Inference to the Best Explanation is Coherent

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Abstract

In his (1989) van Fraassen argues that Inference to the Best Explanation is incoherent in the sense that adopting it as a rule for belief change will make one susceptible to a dynamic Dutch book. The present paper argues against this. An epistemic strategy is described that allows us to infer to the best explanation free of charge.

Advocates of the inference rule commonly referred to as *Inference to the Best Explanation* (IBE) all share the conviction that explanatory considerations have confirmation-theoretical import. They equally agree that the rule still awaits an adequate precise formulation. However, in his (1989) van Fraassen advances an extensive argument for the claim that, "whatever the details" (169), IBE is unsound as an inferential principle. He first argues that if IBE is to work at all it must be in the form of some probabilisitic rule (ch. 6) and then goes on to argue that if that rule is at variance with Bayes' rule—and if it is not, it is otiose of course—it is, as any such rule, incoherent (ch. 7). Thus, he concludes, it would be irrational to adopt IBE as a rule for belief change. In the present paper I argue against this. It will be shown that van Fraassen's objection against probabilistic IBE is not backed up by the argument he provides for it; the true conclusion of that argument is not that IBE is incoherent but that the supporter of the rule must be consistent, in a sense to be made precise, in his preference for IBE. The conclusion is in fact more general than that; it holds for any update rule. But first I briefly rehearse some basic notions central to the discussion.

According to Bayesians an agent is rational exactly if: (i) at any given moment his degrees of belief obey the axioms of probability; (ii) as he acquires new evidence, he changes his degrees of belief in accordance with Bayes' rule.[1] In what follows $p(\cdot)$ is, as usual, defined to be a function from sentences/propositions to $[0, 1]$ and is assumed to represent some agent's belief system at a certain point in time; $p_{E_k}(\cdot)$ represents the belief system that results from $p(\cdot)$ upon learning E_k. Bayes' rule for belief change can then be

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[1]'Liberal' Bayesians, such as van Fraassen, think the second condition is too strong: although they too hold that Bayes' rule is the only rational *rule* for belief change, they deny that rational belief change *must* be guided by a rule. To our discussion this disagreement is irrelevant however.

written thus:

$$p_{E_k}(H_i) = p(H_i|E_k), \tag{1}$$

with $p(H_i|E_k)$ defined as follows:

$$p(H_i|E_k) = \frac{p(H_i) \cdot p(E_k|H_i)}{\sum_{j=1}^{n} p(H_j) \cdot p(E_k|H_j)}, \tag{2}$$

where the H_i's are assumed to be collectively exhaustive and mutually exclusive. The second equation is one of the many forms of Bayes' theorem; depending on the axiomatization, it either follows from the axioms of probability or from the definition of conditional probability. Bayes' rule, on the other hand, does not follow from probability theory: that only tells you what conditional probability to assign to H_i in case E_k, given the probabilities you already assign to H_i and E_k and given your conditional probability of E_k in case H_i, but it is silent on what new probability to assign to H_i when you have come to know that E_k obtains. It's only Bayes' rule that says that it has to be exactly your earlier conditional probability of H_i given E_k as computed according to Bayes' theorem.

Ramsey (1926) and de Finetti (1937) had given so called Dutch book arguments to justify the axioms of probability. They showed that unless your degrees of belief obey these axioms, a bookie who knows nothing but your belief state can make a series of bets against you such that all bets seem fair by your lights but together ensure a loss come what may (such a series of bets is called a Dutch book). More recently a related argument has been devised to justify Bayes' rule. The argument, due to David Lewis and reported in Teller (1973, sect. 1.3), purports to show that in case you update by some rule other than Bayes', a bookie, knowing nothing but your current probabilities and your update rule, can make a series of bets against you, offered at distinct times, which will all seem fair to you at the moment they are offered, but which together will inevitably make you lose money. This 'dynamic' Dutch book argument is elaborated in van Fraassen (1984) and (seemingly) effectively put to use in his (1989) critique of probabilistic versions of IBE. It shows, van Fraassen contends, that "there cannot exist any such probabilistic rule of Inference to the Best Explanation, on pain of incoherence" (1989:138). That doesn't follow at all from the Lewis/Teller/van Fraassen-argument, I want to insist. The insight delivered by that argument may well be an important one, but it is not what van Fraassen and many others have taken it to be. To see that let us consider the argument in more detail.

