowledge is not evidence for p since the subject no longer has that evidence. Nor, according to the counter-proposal, is any of the intermediate propositions evidence for p , since these are inferred knowledge. So the subject can know p by inference yet have no evidence for p . This is an unhappy conclusion that can be circumvented only by allowing the intermediate inferred propositions to acquire the status of evidence. A similar case can be made that does not rely on memory failure but on the idea that knowledge (non-inferred knowledge included) can be undermined by misleading additional evidence. That need not undermine the inferred knowledge, since that can remain knowledge so long as one is causally sensitive to subsequently acquired positive evidence.

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(One can possess evidence e for a hypothesis h without inferring h from e . Nonetheless one's belief in h may be sensitive to the presence of e .) I have presented this argument at length elsewhere (Bird 2004).

In conclusion, we do have non-inferential knowledge, but not all of it is observational. More importantly not all non-inferential knowledge is always retained. We often forget or intentionally discard our non-inferential knowledge once some useful inference has been reliably made from it. If only non-inferential knowledge were evidence, then we would have rather less evidence than we think we do. As it is we don't use 'evidence' that way and are happy to regard inferred knowledge as evidence when it is being used as a premise in some further inference. I surmise that the proponent of 'evidence is non-inferential knowledge' has mistaken a local asymmetry for a global one. As (IE) suggests, in some particular knowledge-producing inference, what is inferred *from* are the evidence propositions and what is inferred *to* is the conclusion proposition. But it is a mistake to think of our evidence propositions as being limited to the propositions at the start of a global chain of inferences that may extend well into one's past.

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4. Underdetermination Refuted

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Let us now take Williamson's equation, $(E = K)$, as established. According to $(E = K)$ any fact might be part of our evidence that is knowable. A sceptic will argue that what is knowable is limited. But on pain of begging the question one may not appeal to any view of the limited nature of our evidence in order to reach a sceptical conclusion asserting the unknowability of the conclusion propositions of arguments. This line of thinking may be applied to the case in hand. Let us recall the target empiricist sceptical argument:

- (OBS) all evidence is observational;
- (INF) from observational premises only observational conclusions may be rationally inferred;

therefore

- (SCEP) only observational propositions can be known.
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(OBS) says that our evidence is of a certain kind (observational). On the equation ($E = K$), the premise (OBS) is identical to:

(OBS)* all knowledge is observational.

But the sceptic's *conclusion* is that we can have no knowledge of facts, and their constituent entities, that are not observable. The premise (OBS) and the conclusion (SCEP) are identical. So the question is being begged.

The argument against the underdeterminationist may be looked at from another angle. Why should we think that all evidence is observational? It is by no means obvious that it is, and as I have already suggested, our normal usage conflicts with this claim. We are thus entitled to request an argument for it. Such an argument will want to have a restrictive conception of evidence. For if it is very liberal and inclusive (e.g. 'accepted belief'), then it will be the case that both (a) that it is less plausible that evidence is itself restricted to the observational, and (b) that it is more plausible that what may be inferred from the evidence includes non-observational conclusions. So let us restrict ourselves to those views of evidence that take knowledge to be a necessary condition on evidence. One might then straightforwardly employ a sceptical argument that establishes that since our knowledge is limited to the observational then so is our evidence. But then we could hardly use (OBS) as a premise in the underdetermination argument, since just that sort of argument is presupposed by the premise. So a less direct argument is required that refers to the particular proper subset of knowledge that evidence is supposed to be. Such arguments face the following objection. Such conceptions of evidence allow for knowledge that is not evidence ('non-evidential knowledge'). We might, therefore, make inferences from non-evidential knowledge and thereby gain new knowledge. As a result, the sceptical argument above would not be valid. Even if (OBS) and (INF) were true, (SCEP) would not follow, since it is not ruled out that one can get knowledge of the non-observational by inferring from premises that are known but are not evidence.

