A NEW GARBER-STYLE SOLUTION TO THE PROBLEM OF OLD EVIDENCE

STEPHAN HARTMANN AND BRANDEN FITELSON

1. Background: Garber, Jeffrey and Earman

By the time Einstein had formulated the general theory of relativity (H), the evidence regarding the perihelion of Mercury (E) — which Newtonian theory was unable to adequately explain — had long been known (Roseveare 1982). Indeed, it is not implausible to suppose that Einstein was *certain* (in 1915) that E was true. Nonetheless, it is widely accepted that Einstein learned *some* proposition X (in 1915) which had the effect of confirming H (*i.e.*, rationally raising Einstein's credence in H).

Garber (1983) proposes that what Einstein learned was a *logical* fact (*i.e.*, that X = "H entails E"). By adding an additional atomic statement "X" to the H, E-language (and interpreting "X" extra-systematically as "H entails E"), Garber showed how it was *possible* to write down Bayesian models of this sort, having the following desired confirmation-theoretic property

$$(\dagger) \qquad \Pr(H \mid X) > \Pr(H).$$

Garber did not, however, endorse specific constraints on $\Pr(\cdot)$, which ensure (†).

Subsequently, Jeffrey (1992) and Earman (1992) offered such constraints. Their approaches require the addition of X *and* a fourth atomic statement "Y" to the H, E-language, where Y is extra-systematically interpreted as "H entails $\neg E$" (*viz.*, H *refutes* E). In these traditional Garber-style approaches, the background extra-systematic constraints on $\Pr(\cdot)$ consist of the following pair of "*modus ponens* principles" for the extra-systematic entailment relation.[1]

$$(1) \qquad \Pr(E \mid H \,\&\, X) = 1$$

$$(2) \qquad \Pr(E \,\&\, H \,\&\, Y) = 0$$

In addition to (1) and (2), Jeffrey offers the following extra-systematic constraint:

$$(3) \qquad \Pr(X \vee Y) = 1$$

Informally, (3) expresses *certainty* in the disjunction: *either* H entails E *or* H refutes E. Given the background assumption that $\Pr(E) = 1$, it is straightforward

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[1]Traditionally, Garber-style approaches also rest on the background assumption that $\Pr(E) = 1$. In standard probability theory, this background assumption entails (1). We have chosen to state (1) explicitly here, since it reflects the fact that X is (in traditional Garber-style approaches) couched in terms of *entailment*. In the next section, we explain how to relax both of these assumptions.

to show that (1)–(3) jointly entail the desired confirmation-theoretic conclusion (†). This, in essence, was Jeffrey's Garber-style approach to the old evidence problem.

As Earman rightly points out, Jeffrey's constraint (3) is not very plausible as a rational stricture on Einstein's credences, prior to his learning X (in 1915). There was no good reason for Einstein to be *certain* (prior to learning X) that H either entails or refutes E. Earman offers the following alternative extra-systematic constraint:

$$(4) \qquad \Pr(H \mid X) > \Pr(H \mid \neg X \,\&\, \neg Y)$$

Earman shows that — given the background assumption that $\Pr(E) = 1$ — (1), (2) and (4) jointly entail (†); and, he argues that (4) is a plausible assumption regarding Einstein's credences (in 1915). We agree that (4) is *more* plausible than Jeffrey's (3). For one thing, (4) is not a *numerical* constraint, but merely an *ordinal* constraint on Einstein's 1915 credences. Moreover, because Einstein was (antecedently) *certain* of E, it is reasonable to suppose that he would have judged that X confers a greater probability on H than $\neg X \,\&\, \neg Y$ does. Having said that, we would prefer a more general approach, which (a) doesn't presuppose that $\Pr(E) = 1$; and, (b) doesn't require interpreting X and Y in terms of *entailment* relations. In the next section, we describe just such an approach.

