# Comments on Belief and Beyond

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## 1 Introduction

In their paper "Belief and Beyond: Toward a New Orientation in Epistemology"[2] Bandyopadhyay and Taper argue that this century's epistemological tradition has emphasized only the concept of belief and paid insufficient attention to the notion of evidence. In particular they argue that evidence is a conceptually distinct concept from that of belief and thus conflating the notions of belief and evidence leads to error. They further argue that evidence ought to be accorded similar status to belief in epistemology because of it's utility in answering questions and resolving paradoxes.

Bandyopadhyay and Taper take belief questions to be those questions which ask how much some piece of data confirms a hypothesis. Strangely, however, they take E to confirm H if and only if $1 > P(H \mid E) > 0$ and the degree to which E confirms H (denoted by $c(E, H_1)$) to be given by $P(H \mid E)$. In a footnote they call this absolute confirmation and defend it against incremental confirmation (notions of confirmation requiring $P(H|E) > P(H)$) by noting it is the best measure of the quality of a single model [1]. However useful this concept of absolute confirmation may be it surely doesn't capture the pre-theoretic notion we

[1] The further argument they provide, namely that if they adopted $P(H|E) > P(H)$ as defining confirmation there would be many different ways to calculate the degree of confirmation, is not compelling. We only believe absolute confirmation has only one measure of strength (up to ordinal equivalence) because we believe the principle that the degree of confirmation should be monotonic in $P(H|E)$. However, it seems we might also formulate reasonable principles which restrict our choice of the degree of confirmation. Indeed some such principles are formulated by Eells and Fitelson[5]. Moreover, it is unclear why this would

call confirmation which generally requires that the evidence makes the hypothesis more probable. For instance if we were to flip a fair coin 10 times we would not normally speak of seeing a head on the first toss as strongly confirming 'at least one toss will land tails' despite the fact that P(not all heads|first coin heads)=.998. One might speak of the hypothesis being strongly confirmed in such cases but the only normal usage of confirmation as a relation between a particular piece of data and a hypothesis seems to

While the notion of confirmation chosen does not play an integral role in later arguments the fact that incremental confirmation is a well studied notion of confirmation is relevant to the status of Bandyopadhyay and Taper's concept of evidence. Bandyopadhyay and Taper define evidence as a relation which holds between a piece of data D and two hypothesizes $H_1$ and $H_2$ with degree given by e(D,$H_1/H_2$) which is meant to capture the amount the data favor one hypothesis over another. While not stated explicitly it seems the they are suggesting that evidence is not only a different notion than confirmation but one which cannot be easily reduced to talk of confirmation. The question of whether to take evidence as a primitive three place relation or to define it in terms of confirmation is beyond the scope of this paper and we refer the reader to Fitelson[6] for some interesting thoughts on the subject. However, one important observation that Fitelson makes is that the Bayes Factor which Bandyopadhyay and Taper choose as their measure of evidence can be easily defined in terms of a common measure of incremental confirmation.

Following the Bandyopadhyay and Taper we let $e(D, H_1/H_2) = \frac{P(D|H_1)}{P(D|H_2)}$. Choosing a common measure of confirmation (incremental confirmation to be specific) we let $c(D, H) = \frac{P(H|D)}{P(H)}$ Yet now $e(D, H_1/H_2) = \frac{c(D,H_1)}{c(D,H_2)}$. Thus the Bandyopadhyay and Taper's notion of evidence is seen to be the same as how many times more the data confirms the first hypothesis than the second hypothesis[2]. This seems to suggest that one could get by perfectly fine viewing confirmation

be an argument against this notion of confirmation in the first case or what the argument for absolute confirmation is even supposed to show (the fact that there *is* an argument suggests it is not just a matter of convenient definition.

[2]Other choices of a confirmation function would then give rise to different notions of evidence. Thus the fact that we had to choose a particular measure of confirmation is not a concern as Bandyopadhyay and Taper allow for the possibility of other measures of evidential strength.

as the central notion and evidence as a derivative concept. Still just because the this notion of evidence can be easily cached out in terms of a notion of confirmation does not mean the belief evidence distinction is unimportant. Answering this question requires determining if this distinction truly aids in solving problems.