So the sceptic needs a conception of evidence such that (i) evidence is a proper subset of knowledge (limited to the observational), and (ii) there is no knowledge generated by arguing from known premises that are not evidence that could not have been gained by arguing from premises that are evidence. Thus the sceptical argument above would show that inferences from evidence yield only observational knowledge, while (ii) would show that inferences from non-evidential knowledge could not generate conclusions

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that go beyond that observational knowledge. So either way one is limited to knowledge of the observational. The conception of evidence that seems to satisfy (i) and (ii) is that of evidence as the *starting point* (whatever it may be) of all our inferences—that is, non-inferential knowledge. Non-inferential knowledge is clearly a proper subset of knowledge, satisfying (i)—to which we may add the claim that non-inferential knowledge is observational. It seems to satisfy (ii), for although there will be non-evidential knowledge, that must itself have been inferred ultimately from evidential knowledge.

The argument sketched in the latter part of Section 3 is intended to counter the view that evidence could be limited to non-inferential knowledge. As we saw, that conception of evidence requires the possession of evidence to be monotonic in this sense: it requires evidence to be retained and not lost if one is to retain knowledge of propositions inferred from the evidence. But this monotonicity is false. One can lose evidence through forgetting and undermining without always losing knowledge inferred from it.

5. An Aside Concerning Inferences from the Observable

Let us return to the sceptical argument from underdetermination. Its second premise was:

(INF) from observational premises only observational conclusions may be rationally inferred.

The propositions we are supposed to regard as observational are those such that an act of unaided sense-perception can typically, in favourable circumstances, decide whether they are true or false. Thus if p is observational then so is $\sim p$. Let O be an observational proposition and T be a non-observational proposition. Now consider $O \vee T$. If this is non-observational, then, since it is entailed by O , we may infer a non-observational proposition from an observational one. If instead $O \vee T$ is observational, then from this plus the observational $\sim O$ we may infer the non-observational T , by disjunctive syllogism. Thus, either way, we may infer a non-observational conclusion from observational premises. Clearly this argument, adapted from Prior's argument concerning the division of propositions into ethical and non-ethical ones, can be generalized to show that it is never the case that we can divide propositions into two kinds, such that each kind includes the negations of its own members and propositions

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belonging to one kind cannot be inferred from premises belonging to the other kind. This conclusion raises problems, for example, for sceptical arguments about knowledge of the external world that have as an assumption the impossibility of inferring facts about the external world from knowledge that exclusively concerns a subject's sense-impressions.

6. Underdetermination Revisited

So far I have argued that the empiricist sceptical argument from the premise that our evidence is observational fails. I now want to consider an argument weaker than the sceptical one, an argument that does not depend on the premise (OBS) and which might be used by a modified sceptical empiricist. Observability comes in degrees. And even realists are likely to accept that current science puts forward hypotheses about entities which are much further along the observable–unobservable continuum than earlier theories. So, even if we may allow that among our evidence for a current theory, there is knowledge of a theoretical nature, it might be conceded that often this theoretical knowledge will be more observational than the theory it is put forward as evidence for.² Thus, in such cases where the inference from evidence to theory is a movement from more observational to less observational, the modified sceptical empiricist will argue that more than one (slightly less observational) theory will fit the (slightly more observational) evidence.

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In effect the modified sceptical empiricist replaces (OBS) by:

(OBS)[†] theories are typically less observational than their evidence.

(OBS)[†] has the advantage over (OBS) of not being question-begging. To get the same sceptical conclusion we require a premise stronger than (INF):

(INF)[†] from premises of degree of observability g only conclusions whose observability is greater than or equal to g may be rationally inferred.

Even though prima facie plausible, (OBS)[†] is not indisputably true. For example, let us take a case that ought, if any case can, to instantiate (OBS)[†]: the

² For convenience I shall take 'theoretical' propositions to have some non-observational content.

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inference of the existence of molecules with rapid motions as the cause of the observable phenomenon of Brownian motion. Part of one's basis for this inference is the knowledge that *something* is causing the Brownian motion. If it might be a phenomenon without a cause then we would have no reason to infer of anything that it is the cause. (OBS)[†] tells us that the theory, that there exist molecules in rapid motion, is less observational than the evidence, which is (a) that there is Brownian motion, and (b) that Brownian motion has a cause. It may be agreed that the inferred molecules are less observational than Brownian motion. But are they less observational than the evidence claim that Brownian motion has a cause? That is unclear. For in the observation of Brownian motion one does not observe that it has a cause.