2. A New Garber-Style Approach

We like Garber's idea of adding a pair of extra-systematic statements (and extra-systematic credal constraints) to the H, E-language. But, we think the existing implementation of this general strategy has two main shortcomings. First, we think interpreting "X" and "Y" as "H *entails* E" and "H *refutes* E" is unduly restrictive. It is more plausible to suppose that what is learned in cases of old evidence (*viz.*, X) may not (always) be a *logical* fact. To be more precise, let "X" and "Y" be interpreted as follows:

- $X \stackrel{\text{def}}{=} H$ *adequately explains* (or *accounts for*) E.[2]
- $Y \stackrel{\text{def}}{=} H$'s best competitor ($H'$) *adequately explains* (or *accounts for*) E.[3]

What really matters here is not whether H *entails* E (or $\neg E$), but whether H *adequately explains* (or *accounts for*) E and/or whether the some *alternative* theory H' (which is H's *best competitor*, *fn.* 3) adequately explains (or accounts for) E. It *may* be that H adequately explains E in a *deductive-nomological* sense. But, why not allow for the possibility that H (and/or H') explains E in a *non*-deductive-nomological way? In this regard, we think that the original Garber-style approaches are too narrow in their explanatory scope.

[2]We are not the first to consider this sort of generalization of Garber's approach. Garber himself (1983, 112) considers some alternative interpretations of X and Y, which have a more general explanatory flavor. However, all of the alternatives Garber mentions involve some pattern of *entailment* relations between the salient propositions. So, Garber's account(s) would still be restricted to forms of explanation that supervene on deductive entailment relations. Moreover, Garber never works out any of these alternatives in any detail. Hartmann (2014) uses general explanatory language to interpret X and Y. Our approach is intended as a simplification of Hartmann's original idea (which is more complex, theoretically). A new paper by Jan Sprenger (Sprenger 2015) also appeals to explanatory relations, but in a different way.

[3]When we say H' is H's "best competitor," we mean that H' is H's best competitor *with respect to explaining/predicting phenomenon E* — *e.g.*, in our Mercury example, H was general relativity, H' was Newtonian theory, and E was the evidence (available in 1915) regarding the perihelion of Mercury.

A second problem with the traditional Garber-style approaches is that they have required extra-systematic credal constraints [*e.g.*, $\Pr(E) = 1$, (2), and (3)] which are implausibly strong. This defect is also remedied by moving to our alternative, explanatory extra-systematic interpretation of X and Y. Consider the following four ordinal constraints on $\Pr(\cdot \mid \cdot)$.

(5.1) $$\Pr(H \mid X \,\&\, \neg Y) > \Pr(H \mid \neg X \,\&\, \neg Y)$$

(5.2) $$\Pr(H \mid X \,\&\, \neg Y) > \Pr(H \mid \neg X \,\&\, Y)$$

(5.3) $$\Pr(H \mid X \,\&\, Y) > \Pr(H \mid \neg X \,\&\, Y)$$

(5.4) $$\Pr(H \mid X \,\&\, Y) \geq \Pr(H \mid \neg X \,\&\, \neg Y)$$

Let's examine each of four constraints, in turn. Suppose that H adequately explains E, but its best competitor H' does *not*. Constraints (5.1) and (5.2) assert that H is more probable, given *this* supposition ($X \,\&\, \neg Y$) than it is given *either* the supposition that *neither* H *nor* H' adequately explains E (*i.e.*, given $\neg X \,\&\, \neg Y$) *or* the supposition that H's best competitor (H') adequately explains E, but H does *not* ($\neg X \,\&\, Y$). These two constraints seem uncontroversial.

Constraint (5.3) also seems quite plausible. It asserts that H is *less* probable, given the supposition that its best competitor (H') adequately explains E, but H does *not* ($\neg X \,\&\, Y$) than it is given the supposition that *both* H *and* its best competitor (H') adequately explain E (*i.e.*, given $X \,\&\, Y$).