Before we examine the applications of this distinction it is important to note that their are other concepts of evidence than the one the Bandyopadhyay and Taper use. Not only are there different possible choices of a measure for evidence but the word evidence is also sometimes used to describe what they would call a question of belief. In addition to the several philosophers quoted in their paper as explicitly using the word evidence to refer to questions of belief the Oxford English Dictionary offers as one definition evidence, "Ground for belief; testimony or facts tending to prove or disprove any conclusion."[11] Indeed Bandyopadhyay and Taper themselves note, "There is little point in denying that the meanings of 'evidence' and 'confirmation' (or its equivalents) often overlap in ordinary English as well as among epistemologists."[2] Thus when analyzing arguments about evidence we must be careful and not assume they are talking about the same notion of evidence that Bandyopadhyay and Taper use. In light of this observation we will examine the three applications offered for the evidence/belief distinction.

## 2 Base Rate Fallacies

The first application Bandyopadhyay and Taper suggest for their newly clarified belief evidence distinction is to base rate fallacy situations. These are a type of probabilistic reasoning problem where most people give dramatically incorrect answers and which some critics[10] have used to argue that Bayesianism rationality is an unrealistic standard. Kahneman, Slovic and Tversky advocate what Bandyopadhyay and Taper call a 'heuristics and biases' approach to explain these results which postulates people use certain heuristics in these situations and the deficiency in these heuristics explains the observed biases in the outcome. Bandyopadhyay and Taper suggest that instead these same results could be explained instead by the belief/evidence distinction.

A prototypical problem which demonstrates the base rate fallacy is determining the probability of an illness given a positive test for that illness. For instance in the example Bandyopadhyay and Taper use the subjects are asked to evaluate the probability that an individual suffers from TB given they test positive. They are informed the test has a 10% false positive rate and 1% of the population suffers from TB. The correct response given this information is 10% but subjects give much higher answers in these kinds of situations. That is subjects discount the base rate of TB in the population. Bandyopadhyay and Taper point out that while the probability of the person having the disease is quite low a positive test is very strong evidence for having a disease. Thus they reason that the true cause of the subject's poor performance is a confusion of the belief and evidence questions.

If this is true it still provides no defense of the rationality of the subject unless the subjects truly believed they were being asked an evidence question. However, it seems quite unlikely that the subjects truly thought they were being asked about the state of evidence rather than how probable it was the individual had TB. In fact this tendency to overlook base rates in medical tests persist even among medical students and faculty [4]. Since this question resembles the sort of diagnostic decisions they face (or will face) in their profession it seems implausible to assume they believed the question was inquiring about the strength of evidence rather than the reliability of the diagnosis.

The base rate fallacy even persists when questions are phrased in more familiar terms. Kahneman, Tversky and Slovic [10] describe an experiment where the subject is told that a witness identified a blue cab at the seen of a hit and run. They are also informed that only two cab companies operate in the city and given the following information.

- 85% of the cabs in the city are green. 15% are blue
- Given a 50/50 sample of green and blue cabs in a similar situation the subject makes an incorrect identification 20% of the time.

The subjects are then asked what is the probability the cab was blue. While it stands to reason that subjects would interpret this as a question of guilt or innocence (or at least liability) rather then one about the evidentiary value of just the witness they still overlook the base rate. While the correct answer is 41% the subjects give a median response of 80% [12]. This in itself is troubling as it seems best explained by the simple hypothesis that the subjects simply copied the probability the witness was correct rather than computing the strength of evidence favoring the cab was blue over the cab was green.

Another troubling wrinkle develops when the problems description is changed to say that 85% of the cabs involved in *accidents* in the city are green while 15% are blue. While the correct probabilistic reasoning and answer remain unchanged the median subject response improves to 60% [12]. Since it makes sense to suppose the subjects interpret the question similarly in both cases in at least one of these cases the subject is answering their interpretation of the question incorrectly. Moreover this is a pattern which is fairly consistent across experiments, when the base rate can be interpreted causally (as in the later description) it is given a greater weight[10]. While there is some disagreement over whether it is really the causal nature of the base rate or merely the perceived relevance of these base rates which makes the difference[3] in neither case does the belief/evidence distinction offer a defense of the subjects rationality.

So it seems that the belief/evidence distinction can offer no defense of people's rationality in base rate fallacy situations. This need not mean this distinction is useless in understanding cases of the base rate fallacy. Rather than understanding the confusion of belief and evidence questions as an alternative to the heuristics and biases model advocated by Kahneman, Tversky and Slovic one might instead understand it as a refinement of that model. In particular one might suppose that people use evidence as a heuristic to arrive at probability judgments, perhaps modifying prior levels of belief. If we further suppose that people are able to integrate causal or relevant base rates more easily into their background beliefs than other base rates we may be able to explain the experimental results.