Van Fraassen's presentation of the argument makes use of a particular statistical model. In the model a number of hypotheses are considered about the bias of an 'alien' die to come up ace. As the die is tossed, and the data come in, the probabilities of the hypotheses are updated according to Bayes' rule. Then an agent, Peter, is introduced who adheres to some (unspecified) probabilistic version of IBE. This adherence results in his giving extra credence to the hypothesis that explains the data best, where a hypothesis is taken to be the best explanation of the data so far simply if it has, so far, highest probability. It is then argued that this practice leads Peter into incoherence in the sense that he will have a 'net loss' in every possible future (i.e. he is susceptible to a Dutch book). Thus "the possibility of vindication for his opinion is sabotaged from the start" (1989:158), whereby Peter has violated "a minimal criterion of reasonableness" (1989:157).

One might complain about the way the argument is set up. Presumably not even the staunchest defender of IBE would want to hold that IBE is applicable in every context. And it is exactly the sort of context in which van Fraassen puts the discussion that seems not to license an inference to the best explanation. For, as Lipton (1991) and Day and Kincaid (1994) have pointed out, IBE is a contextual principle in that it heavily draws upon background knowledge for its application (e.g. in order to judge what hypothesis among a number of rivals is the best explanation for the data gathered). But in van Fraassen's model there is no such knowledge to invoke (the die is purposely chosen to be an *alien* die). Moreover, the notion of explanation van Fraassen introduces is arguably a very impoverished one. Nevertheless, I will attempt to show that not even van Fraassen's critique of what can at best be an extremely impoverished version of IBE is successful. That is to say, I will limit the discussion to applications of IBE in contexts of simple chance setups and accept van Fraassen's notion of explanation for such contexts.

Van Fraassen's argument purports to be entirely general and not dependent on any particular formulation of probabilistic IBE. He himself does not (and need not) bother to provide such a formulation. It cannot harm, however, to have a concrete example before us. Here is a simple one: Again we assume $H_1, \ldots, H_n$ to be mutually exclusive and jointly exhaustive. Then to calculate $p_{E_k}(H_i)$ for each H_i first update by Bayes' rule, then add an explanation-bonus of (let us say) 0.1 to the H_i (or H_i's) that best explain(s) $E_1, \ldots, E_k$ (if any), and then normalize the sum of the thus obtained probabilities to 1 in the obvious way. Let us call this rule EXPL. Note that, in case the hypotheses are explanatory independent of the data in the sense that none of the H_i's can be said to explain $E_1, \ldots, E_k$, EXPL and Bayes' rule agree on $p_{E_j}(H_i)$ for all i, j.

The following illustrates how EXPL works 'in practice' (and how it diverges from Bayes' rule). We consider three hypotheses concerning the bias of a certain coin C:

H_1: C has perfect bias for tails
H_2: C is fair
H_3: C has perfect bias for heads

The three hypotheses are assumed to be jointly exhaustive and to have equal initial probability. We now start flipping our coin and update our probabilities as the first and second toss both come up heads. The table gives the results when we update by EXPL and, between brackets, when we update by Bayes' rule:

	H_1	H_2	H_3	p(next toss heads)
initial probabilities:	0.33	0.33	0.33	0.5
first toss heads:	0 (0)	0.3 (0.33)	0.7 (0.67)	0.85 (0.83)
second toss heads:	0 (0)	0.16 (0.2)	0.84 (0.8)	0.92 (0.9)

If the Lewis/Teller/van Fraassen-argument is correct, then it must be that a dynamic Dutch book can be made against anyone who updates his beliefs by means of EXPL. And that indeed seems to be the case. The following example is essentially the same but somewhat simpler than the dynamic Dutch book that is made against Peter in van Fraassen's (1989, ch. 7).

