Empirical Evidence Claims Are A Priori

Darrell P. Rowbottom

Lingnan University

Darrellrowbottom@ln.edu.hk

This paper responds to Achinstein's criticism of the thesis that the only empirical fact that can affect the truth of an objective evidence claim such as '*e* is evidence for *h*' (or '*e* confirms *h* to degree *r*') is the truth of *e*. It shows that cases involving evidential flaws, which form the basis for Achinstein's objections to the thesis, can satisfactorily be accounted for by appeal to changes in background information and working assumptions.

The paper also argues that the *a priori* and empirical accounts of evidence are on a par when we consider scientific practice, but that a study of artificial intelligence might serve to differentiate them.

1. The A Priori Thesis

Achinstein (1995) offers a critique of the view that empirical evidence claims are *a priori*, and more particularly of the following *a priori* thesis:

> The only empirical fact that can affect the truth of evidential claims of the form 'e is evidence for h' (or 'e confirms h more than h′', or 'e confirms h to

degree r') is the truth of e. All other considerations are a priori. (Ibid., pp. 448–449)

The concept of evidence under discussion is objective, rather than subjective. So whether *e* confirms *h* is a matter of whether *e* bears the correct relation to *h*, irrespective of what anyone thinks, believes, or so forth. For example, the objective relation might be logical. So if *e* entails *h* then *e* is evidence for *h* (and confirms *h* to degree 1 on a logical interpretation of probability). Partial entailment may similarly be allowed for.

Achinstein's critique has attracted little attention in the subsequent literature; it has only been cited three times in articles, as far as I can determine, and has not been challenged in detail (although Huemer 2009 objects to it).[1] This is surprising because work in contemporary formal epistemology often proceeds on the assumption that the *a priori* thesis is true.[2] Objective Bayesians such as Jaynes (2003) and Jon Williamson (2005; 2010), for example, think there are unique degrees of rational belief (in many, if not all, non-continuous cases) that can be determined without appeal to empirical considerations (aside, trivially, from the empirical content of the propositions concerned). In the words of Williamson (2010, p. 11): 'Many applications of probability invoke a notion of probability that is objective in a logical sense: there is a fact of the matter as to what the probabilities are; if two agents disagree about a probability, at least one of them must be wrong.'

[1] The citations are by Staley (2004), Huemer (2009), and Rosenbaum (2010). Further discussion appears in Achinstein (2005a).
[2] As Achinstein (1995, p. 448) notes, there are also accounts of objective evidence that do not employ probability theory. I will not discuss these, since they are less prominent.

Moreover, as I will subsequently show, Achinstein's critique of the *a priori* thesis would also suffice, if it were successful, to refute some *subjective* accounts of evidence, such as some versions of subjective Bayesianism, as well as Timothy Williamson's (2000) epistemic view of evidence. Thus whether it is correct is of wider significance than may initially be apparent.

2. Achinstein's Critique of the A Priori Thesis

Achinstein's critique of the *a priori* thesis rests on the idea that we sometimes discover that our evidence is 'flawed', or more precisely that the correlations we have noticed are not determined by relevant causal connections. Before we look at such a possible scenario, however, I should like to point out that a defender of the *a priori* thesis may accept that situations such as those presented by Achinstein occur, while denying that it is correct to describe these as involving 'evidence that turns out to be flawed' (Achinstein 1995, p. 449). Evidence can certainly change over time, on the *a priori* view, but it cannot ever be flawed (as it cannot be false). Only experiments or tests can be flawed. I will come back to this in due course.

Here's an example of a relevant scenario, which is my own. Consider the following evidence, provided from a study conducted to test whether P is an effective contraceptive:

> *e*: 1000 patients took a pill of type P, every day for a year. None of these patients became pregnant during that year.

And consider how this bears on the following hypothesis:

> *h*: Mary will not become pregnant during a year in which she takes a pill of type P every day.