While this might be an interesting elucidation of our heuristics one can no longer truly say that the belief/evidence distinction explains the base rate fallacy. Rather we have pushed the explanation of the base rate fallacy back to our ability to integrate base rates into our background knowledge. So at best in this account we could hope the belief/evidence distinction explains how the base rate fallacy occurs though not why.

While this second use of evidence in the base rate fallacy seems promising neither it, nor the somewhat implausible confusion hypothesis are particularly well supported. Just observing that the hypothesis predicts (at least qualitatively) the phenomena is not sufficient to establish the truth of the hypothesis. At best we might hope for an inference to the best explanation to justify this move but in this case it is unclear why this provides a better model than the standard heuristics and biases approach. In other words Bandyopadhyay and Taper have at best given evidence for the hypothesis that a belief/evidence confusion underlies the base rate fallacy in comparison to the negation of that hypothesis. However what they have not done is shown any evidence supporting the belief/evidence distinction over the heuristics and biases approach which is what would be necessary to support their point.

Finally Bandyopadhyay and Taper have offered preliminary results (at FEW 2005) showing that individuals who were asked both the evidence and belief questions in a base rate fallacy situation performed somewhat better than those who were just asked the belief question. While they take this as data supporting their theory this is not so clear. The relatively minor improvement could simply be explained by supposing asking the question about evidence made them think harder and longer about the problem, or simply hint to the subjects that they should come up with a different belief answer than evidence answer. Moreover, the fact that the majority of subjects still made the error shows the belief/evidence distinction can't explain the entirety of the base rate fallacy. However, as the standard heuristics and biases approach at least offers the possibility of explaining the entire effect without more data it seems premature to overturn the heuristics and biases explanation in favor of (or even supplemented by) the belief/evidence confusion.

## 3 Old Evidence Problem

In the old evidence problem Glymour[7] argues that Bayesianism can't explain the apparent validation of General Relativity (GR) by the discovery that GR predicted Mercury's perihelion shift (M). The difficulty is said to arise because the perihelion shift had already been observed (and known to Einstein) when GR was formulated. In particular if P denotes the probability function at the discovery of GR Glymour argues that $P(GR|M) = P(GR)$ since $P(M) = 1$. $P(M)$ must be 1 because the phenomena had already been observed. Thus he concludes that M cannot constitute evidence for GR.

Admittedly Glymour's use of the word evidence is at odds with that used by Bandyopadhyay and Taper. However, as the term evidence appears to be genuinely unclear about what concept it picks out we cannot dismiss Glymour's argument merely because it's notion of evidence is different than ours. Indeed as Bandyopadhyay and Taper note Glymour's presentation of the old evidence problem implicitly rests on a notion of evidence where E constitutes evidence for H if and only if $P(H|E) > P(H)$, i.e., he is using evidence to mean incremental confirmation.

This equivocation on the use of the word evidence suggests (at least) two distinct problems of old evidence. In the first interpretation it is asking how could data (M) already known at the creation of a theory (GR) ever give us cause to later accept that theory. That is if we discover a logical relationship between previously known data and a new theory how should this affect our confidence in the theory. Following Joyce [9] we will refer to this as the problem of logical learning.

In contrast the second interpretation asks how can we explain the status of data (M) as evidence when it was already known at the creation of the theory (GR). In other words whether or not we should come to accept this new theory what authorizes the status of old data as evidence for that theory. Also following Joyce we will call this the problem of evidential relevance.

It is unclear exactly which problem Bandyopadhyay and Taper seek to address. Their focus on the status of M as evidence for General Relativity (GR) over Newtonian Gravity (NG) would seem to suggest they were addressing the problem of evidential relevance. However, Glymour appears to concentrate primarily on the question of logical learning so their critique of his solution would suggest they meant to deal with this problem. However, in either case it does not seem like the notion of evidence Bandyopadhyay and Taper want to introduce can solve the problem.

## 3.1 The Problem of Logical Learning

If we understand Glymour to simply be using the word 'evidence' to mean grounds for belief the problem he describes is clearly the problem of logical learning. That is if scientists already knew about the perihelion shift of Mercury when General Relativity was formulated how could this observation give us grounds to believe in GR as opposed to NG. In particular since the scientists in 1915 already assign probability 1 to M conditionalizing on M should not change any of their probability judgments. Thus it seems M should not be able to change which theory they thought is most likely to be true, i.e., induce them to accept GR over NG. However, in actual fact learning that GR entailed M did cause many scientists to accept GR over NG apparently at odds with any Bayesian theory of scientific practice.