One might rescue (OBS)[†] by denying that that causal claim is really part of the evidence. In which case either (i) we must deny that the causal claim is ever known, or (ii) we must assert that it is itself inferred from the observational evidence (the Brownian motion itself). The first, (i) is unsatisfactory, for clearly we do know of particular events and kinds of events that they have explanations and causes (even if we do not know always what they are). Furthermore, to deny knowledge of the existence of causes would be to assume a scepticism of at least as strong a kind as the use of (OBS)[†] is seeking to establish. If we do not assume such scepticism, then we must hold that (OBS)[†] requires that knowledge of the existence of causes always be inferred, as stated in (ii). However, to admit this is to admit that from observational evidence (Brownian motion) the existence of something not observed (the existence of a cause) may be inferred. That itself would be a refutation of (INF)[†] and so no sceptical conclusion could be drawn.

(INF)[†], being rather stronger than (INF), is also rather less plausible (while also sharing the Prior-style objection discussed above). Why should a theory that is slightly more theoretical than the evidence be radically underdetermined by that evidence? (Remember that at this point we are assuming that there is no *quantitative* problem of underdetermination. Thus evidence can determine theories of the same degree of observability as the evidence. The empiricist typically holds that the empirical adequacy of theories can be known, which means that observation can rationally determine theories with no non-observational content.) If among our evidence is knowledge of the existence of atoms and molecules and a raft of facts about them (including facts concerning the existence of causes), why must those facts always fit with more than one explanatory theory of the *structure* of atoms?

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7. Epistemic Reduction

We have considered two arguments for sceptical underdetermination: (a) (OBS) & (INF), and (b) (OBS)[†] & (INF)[†]. (OBS) is stronger than (OBS)[†], while (INF)[†] is stronger than (INF). While I think that all four premises are false, we may for sake of argument concede the two weaker claims, (OBS)[†] and (INF). Even so the sceptical empiricist would seem to fall between two stools. For in (a) (OBS) remains unjustified, even if we allow (INF) or a relative thereof, while in (b) even if (OBS)[†] is acceptable, (INF)[†] has not been shown to be.

With this in mind let us focus our attention on the premises of the original argument (a). Although we lack justification for (OBS), the sceptical empiricist may nevertheless have a further argument to the effect that the realist who has conceded (OBS)[†] ought to accept any epistemological conclusions drawn from (OBS) (even if (OBS) is not strictly true). This argument says that it makes little difference whether we accept (OBS) or (OBS)[†], since in the end it is still facts that are thoroughly observational by van Fraassen's standards that do the epistemic work or provide the epistemic bedrock. So someone who accept the two weaker claims (OBS)[†] and (INF) should accept any conclusions about what is knowable that follow from (OBS) and (INF).

An analogy would be this. Although it is not possible to build an office block in one go, so one has to build the structure one storey at a time, it remains the case that the whole weight of the structure is borne by the foundations. Nothing we can do higher up can increase the load the foundations may bear. Similarly, we may in fact need to argue via intermediate theoretical beliefs when arguing for some advanced theoretical conclusion. But, be that as it may, the epistemic support lent to the conclusion ultimately derives from the observational evidence upon which the intermediate beliefs rest.

Let us assume, contrary to the arguments briefly presented above, that all our non-inferential knowledge is observational. Let us then consider some hypothetical individual M for whom this is the sum total of their knowledge at time *t*. M has made no knowledge-yielding inferences. Does M's knowledge—M's evidence—underdetermine belief in some highly theoretical proposition concerning deeply theoretical entities? The argument I am considering suggests that it is clear that it does. That argument then goes on to claim that if M's theoretical belief is underdetermined then so is the same

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belief held by N, who has the same non-inferential, observational evidence but has also acquired some additional knowledge by inferences from that non-inferential knowledge. The fact that N has made some inferences from his non-inferential knowledge cannot make a difference to which beliefs are underdetermined.