Achinstein points out that many philosophers are attracted to the view that *e* is strong evidence for *h*, and therefore that P(*h*, *e*) is high, in this kind of case.[3] But at this stage, I must already raise an objection. We specified not only *e*, but also that *e* comes from a study conducted to test whether P is an effective contraceptive. Therefore an advocate of the *a priori* view will warn us against confusing P(*h*, *e*) with P(*h*, ee_1) where:

> e_1: *e* is a report from a study conducted to test whether P is an effective contraceptive.

In short, the information that *e* is derived from a study may be relevant; and plausibly we should consider P(*h*, ee_1) even if it just so happens to have the same value as P(*h*, *e*), when we are aware of e_1. (This is on Carnap's (1962, p. 211) principle of total evidence, according to which 'the total evidence available must be taken as a basis'.)

[3] Note that an *a priori* theorist need not accept that confirmation (or corroboration) value is equivalent to a probability. Consider, for instance, the functions favoured by Popper (1983), Milne (1996), and Huber (2008). If the extent to which *e* is evidence for *h* is to be determined by the extent to which it confirms *h* on such measures, then it might be maintained that P(*h*, *e*) having a high value does not entail that *e* is strong evidence for *h* (and/or *vice versa*). Thus the discussion in the main body of the text may be recast in terms of C(*h*, *e*), an appropriate confirmation function (which might be defined in terms of probabilities involving *h* and *e*).

If we are only interested in judging P(h, e), the fact that e came from a test is irrelevant and should not be mentioned in setting up the scenario.[4]

I am not nitpicking. In fact, an advocate of the *a priori* thesis will see this minor mistake as indicative of a deeper problem with Achinstein's argument. To be more specific, the charge will be that Achinstein fails to consider appropriate conditional probabilities, in so far as he neglects to include background suppositions. Achinstein (1995, pp. 464–466) anticipates and addresses this line of objection, as we will see.

But let us return to Achinstein's critique of the *a priori* thesis. Take either P(h, e), in the event that we are (supposed to be) ignorant of e_1, or P(h, ee_1) in the event that we are not. Now imagine we learn something like:

> e_2: The 1000 patients referred to in e were all infertile before taking P.

Or even,

> e_3: The 1000 patients referred to in e were all infertile before taking P, but 250 of them are now fertile although none have undergone any fertility treatments or taken any other drugs either during or after the year referred to in e.

Such additional information shows, or at least strongly suggests, that the study resulting in e was flawed. On learning e_3, for example, one might even think that the

[4] After giving his similar example, Achinstein (1995, p. 449) mentions 'a companion study' when considering additional evidence that may subsequently be acquired. Thus he must accept that that e_1 is part of the initial evidence.

drug was responsible for *restoring* fertility to some of the participants. Achinstein (1995, p. 450) concludes that such facts:

> should make us withdraw, or at least modify, our original claim about the evidential efficacy [of e, or e & e_1, with respect to h]

In section four, I will begin to explain why the possibility of discovering such evidence does not, *pace* Achinstein, present any problem for the *a priori* thesis. Beforehand, however, I will discuss the scope of his critique. This is to show that its potential significance is considerable, and not limited to objective accounts of evidence in formal epistemology.

3. The Scope of Achinstein's Argument

As mentioned in the introduction, Achinstein targets objective accounts of evidence. However, his critique of the *a priori* thesis would also have the (presumably unintended) effect of telling against some subjective accounts of evidence, such as some forms of subjective Bayesianism, if it were successful.[5] In particular, his argument potentially tells against the following thesis:

> *Subjective a priori thesis*: The only empirical facts that can affect the truth of evidential claims of the form 'e is evidence for h for subject S' (or 'e confirms h more than h' for S', or 'e confirms h to degree r for S') are: (a) the truth of e; and (b) the *fixed values* of S's conditional degrees of belief (such as P(h, e)).

[5] Although the effect may have been unintended, Achinstein (2001) has independent reasons for rejecting subjective accounts of evidence. As such, he might view this result as a bonus.