In this case Bandyopadhyay and Taper's criticism of Glymour for confusing belief and evidence is unfounded. Glymour is really asking a belief question (why did the scientists have grounds to believe in GR over NG when they didn't previously?) and is thus justified in addressing it in terms of belief. The mere linguistic difference that Glymour uses 'evidence' to refer to what Bandyopadhyay and Taper call incremental confirmation ($P(H|E) > P(H)$) is not enough to call his argument into question.

Even if Glymour is not making the error that Bandyopadhyay and Taper accuse him of making we should ask if their notion of evidence can solve the problem of logical learning. However, it is hard to see how Bandyopadhyay and Taper's notion of evidence could be used to explain the adoption of a new scientific theory. As they themselves note a piece of evidence $E$ can be evidence for $H_1$ over $H_2$ even though $H_1$ logically implies $H_2$ and thus $H_2$ is guaranteed to be more probable than $H_2$. Yet it seems absurd to suggest scientists should reject a theory so as to accept another theory which logically implies the first. While one might think this is merely a peculiarity of the situation where the two theories are compatible and thus not relevant to true scientific theory change equally severe problems arise when comparing two incompatible theories [3]. In short

[3]Suppose $H_1$ is the current best theory of physics and $H_2$ is some absolutely huge polynomial fit to all previously observed events plus the next piece of evidence E. $H_2$ clearly exists even though we can't identify it in advance of learning E. If our current best theory of physics is probabilistic like quantum mechanics and does not predict E with certainty then

despite Glymour's wording this problem is very much a problem of belief and Bandyopadhyay and Taper's notion of evidence is ill suited to solve it.

This raises another troubling issue. Bandyopadhyay and Taper claim to have solved the old evidence problem by showing the evidence provided by M for GR over NG, denoted by $e(M, GR/NG)$, is quite high yet they use the Bayes Factor to measure the strength of evidence in this case. That is $e(M, GR/NG) = BF = \frac{P(M|GR)}{P(M|NG)}$ [4] Yet one can easily show using Bayes Theorem that the ratio or the priors multiplied by Bayes Factor gives the ratio of the posteriors. If we let $P_i$ denote the prior probabilities and $P_f$ the posterior probabilities this becomes:

$$\frac{P_f(GR)}{P_f(NG)} = \frac{P(M|GR)}{P(M|NG)} \times \frac{P_i(GR)}{P_i(NG)}$$

This tells us that if the evidence given by M for GR over NG then the ratio of the posteriors should be much higher than the ratio of the priors. Thus if Bandyopadhyay and Taper are correct (which we will see they are not) in their calculation of M providing strong evidence for GR over NG then they have also shown that conditionalizing on M increases the probability for GR relative to the probability for NG. Thus as a matter of mathematical necessity any solution of the logical learning problem in terms of this notion of evidence guarantees a solution in terms of belief. In other words either the problem itself was an illusion an M really does raise the probability of GR over NG or this notion of evidence provides no solution.

In fact I'm inclined to believe that the problem of logical learning is not much

$P(E|H1) < P(E|H_2) = 1$. Thus E provides evidence for $H_2$ over $H_1$. In fact by picking E to be a sufficiently unlikely event in theory $H_1$ we can make E arbitrarily strong evidence for $H_2$ over $H_1$. Yet if evidence was supposed to dictate our acceptance of scientific theories every time we observe an unlikely quantum event we should abandon quantum mechanics in favor of some polynomial fit to the data. Thus evidence on its own is not enough to justify theory change.

[4]Actually the authors do not calculate BF in this situation to show that the evidence is strong but instead they calculate BF with the additional 'reasonable' assumptions that in the Newtonian situation no undetected gravitational bodies are affecting Mercury in addition to some approximating assumptions to simplify calculation. Completely neglecting the possibility of over gravitational bodies seems dangerous as this probability is likely quite high given M and NG. Still $P(M|GR)$ is still likely much lower than $P(M|NG)$ as we can bound $P(M|NG)$ by Bandyopadhyay and Taper's result plus the probability there is a gravitational body which would explain M in NG.

of a problem at all once we realize that Bayesianism is a normative theory of scientific practice. Just as deductive logic tells us that we should strive to close our beliefs under logical consequence Bayesianism tells us we should strive to assign probabilities as if we had conditioned on every certain belief we held. When we learn that the axioms of number theory entail Fermat's last theorem there is no paradox that we come to endorse Fermat's last theorem[5]. In the deductive case we simply realize that the norm we aspire to says we must accept Fermat's last theorem if we wish to keep accepting the axioms. Similarly when we learn that GR entails M we realize the Bayesian norm we aspire to either requires that we give up M (or some other background beliefs) or assign a higher probability to GR. That is once we understand that the probability calculus is no more a descriptive characterization of actual scientific beliefs than deductive logic is a description of mathematical beliefs, but instead both are norms the respective subjects try to follow, the difficulty disappears.