How should the realist respond to this version of the underdetermination argument, which suggests that if a hypothesis is underdetermined by our non-inferential knowledge, it is underdetermined by that knowledge plus any knowledge inferred from it? If this argument is not to be trivial, it must be comparing N who seeks to infer the theoretical proposition via some intermediate inferred proposition, and M who makes an inference to the same proposition but directly, without any intervening propositions; the argument asserts that if M cannot know some proposition, then N cannot know it either. Contrapositively, if N can know something, then M can know it. This view one might call epistemic reduction:

(ER) If H_d is knowable on the basis of evidence H_i where H_i is known on the basis of evidence E, etc., then H_d is directly knowable on the basis of evidence E etc. (i.e. without having to reason via knowledge in H_i).³

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In doing epistemology we may wonder what conclusions are justified by some set of evidence. (ER) says that we could always replace that set of the evidence by a second, deeper, set of evidence, the evidence that was evidence for the first set, and so on, until we have replaced all the evidence by the non-inferential (e.g. observational) evidence, and that doing so does not change the nature of the epistemic relations under consideration. In particular (ER) says, in effect, that although (OBS) may be false, it might as well be true as regards what it is possible to know. Imagine that some highly theoretical proposition, T, is inferred from theoretical 'evidence' E. Let E be inferred from observational evidence O. (ER) says that whatever epistemic status T has in virtue of its relationship to E, it has also in virtue of its relationship to O. Contrapositively, if O fails to justify or ground knowledge in T, then E fails to do those things also. Hence, if we use (OBS) in an argument that concludes that E is unknowable, then we may conclude that there is no possible evidence that could permit knowledge of E.

³ (ER) has relatives that have 'is confirmable' and 'is justifiable' in place of 'is knowable'. (They do not replace the 'is known', however, since it is the being known of h_i that makes h_i intermediate evidence.)

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(ER) is likely to be denied by those who believe in epistemological gradualism—the view that it makes a difference to what you can know whether you progress by a series of small inferential steps or by one big leap. Viscount St Alban’s commitment to gradualism impressed Sir William Whewell (1847: ii, 232):

By far the most extraordinary parts of Bacon’s works are those in which, with extreme earnestness and clearness, he insists upon a *graduated and successive induction* as opposed to a hasty transit from special facts to the highest generalizations.⁴

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But gradualism is largely absent from modern epistemology, because, I surmise, emphasis on semantic and syntactic accounts of confirmation deny its relevance. The semantic and syntactic relations between a set of evidence propositions and a theory are transitive and so remain the same whether or not intermediate propositions are employed. In the hypothetico-deductive model of confirmation, if O confirms E and E confirms T, then O is deducible from E and E is deducible from T, hence O is deducible from T. And so O confirms T. Thus hypothetico-deductivism endorses the relative of (ER) that concerns confirmation. Contrast inference to the best explanation. E might be the best explanation of O and T might be the best explanation of E. But it does not follow that T is the best explanation of O. After all T might be highly complex, and so, if simplicity is a criterion of goodness, T might lose out to competing explanations of O, which all may be quite simple.

The structural analogy may give (ER) some plausibility; nonetheless, we can see that (ER) must be false. Sometimes we can reason directly from evidence to a conclusion but on other occasions a chain of reasoning is required. The contrast between simple arithmetic and more complex mathematics illustrates this. And it need not be mere human limitation that prevents directly inferred knowledge of a complex proposition. Knowledge of the truth of a conjunction is a simple case where intermediate propositions, the conjuncts, will typically need to be known first. But even for atomic propositions, we sometimes must argue via intermediate steps to gain some piece of knowledge. There is not always some direct route from the same initial evidence to knowledge of the same distal conclusion. As an illustration of this, consider Commander Leonard Read, ‘Nipper of the Yard’, who is investigating a serious crime, committed at 11 p.m. at Mile End. Some part of his evidence gives him the

⁴ Cf. Bacon (1859: 166). For discussion see Cohen (1989: 4–13).