The idea behind this is just that any individual's conditional degrees of belief are fixed; and although determining these degrees of belief may be an empirical matter, there may be no way to change them. This holds in De Finetti's (1937, pp. 146–147) version of the subjective interpretation of probability, where degrees of belief are construed behaviourally:

> Whatever be the influence of observation on predictions of the future, it never implies and never signifies that we *correct* the primitive evaluation of the probability $P(E_{n+1})$ after it has been *disproved* by experience and substitute for it another $P^*(E_{n+1})$ which *conforms* to that experience and is therefore probably *closer to the real probability*; on the contrary, it manifests itself solely in the sense that when experience teaches us the result A on the first *n* trials, our judgment will be expressed by the probability $P(E_{n+1})$ no longer, but by the probability $P(E_{n+1}|A)$, i.e. that which our initial opinion would already attribute to the event E_{n+1} considered as conditioned on the outcome A. Nothing of this initial opinion is repudiated or corrected; it is not the function P which has been modified (replaced by another P*), but rather the argument E_{n+1} which has been replaced by $E_{n+1}|A$, and this is just to remain faithful to our original opinion (as manifested in the choice of the function P) and coherent in our judgment that our predictions vary when a change takes place in the known circumstances.

So for De Finetti, there is a temporally invariant fact of the matter about what P(*a*, *b*) is for you. Its value will not change as your beliefs change. And this premiss is consistent with a subjective Bayesian account of confirmation, where updating simply involves changing which probability relations one is interested in (e.g. for guiding action). However, Achinstein's argument relies on the idea that we *do* (rationally) change our conditional probability assignments on the basis of empirical evidence.[6]

This is also inconsistent with much more recent accounts of evidence, such as Timothy Williamson's (2000). If we take one's evidence to consist purely in what one knows, then we may consider the following thesis:

> *Williamsonian a priori thesis*: The only empirical fact that can affect the truth of evidential claims of the form '*e* is evidence for *h* for subject (or community) S' (or '*e* confirms *h* more than *h*′ for S', or '*e* confirms *h* to degree *r* for S') is that S knows *e*.[7]

For Williamson, one's evidence is one's knowledge and *vice versa* (i.e. E=K). It is consistent with this E=K thesis to hold that: whether *e* would be evidence for *h* for some subject S, *were it to be known*, is temporally invariant. In fact, Williamson does hold this. And since knowledge is not transparent, one may be mistaken about the evidence one has (and revise one's views about the evidence one has on the basis of new evidence).

4. In Defence of the A Priori Thesis

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[6] An advocate of the *subjective a priori thesis* could allow that we have beliefs such as P([P(*a*, *b*)=*r*], *c*). This enables her to provide an analysis of how statements such as "My view on P(*a*, *b*) has changed" may be interpreted as true (but elliptical).
[7] Here I assume Williamson's view of knowledge, and therefore that knowledge entails truth.

Let us grant that sometimes, we are 'mistaken about the evidential support' (Achinstein 1995, p. 451) that some evidence gives to some hypothesis. One obvious way to handle this is to say that sometimes our degrees of belief are not rational, in so far as they fail to map on to the appropriate objective probability relations.

How does this bear on the *a priori* thesis? It does not. To put it plainly, that we sometimes make mistakes about evidential relations does not entail, or even strongly suggest, that there are no *a priori* evidential relations (or even that there are *sometimes* no such relations). This is especially persuasive if we consider deductive cases. Even highly intelligent individuals can have inconsistent beliefs, which they mistakenly think are consistent; consider Russell's paradox.

But Achinstein (1995, p. 451) argues that:

> [I]f empirical information can make us withdraw or modify the claim that e is evidence for h, and the claim that e confirms h to such and such a degree, then the a priori thesis should be rejected. It is not the case that the only empirical consideration in determining the truth of such claims is the truth of e. The truth of empirical claims other than e can affect the truth of claims such as 'e is evidence for h'.

Notice the subtle shift. In the penultimate sentence, we are told that whether *e* is true is not the only empirical consideration *in determining* (the truth of a claim) that *e* is evidence for *h*. But in the final sentence, we are told something entirely different, namely that the truth of empirical claims other that *e* can *affect* the truth of the claim that *e* is evidence for *h*. If this is still not clear, consider the following analogy. Imagine that the only way for me to determine whether my mother likes the colour blue is to ask her. Does it follow that that whether she likes blue is *dependent* on the way that she answers the question? Quite to the contrary, the suggestion is absurd.[8]

Despite this error, Achinstein's case is not so easily dismissed. We are left with the worry that there is simply no *a priori* fact of the matter about, to return to our initial example, the value of P(h, ee_1). In fact, we may understand Achinstein's point to be that there is just no *a priori* way, at all, of determining the value of such relations *because empirical evidence could always come along which would show we were wrong in our estimation*. And that seems to be some kind of reason for doubting that such *a priori* relations exist at all.