## 3.2 Evidential Relevance

The problem of evidential relevance does not seem as easily addressed as that of logical learning. In this interpretation Glymour is not using evidence as a mere shorthand for raising the posterior probability but really intends it as an analysis of some pre-theoretical notion of evidence. What is in need of an explanation is what authorizes our conviction that M is evidence for GR. This then is no longer a purely historical puzzle as we *still* consider M to be evidence for GR. However, the fact that M was known at the creation of GR prevents us from making the simple move of saying we call something evidence if it was correctly used as evidence in the past.

Since this question depends on an analysis of our pre-theoretical notion of evidence one might think Bandyopadhyay and Taper's criticism that Glymour is confusing belief and evidence may have some weight in this case. However, Glymour only talks about M being evidence for GR not whether M is evidence for GR over NG. Interpreting this in a comparative notion of evidence it seems

[5]Assuming we believe the axioms of number theory. To avoid difficulties of what this means on nominalist accounts of mathematics interpret both the axioms and the theorems to be truths about operations on beads.

natural to suppose Glymour is talking about M being evidence for GR over  GR. Surely in an intuitive sense, in addition to being evidence for GR over NG, M is also evidence for GR over  GR. So if Bandyopadhyay and Taper's account is to really solve the problem of old evidence they should not only be able to show why M is evidence for GR over NG but also why it is evidence for GR over  GR.

It turns out that Glymour's (implied) criteria for when M is evidence for GR ($P(GR|M) > P(GR)$) holds in exactly the same situations where Bandyopadhyay and Taper's notion of evidence says M is evidence for GR over  GR ($\frac{P(E|GR)}{P(E|\ GR)} > 1$)[6]. So while these two notions will of course disagree about the strength of evidence they should always agree on what is evidence when a hypothesis is being compared to its negation. Therefore either Glymour's original account is in error and M is evidence for GR even on his criteria or Bandyopadhyay and Taper's notion of evidence cannot correct the problem.

This brings up another interesting puzzle. Bandyopadhyay and Taper claim their notion of evidence is independent of whether that agent already knows or believes in the data. That is whether M is evidence for GR over NG or GR over GR will be answered similarly whether or not M is old evidence. Presumably this is meant to rely on the 'objective' nature of likelihoods. However as we observed above Bandyopadhyay and Taper's notion of evidential strength is equal to the ratio of priors to posteriors. Thus if the evidence provided by M for GR over NG is always high repeatedly conditionalizing on M should repeatedly increase our probability for GR over NG. Thus any Bayesian who kept being told about M would be obligated to raise his probability of GR relative to NG each time.

The answer to this puzzle lies in the fact that while likelihoods and likelihood ratios may be more 'objective' than other types of probability they are not

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\begin{aligned}
& e(M, GR/\ GR) = \frac{P(E|GR)}{P(E|\ GR)} > 1 \\
\leftrightarrow \quad & \frac{P(GR|E) \times P(\ GR)}{P(\ GR|E) \times P(GR)} > 1 \\
\leftrightarrow \quad & \frac{P(GR|E)}{P(GR)} \times \frac{1 - P(GR)}{1 - P(GR|E)} > 1 \\
\leftrightarrow \quad & \frac{P(GR|E)}{P(GR)} > 1
\end{aligned}
$$

immune from change by conditionalization. Bandyopadhyay and Taper only calculate $e(M, GR/NG)$ as if M hadn't yet been observed. However, if they had done this calculation assuming M is already known (as Glymour does since it is old evidence) they would have gotten a different result. To calculate $e(M, GR/NG) = \frac{P(M|GR)}{P(M|NG}$ when the agent does not know M you would derive the probability of M under both GR and NG from reasonable assumptions about the solar system as Bandyopadhyay and Taper did in the paper. However, if the agent already knows M then $P(M) = 1$ and $P(M\&X) = P(X)$. Hence $P(M|GR) = \frac{P(M\&GR)}{P(GR)} = \frac{P(GR)}{P(GR)} = 1$ and similarly $P(M|NG) = 1$. Hence $e(M, GR/NG) = 1$ meaning M is not evidence for GR over NG. Thus since M was already known when GR was constructed and therefore should be assigned probability 1 at this time the old evidence problem reappears in Bandyopadhyay and Taper's notion of evidence.