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knowledge that one of the two suspects, Reggie and Ronnie, must have committed the crime, but without making one more likely than the other. Another portion of Nipper's evidence consists of statements from reliable, independent witnesses, that Reggie was seen at the Angel, Islington at 10.50. Nipper's local knowledge tells him that one is unlikely to be able to get from the Angel to Mile End in less than twenty minutes even in the best traffic conditions. Nipper infers that Ronnie committed the crime. Let us allow that this inference gives him knowledge. It is difficult to see how, on the basis of this evidence, Nipper could get such knowledge without first inferring and getting to know the proposition that Reggie did not commit the crime or some other proposition closely related to this one. After all, if Nipper doesn't see at least that it is highly unlikely that Reggie is the culprit, how can he justifiably think that Ronnie is? The case of the elimination of one of a pair of competing cases can be seen as a special case of inference to the best explanation. If IBE does deliver knowledge, then it must do so via knowledge of the explanatory relations between the competing hypotheses and the evidence.

I suggest that the engineering analogy that made (ER) plausible is misleading. Knowledge is a kind of connection with the world. A tempting foundationalism suggests that the connection inferential knowledge has with the world is via the non-inferential knowledge it came from. But we have already had some reason to doubt this since inferred knowledge can survive forgetting and other forms of loss of the evidence from which it was inferred. The arguments sketched against (ER) confirm this. A better way of thinking of this is to regard knowledge as possessing its own links with the world that are to some extent independent of the links provided by the evidence. An analogy might be this. A climber ascending a cliff with pitons needs to be attached to the cliff by at least one piton at a time. Using that attachment he can knock in a new piton to hold him in place. That allows him to relinquish the first piton and to attach a third. While each one connection to the cliff needed an earlier one, the earlier ones play no further part in supporting the climber's weight. And a distance that cannot be traversed in one go can be surmounted in several steps.

Since (ER) is false, we still need to be given reasons why we should think that gradual increases in theoretical belief should mean that later beliefs are badly underdetermined by the evidence. Scientific knowledge has typically been acquired by a gradual process of inference via many intermediate propositions. The Falsity of (ER) shows that one cannot blithely ignore this and argue directly from 'the poverty of the stimulus' (the fact that we have relatively

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little and local sensory stimulation) to the conclusion that beliefs concerning the future, distant regions of the universe, the unobservably small, and so on are badly underdetermined. Of course, this is not to say either that theoretical advances are always gradual, or that theories are never underdetermined by the evidence. Both of those claims are false, and when they are, the beliefs acquired will not be knowledge. Quite possibly Dalton's atomic hypothesis was a large leap into the unobservable that remained underdetermined by the evidence for some time. Not all possible alternative explanations of the law of definite proportions were ruled out by the evidence that Dalton had. But we do not need to know a theory is true for it to be the basis of scientific research. And that research may generate evidence which at some later stage suffices to give us knowledge of the theory. Since we do not always know what we know, we may not be able to say at what point in history the atomic hypothesis became known, but it may have been some time after it had been accepted as the best theory on offer.

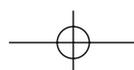
8. Cautious Constructive Empiricism

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Constructive empiricism ought properly be presented not so much as *sceptical* regarding theoretical knowledge but rather as *cautious*. The empiricist might not want to present or endorse a sceptical argument, but instead may suggest that constructive empiricism shows how science functions without having to refute scepticism. A distinction is made between belief and *acceptance*. The empiricist says that the aim of science is not to give us theories that we may believe to be true but instead only to give us theories we can accept. The epistemic component of accepting a theory is simply the belief that the theory is empirically adequate—that it saves the observable phenomena. There will be no error in accepting a theory that happens to be false.