Alternatively, on a less radical note, we may simply be concerned that many relations which we normally take to be evidential, on the *a priori* view, turn out not to be. And this accords with the position articulated by Achinstein (2005b, p. 48) in his most recent work on evidence: 'Although there are some a priori evidence claims, for the most part objective evidence claims are empirical.' Either way, the advocate of the *a priori* view ought to address the problem of flawed tests.

5. A Resolution of the Problem of Flawed Tests

It is possible to deal with the specific problem that Achinstein presents, namely that of flawed studies/tests, by declaring that P(h, e) = P(h, ee_1) = 0.5, say on the basis of

8 That is, unless one wants to adopt an epistemic theory of truth and/or intuitionist logic. Achinstein offers no arguments for either.

objective Bayesian equivocation norms, because neither e nor the conjunction of e and e_1 serves to confirm or disconfirm h. In fact, one might insist, the truth of these propositions is irrelevant to the truth of h. This avoids Achinstein's (1995, p. 452) worry that the *a priori* view: 'fails to note that… [$e\&e_1$] contains insufficient information to preclude… flaws.'

Now one might suppose that this is a worrying result for the objective Bayesian (or the logical theorist of probability), because it is difficult to see what kind of evidence could bear on h. First, however, note that even if no evidence we could ever gather would bear on h, it would not follow that the *a priori* thesis is false. A different possible conclusion, which Achinstein does not consider, would be that confirming such hypotheses is not possible given our practical limitations. Such a conclusion will seem unpalatable to most philosophers of science, but it is worth highlighting because it dovetails with the arguments of Popper (1959, app. *vii)—see also Rowbottom (2011a; Forthcoming)—for the zero logical probability of universal statements. (Note that h has a universal character in so far as it covers all space-time, in my example; admittedly, however, some of Achinstein's examples instead concern simple predictions of whether events will occur.) So one might even suggest that Achinstein (unwittingly) offers a powerful argument for anti-inductivism. That is, in the absence of an independent argument that the *a priori* thesis is false.[9]

[9] On a related note, the 'objective epistemic' interpretation of probability proposed by Achinstein (2001, ch. 5) is not novel in one respect that Achinstein appears to think it is, namely in severing the link between (epistemic) probability and rational degree of belief. To see this, we need merely note that although Popper thought that the logical probability of all universal laws was zero, he did not declare that it was irrational to have a non-zero degree of belief in some such laws. The possibility of severing the link between logical probability and rational degree of belief was also clear from the earlier work of Keynes (1921). For a more detailed discussion, see Rowbottom (2011a, pp. 45–50).

Second, there is an alternative that Achinstein does not consider. To his credit, he does entertain the possibility that the defender of the *a priori* thesis will appeal to 'background assumptions being made' (Achinstein 1995, p. 464); and he argues against such a move, because background assumptions can't rule out flaws in general.[10] However, he misses a kind of bold background assumption that avoids this problem. Consider P(h, ee_1b), where b is 'The study referred to in e_1 was not flawed'. Clearly, the advocate of the *a priori* theory will say, *this* conditional probability is positive, and indeed equal to unity. After all, h is entailed by the conjunction of e, e_1 and b.[11]

Let's allow scientists to use claims such as b as working hypotheses. The *a priori* thesis is saved without accepting the radical conclusion that confirmation is never (or infrequently) possible. To illustrate this, let us represent e and e_1 and b as E. All that matters for determining whether E is evidence for h is the truth of E. So all that Achinstein (1995) shows, it may be said, is that we often have great difficulty in determining the truth of the evidence by empirical means. In particular, it is extremely difficult to determine, empirically, whether it is true that a study/test is not flawed. But no advocate of the *a priori* thesis need declare otherwise.