Suppose a bookie approaches you and offers to make bets on the following propositions concerning tosses of C, of which you know it is either fair or has a perfect bias for either heads or tails:

> A: The first two tosses will land heads
> B: The third toss will land tails

The bookie offers you two bets:

> I pays \$240 if A is false
> II pays \$3000 if $A \,\&\, B$ is true

the values (initial probability times pay-off) of which are ($7/12 \cdot \$240 =$) \$140 and ($1/24 \cdot \$3000 =$) \$125 respectively. The total cost of the bets thus equals \$265.

Suppose at least one of the first two tosses does not come up heads. Then your loss equals \$25. That, it seems, is all in the game. But now suppose the first two tosses do come up heads. Then the bookie proposes to buy the following bet from you:

> III pays \$3000 if B is true

If we assume your update rule is EXPL, your probability that B will come true is now 0.08, as can be read off from the table above. So you will agree to sell III for the price of \$240. But whatever further may happen, you now have lost money, *viz.*, $\$265 - \$240 = \$25$. Hence whether A does or does not come true, you will lose \$25. Were you to use Bayes' rule instead, nothing the like could happen. In that case you would, in our example, have assigned a probability of 0.1 to B had the first two tosses come up heads. Thus not \$240 but \$300 would have seemed a fair price to ask for III, in which case you would have had a gain of \$35 (in which case presumably no bookie would have proposed to buy III).

The argument has met with many objections in the literature.[2] I here concentrate on two objections raised by Maher, one directed against what (using a distinction Maher 1993:112 makes) could be called the dynamic Dutch book *theorem*—which asserts that updating by any rule other than Bayes' makes one susceptible to a dynamic Dutch book—the other against the dynamic Dutch book *argument*—which infers from the theorem that non-Bayesian updating is irrational.

The gist of Maher's (1992, 1993) critique of the dynamic Dutch book argument is that a loss in one respect may be outweighed by a benefit in some other. The point can be brought out quite nicely with the help of EXPL: if you test it against Bayes' rule in computer-simulations you will find that, in *some* environments, it does on average much better than Bayes' rule in the sense that in those environments EXPL approaches the truth more rapidly. It's not hard to think up other rules for which the same conclusion holds.[3] Suppose, then,

[2] Many authors have rightly challenged the alleged generality of the argument. E.g., it has been pointed out that the Lewis/Teller/van Fraassen-argument still allows explanatory considerations to be taken into account when determining: the prior probabilities of hypotheses (Harman 1997); priors as well as likelihoods (Day and Kincaid 1994); the evidential weight of the data (Leplin 1997).

[3] It may be difficult to find a rule that does better than Bayes' in *every* environment; perhaps there is no such rule. From a methodological perspective this point is hardly important however; as especially realist philosophers of science have been urging since long, what counts is whether a rule for belief change, or, more generally, a methodology, is 'fit' for *our* world. See for a clear and concise statement of this view Leeds (1994:203).

we had a ‘realistic’ version of a probabilistic IBE and suppose in our world it happened to be the case that that rule approaches, on the average, the truth much faster than Bayes’ rule—*nothing van Fraassen or any other author has said excludes that this could be the case.* Following this hypothetical IBE instead of Bayes’ rule would perhaps have advantages which are not so easily expressed in dollars; yet they clearly should somehow be taken into account when the bill is totted up. In other words, if van Fraassen is right, adhering to IBE or any other non-Bayesian rule may cost you, but as long as it hasn’t been shown that adherence to some such rule cannot be *worth* the costs, the non-Bayesian shouldn’t be convicted of irrationality.