If this is right then, first, we do not need to tackle scepticism about the unobservable, and secondly, if belief in the empirical adequacy of a theory is the only epistemic component of accepting the theory then we do not run the same risks of error that we would face if acceptance were to include belief in the truth of the theory. This sort of approach is suggested by the following remarks, among others, from van Fraassen:

There does remain the fact that even in endorsing a simple perceptual judgement, and certainly in accepting any theory as empirically adequate, I am sticking my neck



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out. There is no argument there for belief in the truth of accepted theories, since it is not an epistemological principle that one might as well hang for a sheep as for a lamb. A complete epistemology must carefully investigate the conditions of rationality for acceptance of conclusions that go beyond one's evidence. What it cannot provide, I think (and to that extent I am a sceptic), is rationally compelling forces upon these epistemic decisions. (1980: 72–3)

I will suggest that it is indeed sceptical to claim that there is no rational compulsion on epistemic decisions. Maybe, if one has such an aversion to false belief that one refuses to believe anything, then nothing could rationally compel one to believe. However, so long as one has even quite limited positive doxastic goals (e.g. belief in empirical adequacy), one may be making one's chances of achieving them poorer if one fails to believe some proposition that one is in a position to know. One way to see this is to consider the requirement of total evidence. If one is rationally required not to ignore one's evidence, it must come pretty close to a rational requirement that one does not ignore a fact that would be part of one's evidence were one to believe it. Hence a refusal to believe a whole class of propositions ought to be backed up by a commitment to the view that they would not be known even if they were to be believed—that is, one should be a sceptic about such propositions.

This, I believe, applies to constructive empiricism. In regarding acceptance as a proper attitude to take towards a theory, an attitude that is epistemically correct whenever the theory is empirically adequate, the empiricist must endorse scepticism one way or another. The argument is a disjunctive dilemma, where the disjuncts concern whether belief in theories is permitted. If, on the one hand, belief in theories is permitted then (so I will argue) the empirical adequacy of a theory is a sufficient epistemic reason for accepting a theory *only if* theories in general cannot be known, that is, only if scepticism is true. If, on the other hand, we say that belief in theories is forbidden, then there seems no independent justification for this injunction short of scepticism.

The first horn of the dilemma asserts that if belief in the empirical adequacy of a theory is the only epistemic requirement on acceptance, then theories that go beyond the observable must be unknowable, on the pain of finding that acceptable theories are known to be false. The argument is this. Since we are none of us positivists, we agree that there are facts that are not included among the observable phenomena. Let us now assume that it is possible to know at least some such facts. Then we may further imagine a situation in which one such fact, p , is indeed known. Note that because p goes beyond the

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observational evidence there are theories that are consistent with all observable facts but which are inconsistent with p —one such theory is T^* which says ‘ p is false but empirically adequate’. If science aims only to save the observable phenomena, then theories like T^* are acceptable. But since the falsity of T^* is trivially deducible from p , and since p , *ex hypothesi*, is known, we can know the falsity of T^* . Hence constructive empiricism would have to say that a theory is acceptable even though it is known to be false. Since the latter is not a happy conclusion, we must deny that, if theoretical belief is permitted, such belief can ever amount to knowledge. That is, we should be sceptics.

The other way to avoid the problem is to forbid belief in any proposition that goes beyond the observable. Which brings us to the second horn of the dilemma. If belief in theories is forbidden, then a fortiori knowledge of them is forbidden. We would then have to understand van Fraassen’s emphasis on acceptance not so much as stating an appropriate, satisfactory, or safe attitude to take, but rather as placing an upper limit on the permissible content of one’s beliefs. However, it is not clear why one should impose such a bound unless one thought that there is something epistemically unacceptable about propositions beyond that bound. For example, if one thought that all such propositions were false, that would be a reason for forbidding belief in them. But we don’t think that all theoretical propositions are false. It seems that the weakest reason one could have for avoiding belief in a proposition is that one would not know the proposition to be true. After all, if one would know it if one did believe it, why should one be required not to believe it? Thus even this route suggests that the constructive empiricist ought to endorse scepticism.

Since it is fairest to see van Fraassen’s constructive empiricism as cautious yet permissive, it is the first horn of the dilemma that bears the weight of the argument. And the strength of that horn depends upon there being something wrong with one’s accepting what one knows to be false. Consider the following principle:

(ACC) the proposition p is not rationally acceptable for S if there is some proposition q such that S knows that q is inconsistent with p , and S knows q .