The natural way for Achinstein to respond would be to ask us to consider what would count as evidence for b, i.e. for 'The study referred to in e_1 was not flawed', or for similar claims about other studies. Do we not need further tests? Let's grant that we do. When we're trying to work out whether the results of those tests are evidence for

[10] As he puts it, 'there may be an unexpected evidential flaw' (Achinstein 1995, p. 466).

[11] In saying this, I understand 'not flawed' in a particular way; I assume, for example, that the test would be flawed if it failed to indicate the absence of a causal relation of necessitation (or the falsity of a particular universal law statement) when such a relation did not exist (or when such a law was false).

b, we will need to consider a statement similar in kind to *b*, which we may label b', e.g. 'The test to determine whether the study referred to in e_1 was flawed was not itself flawed'. But all this shows is that we have to stop somewhere, which is old epistemological news. This is just a special case of the well-known regress problem. It is not ultimately pertinent to the *a priori* thesis. In asking whether *e* is evidence for *h* we do not require that there is further evidence for *e*. What we require is that *e* is true.

Note that it would also be false to claim that this 'working assumption' strategy 'fails to provide any motivation whatever for seeking any new information of the sort reported by [e_2 or e_3]' (Achinstein 1995, p. 453), as some of the alternatives do. On the contrary, we are painfully aware that our working assumptions may be false. When we think of these as auxiliary hypotheses, the point is familiar (and rather unremarkable). To bring hypotheses in to contact with experience, we must make assumptions. But we needn't hold those assumptions fixed, come what may. We can investigate whether they are true. Like Le Verrier in the face of the aberrant orbit of Uranus, we may also imagine how they might differ in order to save our theories and observation statements.

So just because it is difficult to tell whether a given study/test is flawed, it does not follow that it is never reasonable to assume that it is not (especially if one has done one's best to check whether it is, and is open to the possibility that it is not). We can always choose to probe our background assumptions more deeply, whether they concern naked eye observations or studies/tests, and the *a priori* thesis does not suggest otherwise.

6. Scientific Practice and Ellipsis in Scientific Discourse

Even if it is accepted that an advocate of the *a priori* thesis can deal with the problem of flawed tests in the fashion detailed above, this does not show that one should not reject the *a priori* thesis instead. So we also should consider Achinstein's positive argument for his alternative, empirical, view of evidence. At its heart is the idea that we should look to the way that science is done, and in particular at how scientists treat evidence claims, in order to inform our view of evidence. In the words of Achinstein (1995, p. 464):

> The fact that there are evidence statements that are defended or criticized by making empirical claims appears to conform with the idea that such statements employ an empirical rather than an a priori concept of evidence. In the former but not the latter case, whether e is evidence for h is, or at least can be, affected by the truth of empirical claims other than e.[12]

My counter claim is that there is no evidence from the way that science is done (in the sense of scientific method) which tells in favour of *either* view of evidence, *a priori* or empirical. Conversely, I also hold that neither view of evidence has any direct consequences for scientific method. In order to see why, let's consider Achinstein's (1995, p. 466) scientific example:

> Thomson seems to have regarded the fact that Hertz's tubes were not sufficiently evacuated as an experimental flaw that Hertz did not reasonably

[12] See also: 'evidential claims made by scientists and others are frequently defended by appeal to empirical considerations not part of the evidential claim itself. An empirical concept of evidence makes better sense of such defences than does an a priori concept.' (Achinstein 1995, p. 470).

expect given his information and techniques. All of this conforms better with the empirical concept of evidence.

Achinstein thinks it is natural to see Thomson's appeal to a further possibility, empirically investigable but not considered by Hertz, as support for an empirical view of evidence. The story goes that Hertz made a claim of the form '*e* is evidence for *h*', and Thomson pointed out that there might be an evidential flaw such that '*e* is evidence for *h*' was false. On the alternative view outlined above, the story is that Hertz made a claim of the form '*e* is evidence for *h*', which should be interpreted elliptically as '*e* is evidence for *h* given my background assumptions'[13], and Thomson questioned a key background assumption (which might have been implicit rather than explicit). What happened is consistent with either tale.