Maher (1992) also attacks the dynamic Dutch book theorem. He argues, roughly, that if the bettor in the dynamic Dutch book arguments looks ahead he can see the sure loss coming and so will decline to bet. In terms of the example given earlier, already when the bookie offers bets I and II you can figure out that, should the first two tosses indeed come up heads, you will prefer to sell back the bet on B to the bookie and thus have a guaranteed negative net pay-off. This situation is easily avoided: simply do not buy bets I and II in the first place. We just saw that even if updating by some rule other than Bayes’ should entail costs, it doesn’t follow that non-Bayesian updating is irrational: it might have advantages that outweigh the costs. However, it seems Maher’s ‘look before you leap’ principle (cf. Earman 1992:49) opens up the possibility of non-Bayesian updating at no costs at all!

Unfortunately, as Skyrms (1993) points out, Maher’s argument crucially hinges on the assumption that if you refuse to buy the first two bets, the bookie will not propose III, and that just need not be so. If the assumption is dropped and you calculate the expected pay-off of not buying I and II, you will come to conclude that, although this time there is no sure loss, the *expected* loss is the same as when you do buy these bets. In fact, if the bookie should add some premium, however tiny, for each transaction made, buying all bets will be preferable to buying III only (Skyrms 1993:324).

Skyrms’ point of critique is acknowledged by Maher (1993:110). Maher concludes from it that the dynamic Dutch book theorem is correct after all. But that is too quick, I believe. In the remainder of the paper I will propose an epistemic strategy that promises exactly what Maher’s principle promised (non-Bayesian updating free of charge) but that, as far as I can see, does not depend on any unwarranted assumption. I conclude that already the dynamic Dutch book theorem is false.

The first point to note is that dynamic Dutch book vulnerability has everything to do with violating equation (1) and nothing with (2). Put more precisely, the Lewis/Teller/van Fraassen-argument shows that to be safe from dynamic Dutch bookies it is necessary to obey (1), not that it is necessary to obey (1) *and* (2). The claim now is that one thing the non-Bayesian who wants to be safe in that respect should (in general—cf. note 4) do is stick to (1) but give up (2). In other words, he should adhere to the following double equation:

$$p_{E_k}(H_i) = p(H_i|E_k) = \ldots, \tag{3}$$

with the definition of the preferred rule filled in after the second ‘=’.[4]

[4]There is a famous footnote in Ramsey’s (1929:247n) that has, in the seventies, provoked much controversy in discussions concerning the semantics of conditionals but is, I think, also

It may not be immediately clear how this could affect the problem the non-Bayesian faces, for conditional probabilities apparently had no part in the situation that led to the Dutch book presented above. But that is only so on a particular assumption. The Dutch book in this paper was closely modeled after van Fraassen's more elaborate example. This included the way the initial probabilities of A and $A \,\&\, B$ were calculated. The method van Fraassen uses (cf. his 1989:168f), and I used, to calculate initial probabilities of n successive aces c.q. heads (abbreviate both by E), for a model with m bias hypotheses, is this:[5]

$$p(n\ E\text{'s}) = p(H_1) \cdot p(E|H_1)^n + \cdots + p(H_m) \cdot p(E|H_m)^n.$$

Since it makes use of the Special Multiplication Rule I shall call this method SMR. In case the events are indepedent in the sense that, if we are given that some H_i is correct or if we temporarily assume that it is, it makes no difference whether we use the Special or the General Multiplication Rule, SMR standardly counts as one way to calculate initial probabilities. Another, more general, method (call it GM), which does not assume independence in the sense just specified, is this:

$$p(E_{i_1} \,\&\, \cdots \,\&\, E_{i_n}) =$$
$$[p(H_1) \cdot p(E_{i_1}|H_1) + \cdots + p(H_m) \cdot p(E_{i_1}|H_m)] \cdot$$
$$[\boldsymbol{p(H_1|E_{i_1})} \cdot p(E_{i_2}|H_1) + \cdots + \boldsymbol{p(H_m|E_{i_1})} \cdot p(E_{i_2}|H_m)] \cdot \ \cdots \ \cdot$$
$$[\boldsymbol{p(H_1|E_{i_1}, \ldots, E_{i_{n-1}})} \cdot p(E_{i_n}|H_1) + \cdots + \boldsymbol{p(H_m|E_{i_1}, \ldots, E_{i_{n-1}})} \cdot p(E_{i_n}|H_m)].$$

Note, now, that if the hypotheses and events are of the sort we considered in the dynamic Dutch book examples, antecedent and consequent of all the formulas in boldface are—we assumed for the sake of van Fraassen's argument—explanatory dependent. How shall we interpret these formulas: according to equation (2), or rather according to (3)?