It would seem then that Bandyopadhyay and Taper's notion of evidence is of no use in resolving the old evidence problem. It also calls into question whether their notion of evidence is capturing what we mean by evidence at all since surely we do consider M to be evidence for GR. In fact it seems that both Glymour's approach and Bandyopadhyay and Taper's approach get into trouble for similar reasons. They both define evidence in terms of credences which take into account all of the agent's background knowledge. However, this does not seem to track how we actually reason about such situations. If we are asked whether M is evidence for GR we don't include our beliefs about experiments demonstrating M in our analysis. Rather we reason from some set of background assumptions and ask whether M is evidence for GR over NG relative to these background assumptions. A full discussion of this solution is beyond the scope of this paper but I refer the reader to Hawthorne[8] for what I think is a compelling solution to the dilemma.

# 4 Achinstein

This finally brings us to Bandyopadhyay and Taper's critique of Achinstein. They argue that his account is flawed because Achinstein requires that evidence provide good reason to believe. This they claim is confusing the belief and evidence questions. However, Bandyopadhyay and Taper do not accuse Achinstein of inconsistently using the notion of evidence, rather their critique is that he uses a notion of evidence much like their notion of confirmation. Yet as we noted in the introduction there really are several different notions we refer to as evidence. Indeed Achinstein is well aware that what we mean by evidence is unclear and explicitly notes, "I believe that these three responses represent conflicting tendencies in the way we actually speak about evidence, and that a different but related concept of evidence is associated with each."[1]

Thus the concept Achinstein chooses to investigate and call veridical evidence is just an account of one of the several concepts that are referred to as 'evidence'. Thus observing that this notion of evidence disagrees with the one advocated by Bandyopadhyay and Taper or doesn't respect their intuition that evidence should explain observed correlation can only establish that the type of evidence they are speaking of is not the same as veridical evidence. To show Achinstein's account is truly flawed or guilty of anything besides a different choice of terminology Bandyopadhyay and Taper would need to establish his theory is either incoherent or couldn't hope to explain important phenomena without the notion of evidence they advocate. As they offer arguments for neither of these points it seems premature to dismiss Achinstein.

## 5 Conclusion

The Bandyopadhyay and Taper do make a very compelling argument that there is some notion of evidence distinct from the question of belief. They also correctly advocate for a more systematic use of terminology in regards to these two notions. Furthermore it seems quite reasonable to think that we need to explicate both concepts to get an adequate understanding of the concepts we use in our pre-theoretic discourse.

However, as we noted in the introduction there are different notions which also frequently go by the term 'evidence.' Thus when critiquing other philosophers it is not sufficient to point out that they use evidence to describe a concept you would like called belief but you must also establish their argument actually trades on a confusion and is not merely using a different terminology. In fact when discussing their motivation for using the word 'evidence' as they do Bandyopadhyay and Taper say, "Our case for distinguishing them rests not on usage, but on the clarification thus achieved in our thinking and inferences frequently made in diagnostic studies." [2]. Thus it cannot be said that the philosophers like Achinstein who use evidence in other senses are wrong. At worst they are guilty of not choosing the most clear terminology.

While Bandyopadhyay and Taper's intuition that the old evidence problem needs a notion distinct from belief or credences to solve it does not appear their measure of evidence meets this criteria. Since they still sought to define evidence in terms of subjective credences their notion of evidence inherited all the problems old evidence caused for Glymour. It would seem that any successful attempt to address this problem will require a concept of evidence not defined in terms of credences. Of course this distinct notion of evidence need not be the fundamental new notion and instead be defined in terms of some objective account of incremental confirmation as Hawthorne suggests[8].

So while Bandyopadhyay and Taper may have a case that other discussions of the subject lack the important tool of using both belief and evidence the actual examples suggested as applications for their concept of evidence do not seem compelling. Still it seems their central intuition, that we can't do epistemology only in terms of belief is absolutely correct. Indeed even if their notion of evidence did not go far enough it does seem right that once we have successfully explicated this concept it will offer the sort of solution to the old evidence problem that Bandyopadhyay and Taper suggest.

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