In fact, an advocate of the *a priori* 'working assumptions' view, as articulated in the previous section, might attempt to argue that her view is superior. It is superior, she might say, in so far as it draws attention to the importance of making one's working assumptions explicit. But I don't think that this strategy ultimately works. An advocate of the empirical view of evidence may simply say that one should make explicit the ways in which future evidence might bear on the probability/evidential relation posited. Stalemate again ensues.

[13] The underlying idea dates back to at least Keynes (1921, p. 7):

> [W]hen in ordinary speech we name some opinion as probable without further qualification, the phrase is generally elliptical… As our knowledge or our hypothesis changes, our conclusions have new probabilities, not in themselves, but relatively to these new premises. New logical relations have now become important, namely those between the conclusions which we are investigating and our new assumptions; but the old relations between the conclusions and the former assumptions still exist and are just as real as these new ones. It would be as absurd to deny that an opinion *was* probable, when at a later stage certain objections have come to light, as to deny, when we have reached our destination, that it was ever three miles distant; and the opinion still *is* probable in relation to the old hypotheses, just as the destination is still three miles distant from our starting-point.

Of course, Achinstein might instead insist that it is important to treat what scientists say literally whenever possible, i.e. to avoid positing elliptical discourse if at all possible. This seems to fit with the following passage:

> [Advocates of the empirical view] will separate the evidence statement a speaker is making from any empirical defence of such a statement that might be offered by that speaker or others. They will suggest that this is preferable because it better reflects what scientists and others actually do when they make evidential claims which they then defend empirically. And, they will insist, such an empirical defence makes better sense if the conception of evidence being used is empirical, not a priori. (Achinstein 1995, p. 465–466)

But if Achinstein's argument from flawed tests suffices to show that we should think of new evidence as potentially bearing on a probability relation like $P(h, e)$, then it also shows that we should think of new evidence as potentially bearing on an unconditional probability like $P(h)$. So why is 'e is evidence for h' not, in general, *elliptical* talk of $P(h)$? Imagine a scientist proclaims that 'The probability of Einstein's theories of relativity is high!' Rather than take that as elliptical talk, i.e. to reflect a claim that the probability of these theories is high relative to some unstated evidence (such as our scientific findings at some point in time), we may instead take the statement literally under an empirical view of evidence. We may understand the gathering of evidence as an empirical investigation into the unconditional probability $P(h)$, which proceeds without recourse to probability relations between the hypothesis and our evidence at any particular point in time.

The advocate of the empirical view of evidence is trapped. If the argument from flawed tests is successful, scientific discourse like 'e is evidence for h' need not be taken literally. And if scientific discourse like 'e is evidence for h' *is* taken literally, then it is reasonable to treat other scientific discourse, concerning 'probability of h', as elliptical. Thus it is simply not possible to avoid taking some scientific discourse concerning evidence (or probability) as elliptical, on the empirical view of evidence. It is therefore inaccurate to characterise the difference between the *a priori* and empirical views as whether we take scientific claims at face value. The difference only concerns *which* scientific claims we take at face value.

7. A Possible Objection to the 'Working Assumptions' Strategy[14]

Considering whether talk of P(*h*) should be taken as elliptical suggests a potential objection to the 'working assumptions' account. On what principled grounds, if any, may we say that any given assertion, *p*, is not to be construed as elliptical too? Not all assertions are explicitly evidential. But why should we think that explicitly evidential assertions are elliptical in the way outlined above, whereas others are not?

Here, I think that the advocate of the *a priori* view may simply bite the bullet, and declare that *many* assertions without an obvious evidential character are, indeed, elliptical in the way suggested by the 'working assumptions' account. In fact, this may be an interesting novel suggestion from the point of view of the norm of assertion. To assert that *p* is neither to represent oneself merely as believing that *p*

[14] I am grateful to an anonymous referee for raising the objection covered in this section.