If acceptance were straightforward belief, then the above principle would be clearly true. On the other hand, if accepting p is identical merely to believing that p is empirically adequate, then the principle is, as a generalization, false. However, acceptance is more than just belief in empirical adequacy; it is supposed to involve a certain sort of commitment. Pre-philosophically we

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might think that acceptance involves the sort of commitment that goes along with believing that a theory is true, or at least with believing that the theory has a reasonable chance of being true. However, even if we are persuaded to drop belief from acceptance, we will nonetheless not think that the scientific commitment involved in acceptance can go along with knowing that a theory is false. Correspondingly, that sort of commitment is not rationally acceptable if one is in a position to know one's theory is false. In short I am suggesting that constructive empiricism without scepticism falls foul of the following, slightly weaker but nonetheless plausible principle:

(ACC)* p is not rationally acceptable for S if there is some proposition q such that

- (i) S knows that q is inconsistent with p , and
- (ii) if S were to believe q , S would know q .

The only way for a constructive empiricist to embrace this principle is to deny that any such theoretical q could be known even if believed.

9. Conclusion

I have looked at one set of arguments concerning underdetermination, those that argue from the assumption that evidence propositions have a different kind of content (e.g. observational) from the target inferred propositions (e.g. theoretical), to the conclusion that the latter cannot be known. On the basis of $(E = K)$ I have argued that there is no basis for the restriction of evidence to some limited set of contents. All that is knowable is potential evidence. Thus a sceptical argument that assumes that evidence is restricted must assume that what is knowable is restricted and so assumes what it sets out to prove. Additionally I have looked at the suggestion that the proposition that our theories' content is less observational than our evidence in effect commits one to the same epistemological conclusions as the proposition that all our evidence is observational. This suggestion depends on the truth of a claim of epistemic reduction that is false.

So a natural form of the sceptical empiricist argument for underdetermination is refuted. This might not be thought to impact on constructive empiricism, since the latter is not, *prima facie*, a sceptical doctrine. I argued, however, that constructive empiricism ought to endorse scepticism concerning the unobservable, for otherwise it will permit acceptance of theories that

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might be known to be false. If it is possible to know that theories are true, an epistemology for science that is based only on acceptance will be inadequate. Thus if constructive empiricism is to be adequate it should commit itself to scepticism concerning the unobservable. But if the argument of this chapter is correct, the most obvious argument for such scepticism is flawed.

The bulk of the argument has been negative, aimed at resisting sceptical claims that knowledge of theories or of the existence of the external world is impossible. It does not attempt to show that we do indeed have such knowledge. To do that would require a proper description of our inductive practices (most especially inference to the best explanation) and an assessment of their reliability. While we are on the way to achieving this, we still have some way to go. But in the meantime we may be content with being able to stave off arguments that theoretical knowledge is impossible.⁵

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References

- AUSTIN, J. L. (1962), *Sense and Sensibilia* (Oxford: Oxford University Press).
BACON, F. (1859), *Novum Organum*, in J. Spedding, R. L. Ellis, and D. D. Heath (eds.), *Works vol. i* (London: Longman).
BIRD, A. (2004), 'Is Evidence Non-Inferential?', *Philosophical Quarterly* 54: 252–65.
COADY, C. (1973), 'Testimony and Observation', *American Philosophical Quarterly* 10: 149–55.
——— (1992), *Testimony* (Oxford: Clarendon Press).
COHEN, L. J. (1989), *An Introduction to the Philosophy of Induction and Probability* (Oxford: Clarendon Press).
MAHER, P. (1996), 'Subjective and Objective Confirmation', *Philosophy of Science* 63: 149–74.
van FRAASSEN, B. (1980), *The Scientific Image* (Oxford: Clarendon Press).
WHEWELL, W. (1847), *The Philosophy of the Inductive Sciences* (London: Parker).
WILLIAMSON, T. (1996) 'Cognitive Homelessness', *Journal of Philosophy* 93: 554–73.
——— (1997), 'Knowledge as Evidence', *Mind* 106: 717–41.
——— (2000), *Knowledge and its Limits* (Oxford: Oxford University Press).

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