Van Fraassen, in his argument, must *either* assume that the answer cannot but be (2) *or* that Peter will consider SMR the correct method for calculating initial probabilities in case SMR and GM should give different results. For Peter, who favors an unspecified non-Bayesian update rule, and the bookie are said to agree on the initial probabilities of the hypotheses on which the bets are made and hence on what prices are fair for these bets (van Fraassen 1989:168). But that is not at all evident. As is shown in the appendix, SMR and GM are equivalent methods for computing initial probabilities in the cases we consider if, *and only if*, the conditional probabilities that one encounters when one uses

relevant to the 'epistemics' of conditional probabilities. In it he says: "If two people are arguing 'If p will q?' and are both in doubt as to p, they are adding p hypothetically to their stock of knowledge and arguing on that basis about q.... We can say they are fixing their degrees of belief in q given p". If Ramsey is right and this is how we come to our conditional probabilities, then in general (3) will be respected quite automatically, for the process Ramsey describes is neutral with respect to the rule we use for updating our beliefs (there seems to be no reason to expect someone to use one rule when updating 'hypothetically', and another when updating 'actually'). Of course this is not to say that it is excluded that at the moment we really come to know p we may not want to assign q the probability we earlier thought we would give it in that case—e.g. an EXPL-advocate might in the meantime have changed his opinion about how good an explanation for p q is. But as long as such changes of opinion are not made according to some preset rule, this can never lead one into a dynamic Dutch book situation.

[5] More generally: $p(E_{i_1} \,\&\, \cdots \,\&\, E_{i_n}) = [p(H_1) \cdot p(E_{i_1}|H_1) \cdot \cdots \cdot p(E_{i_n}|H_1)] + \cdots + [p(H_m) \cdot p(E_{i_1}|H_m) \cdot \ \cdots \ \cdot p(E_{i_n}|H_m)]$.

GM are calculated according to Bayes' theorem. And Peter, if he accepts (3) and relies on GM as the principal method for calculating initial probabilities, will not regard the prices for which the bookie offers the bets as fair. In fact, if he does so, no dynamic Dutch book can be made against him at all.

Let me explain why that is so by means of the Dutch book presented in this paper. What the Dutch bookie in that case exploited was the fact that your initial probability for tails turning up in the third toss after twice heads in the foregoing tosses diverged from what you would, given your update rule, assign to B in case heads should indeed turn up in the first two tosses. But that could not have happened had you calculated initial probabilities according to GM and interpreted the conditional probabilities encountered in that process in accordance with (3). The relevant conditional probabilities—the instantiations of the boldface formulas above—would (*ceteris paribus*—cf. note 4), in the process of calculating your initial probabilities for A and $A \,\&\, B$, have obtained exactly the value each H_i would actually get were the first respectively first and second toss to come up heads and were you to use EXPL as your update rule. On this method calculating initial probabilities is something like going through the process of updating hypothetically, 'pushed into the fantasy' that, in our case, both the first and the second toss come up heads. As a result, the predictable divergence the bookie so cunningly made use of can never arise. It might be said that this method of computing initial probabilities *harmonizes* with one's method of revising beliefs. SMR does not in general harmonize in this way: for the Bayesian it does on condition that the events are indepedent on any of the hypotheses under investigation; for the non-Bayesian on the extra condition that the events are explanatory independent of the various hypotheses.

