Reply to Crupi et.al.'s 'Bayesian Confirmation by Uncertain Evidence' ([2008])

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Crupi et al. ([2008]) propose a generalization of Bayesian confirmation theory that they claim to adequately deal with confirmation by uncertain evidence. Consider a series of points of time $t_0, \ldots, t_i, \ldots, t_n$ such that the agent's subjective probability for an atomic proposition E changes from $\Pr_0(E)$ at t_0 to … to $\Pr_i(E)$ at t_i to … to $\Pr_n(E)$ at t_n. It is understood that the agent's subjective probabilities change for E and no logically stronger proposition, and that the agent updates her subjective probabilities by Jeffrey conditionalization. For this specific scenario the authors propose to take the difference between $\Pr_0(H)$ and $\Pr_i(H)$ as the degree to which E confirms H for the agent at time t_i (relative to time t_0), $C_{0,i}(H, E)$. This proposal is claimed to be adequate, because

$$C_{0,i}(H, E) < C_{0,n}(H, E) \text{ if both } \Pr_0(E) < \Pr_i(E) < \Pr_n(E) \text{ and } \Pr_0(H) < \Pr_0(H|E).$$

The authors show the last proposition to hold for all "Pr-incremental" measures of confirmation $C_{0,i}(H, E)$, that is, all functions that depend only on $\Pr_0(H)$ and $\Pr_i(H)$ and that are increasing in $\Pr_i(H)$ and non-decreasing in $\Pr_0(H)$. Examples include the distance measure, the ratio measure, the odds or log-likelihood ratio measure, and the normalized distance measure (Crupi et al. [2008], section 2).

I agree that, from a Bayesian point of view, the authors' proposal adequately deals with confirmation by uncertain evidence. In fact, for the specific scenario described above this is what I claim myself in section 11, pp. 111f, of Huber ([2005])[1], even though I arrive at this conclusion in a somewhat different way. However, the account by Crupi et al. ([2008]) is more general than my stance on this specific situation. More importantly, the authors also distinguish between two crucially different readings of my claim (H) that '[i]f some E speaks in favour of some H–say, because it is a logical consequence of the latter–then […] getting to know that E is probably true should provide confirmation for H*–and the more probable it is that E is true, the more it should do so*.' (Huber [2005], p. 105, italics added) Therefore their paper helps to clarify an ambiguity in mine, and it provides an important contribution to Bayesian confirmation theory.

However, the target of my ([2005]) is not some specific issue–viz. confirmation by uncertain evidence–within Bayesian confirmation theory, but rather that theory itself. Crupi et al. ([2008]) reject–correctly, I think–one reading, called (H.2), of my claim (H). They develop a general Bayesian account of confirmation by uncertain evidence based on a second reading, called (H.1). I argue that Bayesian confirmation theory gets things wrong if it adopts the reading (H.2) rejected by the authors, and that it is subject to a triviality charge if it adopts the reading (H.1) accepted by the authors.

More specifically, I use a feature of (H.1) to prove the following result: for any subjective probability measure $\Pr_i$ at time t_i, for any hypothesis H, and any atomic evidence E that is relevant to H (in the sense of $\Pr_i$), there are assignments of probabilities

[1] There is an unfortunate typographical error at the bottom of p. 112, where the probability measure $\Pr_i$ should not be conditional on E. However, this is clear from what I say on the pages following that paragraph and does not seem to have misled the authors.

Pr_0, Pr_0*, and Pr_0** such that E confirms H at time t_i (relative to time t_0) if the agent starts with Pr_0 as her first assignment, E disconfirms H at time t_i (relative to time t_0) if the agent starts with Pr_0* as her first assignment, and E neither confirms nor disconfirms H at time t_i (relative to time t_0) if the agent starts with Pr_0** as her first assignment. (This result holds for all Pr-incremental measures as well.)

To illustrate, consider a community of scientists that has come to agree on the subjective probabilities to be assigned to the hypotheses of their interest. That is the situation of 'objectivity as inter-subjective agreement for opinions that fall short of certainty' (Earman [1992], p. 138). If these scientists also agree on the (cognitive) utilities they assign to these hypotheses and if the decision rule they use is determined by their probabilities and utilities (as is the case if they maximize expected utility), they accept, reject, and laugh at the very same hypotheses–even though they disagree as to whether the data that have driven their agreement is to be called 'incrementally confirming' or 'incrementally disconfirming' or neither (let alone to what degree).

I am happy to concede that positive probabilistic relevance is the correct explication of the explicandum 'confirmation'. If there were one and only one, I would even be happy to concede that the normalized distance measure (or, for that matter, the odds or log-likelihood ratio measure) is the correct explication of the explicandum 'degree of confirmation'. What I do not concede is that the very concept of (degree of) confirmation explicated in this–or, in case of degrees of incremental confirmation, any other–way is of any use. In fact, I claim the contrary, for this concept sees disagreement where there is nothing but agreement (with regard to all that matters).

The history of confirmation theory is, to a large extent, a history of triviality results. Hempel ([1945]) shows confirmation to be trivial if it satisfies certain conditions of adequacy. Goodman ([2006/1954]) shows confirmation to be trivial if it is construed in purely syntactical terms. Bayesian confirmation theory escapes these triviality results since its notion of incremental confirmation violates Hempel's conditions, and expressions that are syntactically alike need not be alike in their probabilities.

In Huber ([2005]) I argue that Bayesian confirmation theory is nevertheless subject to a triviality charge: we can incrementally confirm everything by anything atomic and relevant as long as we choose an appropriate prior. That result depends on a particular treatment of uncertain evidence that I claim the Bayesian is forced to adopt if she wants to get things right. I take the fact that the authors' general account of confirmation by uncertain evidence yields this treatment for the special scenario I focus on to show that my original triviality charge still holds.

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