(Black 1952), nor to represent oneself as knowing that *p* (Unger 1975, p. 253–270). Instead, it is to represent oneself as believing that *p* in virtue of some further assumptions. Take the claim 'Virtual photons do not exist' (Rowbottom 2011b) as a case in point. When I say this, we might think that I am representing myself as: (a) believing that it is true; and (b) holding the belief in virtue of an appropriate *a priori* evidential relation between it and (some of) my other beliefs. (Of course, some room for manoeuvre is present for the *a priori* theorist. For example, I may instead be representing myself as (b*) *believing* that an appropriate *a priori* evidential relation holds between it and (some of) my other beliefs. But we need not explore these finer distinctions here.) This perspective may seem natural to advocates of Bayesianism, who could give it the following gloss: by asserting that *p*, one expresses that P(*p*, *b*), where *b* is one's background information, is over a particular threshold. And this move is compatible with either subjective or objective Bayesianism.

This cannot work as a *general* strategy, however, because some statements (like some beliefs) are basic. Take 'I saw a black rabbit' as an example. I believe this, in part if not in whole (since observations are theory-laden), because of an experience rather than a further belief. The *a priori* theorist may point to this feature as differentiating some statements, and marking them out as not elliptical (either generally or typically).

However, we should not be too quick to conclude that no basic statements are elliptical. We might, at least *prima facie*, understand some such claims as (something like) 'if my belief formation process on the basis of sensory experience is reliable in context class C, then *p*'. (And 'belief formation process' may include the use of beliefs in theories, if the relevant observation statement is indeed theory-laden.) This may seem slightly odd, but it isn't clearly wrong. Keep in mind, in particular, that non-assertive utterances, and effects such as implicature, play vital roles in discourse that it is easy to lose sight of when thinking in terms of confirmation theory.

In any event, the *a priori* theorist is not forced to take such a route. (It is possible to appeal to context of utterance instead, for example. There is no obvious reason to think that existential claims made in science should be understood in the same way as existential claims made in supermarkets; whether one is engaged in inquiry, rather than shopping, may be highly significant.) Rather, the fact that an a *priori* theorist *may* take such a route—that there is nothing obviously wrong with so doing—serves to diminish the force of the objection.

Note also that Achinstein's argument that evidential claims are *mostly* not *a priori* may be challenged, on the basis of appeal to 'working assumptions' on a piecemeal basis, even if it is conceded that *some* evidential claims are not *a priori*. That is to say, one may oppose (or strongly doubt) a claim such as 'Although there are some a priori evidence claims, for the most part objective evidence claims are empirical.' (Achinstein 2005b, p. 48) without adopting an extreme alternative. This middle ground may be appealing to some readers.

8. Computational Implementation as the Proof of the Pudding?

So far, I have argued only that Achinstein's arguments against the *a priori* thesis—and by implication, related theses such as the *subjective a priori* thesis—fail. In closing, I should like to suggest that it may be possible to choose between the *a priori* and empirical views of evidence by comparing how they fare when they are implemented in computational contexts, e.g. in artificial intelligences and social epistemological simulations.

Let's consider artificial intelligence programmes, particularly those designed to perform scientific discovery or data mining functions, as a case in point. If these programmes work on the basis that the *a priori* thesis (or something similar, like the *a priori subjective* thesis) is true, and nevertheless enjoy considerable successes, then this may serve as evidence that it is indeed true.[15] The same may be true, *mutatis mutandis*, for the rival empirical thesis. For example, perhaps programmes designed around the idea that evidential links can be empirically investigated—and that the values assigned to these should change—will fare better at some tasks than those that are not. (Naturally, I doubt this. For as I said earlier, my belief '[is] that neither view of evidence has any direct consequences for scientific method'. But I may be wrong, and this might be a way to empirically examine whether I am.)

Now it appears, *prima facie*, that many contemporary learning programmes work on the basis that either the *a priori* thesis or the *subjective a priori* thesis is true; in particular, this is true of work done in what Gillies (1996, ch. 2) calls 'the Turing tradition', which uses a 'logical' rather than 'psychological' approach.[16] Let's just

[15] A similar case may be made for social epistemological simulations. For example, Laputa employs Bayesian agents; see Olsson (2011).