Thus the non-Bayesian, at least if he wants to avoid a dynamic Dutch book being made against him, is to be consistent in his preference for his non-Bayesian rule in the sense that he use a method of computation for determining initial probabilities that harmonizes with his method for revising beliefs when the data come in. As we saw, the advice could be made precise: whether you favor Bayes' rule or some other, don't give up equation (1) *and* in case of conflict between SMR and GM, rely on GM. This, I think, is the true lesson to be learned from the Lewis/Teller/van Fraassen-argument.

But the story cannot end here. For, as de Finetti has shown, unless your probability of $A \,\&\, B$ equals your probability of A times your conditional probability of B given A, you will be vulnerable to a static Dutch book. So we may presume you want to respect that equation. Now consider again our three bias hypotheses $H_{1\text{–}3}$. As can readily be checked, if you update in some non-Bayesian way and follow our advice, it does *not* hold for all H_i that $p(H_i) \cdot p(\text{first toss heads}|H_i) = p(\text{first toss heads}) \cdot p(H_i|\text{first toss heads})$. But this means that you will assign two different probabilities to, e.g., $p(H_3 \,\&\, \text{first toss heads})$. Let these probabilities be x and y, and let $x > y$. Then you will regard $\$x$ a fair price to buy a bet that pays \$1 if H_3 is true and the first toss comes up heads, but you will also think it fair to sell the very same bet for $\$y$. Evidently, if you actually buy and sell the bet at those prices, you're guaranteed to lose $\$x - y$. Apparently you can be Dutch-booked anyway. Have we, then, not just traded one problem for another?

Well, what we have really done is trade a problem Maher's 'look before you leap' principle can *not* handle for one it *can*. As Earman (1992:49) notes, commenting on Maher's argument against the dynamic Dutch book theorem: "In essence, the decision-theoretic message is to look before you leap. Such advice is just as valid in the synchronic setting as in the diachronic or multitemporal setting". This was written before Skyrms had shown that, although perhaps valid, the advice to look before you leap was unhelpful in the diachronic case. However, in the synchronic case it is evidently unproblematical: simply check, whenever you want to make a bet, that it does not lead to a sure loss in combination with bets you have already made. Quite evidently, if you follow this principle, no static Dutch book can be made against you. Perhaps in practice it really isn't that simple to follow the advice, but since we know exactly from where the inconsistencies come a rule like: 'in case A and B are explanatory dependent, refrain from making bets on $A \,\&\, B$' (or some refinement thereof), which may be more workable, will also do.[6]

In sum, the proper response for the proponent of IBE to the Lewis/Teller/van Fraassen-argument is not to give up IBE, but to extend his epistemic strategy with two further principles, *viz.* 'harmony' and the 'look before you leap' principle. That strategy will allow him to infer to the best explanation free of charge. Hence the dynamic Dutch book theorem is false. Of course the IBE-proponent still faces the huge task to establish that the proposed epistemic strategy does indeed better (in whatever respects he thinks desirable) than strategies including Bayes' rule or any other rule for belief change. The current paper offers no help on that. However, it does show, I hope, that attempts to justify IBE are not *a priori* wasted effort.

Appendix

Let $H_1, \ldots, H_n$ be a set of mutually exclusive and jointly exhaustive hypotheses such that for no H_i $p(H_i) = 0$. Let $E_1, \ldots, E_n$ be events such that each E_j has various probabilities conditionally on the various hypotheses and such that both the Bayesian and the non-Bayesian agree that, given any H_i, they are independent. Furthermore, it is not the case that Bayes' rule and whatever rule the non-Bayesian uses agree on $p_{E_j}(H_i)$ for all i, j. (This is the situation considered in both van Fraassen's and our dynamic Dutch book example.) Then the following holds:

Proposition:
SMR and GM are equivalent methods for calculating $p(E_{i_1} \,\&\, \cdots \,\&\, E_{i_k})$ for any i, k if and only if conditional probabilities are calculated according to Bayes' theorem.