[16] Gillies also argues, with illustrations, that the logical approach has proven far more successful than its psychological counterpart (which endeavours to simulate how scientists have reasoned). The psychological approach appears to fit better with Achinstein's focus on the history of science.

consider programmes using a Bayesian approach, such as that discussed by Muggleton (1997).[17]

It is not necessary to go into detail, in order to grasp the fundamental point. Once the initial parameters are set, the prior probability distributions in the programme are fixed, and how those distributions will change, in the light of possible future evidence, is also fixed. In short, the value assigned to P(h, e), for any given hypothesis and any given evidence, is itself evidence insensitive.

It is unclear how one would implement the empirical view in a probabilistic framework; at least, I am not aware of a programme that does so. (Don't take this as evidence that there isn't such a programme; I am far from being an expert in the field.) Clearly the values that a programme assigned to evidential relations, such as P(h, e), would need to be allowed to change. But the obvious way to do this is to presuppose that there are second order probability statements that are fixed *a priori*, in so far as the programme should respond to new evidence concerning the

[17] The point I cover below also goes for many non-Bayesian programmes in the field known as inductive logic programming. This grew out of attempts to model concept learning from examples, which involves moving from pre-classified examples (positive or negative) to a suitable classification rule (or rules). A variety of different techniques are employed to enable such moves. The simplest are either 'top-down' or 'bottom-up'. In the former case, rules are generated and tested against the examples, and the process repeated (if necessary) until a suitable rule is arrived at. In the latter case, the examples are used to generate the rule(s). Many modern programmes, such as Prolog, integrate the two processes (in one way or another).

In essence, many of these programmes give the same outputs when given the same inputs, by using the same procedures. So it would seem that the extent to which any given set of examples supports a rule—or, in the case of a model of scientific reasoning, any given data supports a hypothesis—is *effectively* evidence insensitive for the programme. At the very least, the *most supported rule* (hypothesis) by any given set of examples (evidence), in the programme's 'view', would have to be fixed in order for the same examples (evidence) to issue in the same proposed rule (hypothesis).

Key is that the procedures used to arrive at the final rules (hypotheses) are fixed. They do not change in response to any evidence. But presumably they *would* have to be flexible if the empirical view were to be fully implemented. Consider an analogy on the personal level. If I learned that my estimate of P(h, e) was bad, empirically, I might not want to make future estimates of a similar kind—that is, concerning similar hypotheses and similar evidence—in the same way. I might want to adopt a different procedure for doing so (or even withhold judgement in similar scenarios, pending further evidence).

aforementioned relations in a uniquely determined fashion. Think of it this way. The programme will need to consider P([P(h, e)=r], e') for a range of possible values of r, relative to new evidence e'. But it will need to be programmed such that it can respond to this new evidence in a set way in advance. It cannot also be expected to consider P({P([P(h, e)=r], e')=s}, e'') for a range of possible values of s, relative to new evidence e'', and so on back, *ad infinitum*. So it would seem that the programme would have to rest on an *a priori* basis, although this is compatible, of course, with the *a priori* thesis (and the other counterparts discussed previously) being false.

Of course, appearances may be deceptive. An advocate of the empirical view could insist that the aforementioned programmes *really* work on the assumption that the assessment of evidential relations remains fixed empirically. Indeed, it might also be urged that evidential relations empirically determined by the programmers are sometimes put directly into the programmes, and that this may account, in part, for their successes. An *a priorist* riposte might be that relaxing the aforementioned assumption should lead to benefits, if the empirical view is correct, and whether this is so remains to be seen. (So even if there is a way to reinterpret the Bayesian programmes such that they are compatible with the evidential view, this still doesn't presently tell in its favour.)

In any event, the field of machine learning is vast. There are numerous sub-fields, and programmes available within those sub-fields, and my knowledge of these is limited. So I do not want to make any claims about what the state of the art tells us about the relative prospects of the *a priori* and empirical views of evidence. I will content myself with suggesting that it may very well tell us something, and proposing this as an avenue for future investigation.[18]

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[18] Likewise, the nascent field of computational social epistemology may tell us something. But that is liable to be in the future.

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