Proof:
Sufficiency is easy; we prove necessity. For simplicity, we only consider the case where we have two hypotheses, H_1 and H_2, which are assumed to be jointly exhaustive and mutually exclusive, and two outcome events E_1 and E_2 that are independent in the above sense; the generalization is straightforward however.

[6] Earman (*ibid.*) wonders whether, although (as he then thought) the 'look before you leap' principle can guard us against dynamic Dutch bookies, the fact that the prices for the various bets these bookies offer all seem fair to us should not be taken to indicate "that there is something amiss in [our] opinion/preference structure". Our case might give rise to the same worry. However, it is important to note that, unlike Maher, I present the 'look before you leap' prinicple as part of an epistemic strategy. Such a strategy is to be judged on the (de-)merits it has relative to its rivals. And it might well be that, notwithstanding the fact that it may lead to inconsistencies, the proposed strategy does *overall* better than any of its contenders. If so, it seems we would do good to base our 'opinion/preference structure' on it.

If we calculate conditional probabilities not according to Bayes' theorem but according to the formula representing some probabilistic updating rule at variance with Bayes' rule, then for all i, j, k, n:

$$p(H_i|E_k) = \frac{p(H_i) \cdot p(E_k|H_i)}{\sum_{j=1}^{n} p(H_j) \cdot p(E_k|H_j)} + f(H_i, E_k),$$

where f is a function from hypotheses and evidence (and possibly other things as well) to [-1, 1] such that $f(H_i, E_k) = -\sum_{j \neq i}^{n} f(H_j, E_k)$, and it is not the case that for all i, k $f(H_i, E_k) = 0$ (for then our updating rule would no longer be at variance with Bayes' rule). Suppose, then, towards a contradiction, that SMR and GM are equivalent. Then the following equations must hold:

$[p(E_1|H_1) \cdot p(E_2|H_1) \cdot p(H_1)] + [p(E_1|H_2) \cdot p(E_2|H_2) \cdot p(H_2)] =$
$[p(H_1) \cdot p(E_1|H_1)) + (p(H_2) \cdot p(E_1|H_2))] \cdot [(p(H_1|E_1) \cdot p(E_2|H_1)) + (p(H_2|E_1) \cdot p(E_2|H_2))] =$
$[p(H_1) \cdot p(E_1|H_1)) + (p(H_2) \cdot p(E_1|H_2))] \cdot [((((p(H_1) \cdot p(E_1|H_1))/(p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2))) + f(H_1, E_1)) \cdot p(E_2|H_1)) + ((((p(H_2) \cdot p(E_1|H_2))/(p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2))) + f(H_2, E_1)) \cdot p(E_2|H_2))] =$
$[(p(H_1) \cdot p(E_1|H_1) \cdot p(E_2|H_1))/(p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2)) + f(H_1|E_1) \cdot p(E_2|H_1)] + [(p(H_2) \cdot p(E_1|H_2) \cdot p(E_2|H_2))/(p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2)) + f(H_2|E_1) \cdot p(E_2|H_2)] =$
$[p(H_1) \cdot p(E_1|H_1) \cdot p(E_2|H_1)] + [f(H_1, E_1) \cdot p(E_2|H_1) \cdot (p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2))] + [p(H_2) \cdot p(E_1|H_2) \cdot p(E_2|H_2)] + [f(H_2, E_1) \cdot p(E_2|H_2) \cdot (p(H_1) \cdot p(E_1|H_1) + p(H_2) \cdot p(E_1|H_2))]$.

Since (i) $f(H_1, E_1) = -f(H_2, E_1)$, and (ii) $p(E_2|H_1) \neq p(E_2|H_2)$, it must be that $f(H_1, E_1) = f(H_2, E_1) = 0$. That means Bayes' rule and the non-Bayesian rule do not disagree over $p_{E_1}(H_i)$. But we can interchange E_1 and E_2 and then it follows by the same reasoning that they cannot disagree over $p_{E_2}(H_i)$ either. This contradicts our assumption that the rules are at variance with each other. Hence SMR and GM are not equivalent. □

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