The fourth and final credal comparison (5.4) says that H is *at least as* probable, given the supposition that *both* H *and* its best competitor (H') adequately explain E (*i.e.*, given $X \,\&\, Y$) as it is given the supposition that *neither* H *nor* H' adequately explains E (*i.e.*, given $\neg X \,\&\, \neg Y$). One might maintain that it would be reasonable to rank $\Pr(H \mid X \,\&\, Y)$ *strictly* higher in one's comparative confidence ranking than $\Pr(H \mid \neg X \,\&\, \neg Y)$. After all, $X \,\&\, Y$ implies that H *does* adequately explain E (which is already known), whereas $\neg X \,\&\, \neg Y$ implies that H does *not* adequately explain E. On the other hand, one might also reasonably argue that these two suppositions ($X \,\&\, Y$ and $\neg X \,\&\, \neg Y$) place H and H' *on a par* with respect to explaining E, and so they shouldn't confer different probabilities on H. Both of these positions are compatible with (5.4). The only thing (5.4) rules out is the claim that H is *more* probable given $\neg X \& \neg Y$ than it is given $X \& Y$. So, (5.4) is also eminently reasonable.

As it happens, the desired (Garberian) confirmation-theoretic conclusion (†) follows from (5.1)–(5.4) *alone*. To be more precise, we can prove the following result.

Theorem. (5.1)–(5.4) jointly entail (†).

Proof. Let $\mathfrak{a} \overset{\text{def}}{=} \Pr(H|X\&\neg Y)$, $\mathfrak{b} \overset{\text{def}}{=} \Pr(H|X\&Y)$, $\mathfrak{c} \overset{\text{def}}{=} \Pr(H|\neg X\&\neg Y)$, $\mathfrak{d} \overset{\text{def}}{=} \Pr(H|\neg X\&Y)$, $x \overset{\text{def}}{=} \Pr(\neg Y \mid X)$, and $y \overset{\text{def}}{=} \Pr(\neg Y \mid \neg X)$. Given these assignments, (5.1)–(5.4) are:

(5.1) $$\mathfrak{a} > \mathfrak{c}$$

(5.2) $$\mathfrak{a} > \mathfrak{d}$$

(5.3) $$\mathfrak{b} > \mathfrak{d}$$

(5.4) $$\mathfrak{b} \geq \mathfrak{c}$$

Suppose that $x > 0$ and $y < 1$. Then, (5.1)–(5.4) jointly entail

$$\mathfrak{a}x + \mathfrak{b}(1 - x) > \mathfrak{c}y + \mathfrak{d}(1 - y).$$

And, by the law of total probability, we have:

$$\Pr(H \mid X) = \mathfrak{a}x + \mathfrak{b}(1 - x)$$
$$\Pr(H \mid \neg X) = \mathfrak{c}y + \mathfrak{d}(1 - y)$$

Thus, (5.1)–(5.4) jointly entail $\Pr(H \mid X) > \Pr(H \mid \neg X)$, and $\Pr(H \mid X) > \Pr(H)$. □

We think our Theorem undergirds a superior Garber-style approach to the problem of old-evidence. Specifically, our approach has the following two distinct advantages over traditional Garber-style approaches.

(i) Unlike previous Garber-style approaches, ours does not require the assumption that $\Pr(E) = 1$. It may be true that our constraints (5.1)–(5.4) are *most* plausible given the background assumption that E is (antecedently) known *with certainty*. But, we think (5.1)–(5.4) retain enough of their plausibility, given only the weaker assumption that E is (antecedently) known with *high credence*.[4]

(ii) Our approach is not restricted to cases in which H (and/or H') explains E *in a deductive-nomological way*. That is, our approach covers all cases in which scientists come to learn that their theory *adequately explains* E, not only those cases in which scientists learn that their theory *entails* E (or explains E *deductive-nomologically*).[5]

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[4] If we relax the standard Garberian assumption that $\Pr(E) = 1$, then we'll need to specify what happens to the probability of E, when X is learned. Intuitively, the probability of E should not change during this learning process. As a result, models of such a learning event may need to be more sophisticated than our present (naïve strict conditionalization) approach would suggest.

[5] We have not said anything here about the nature of scientific explanation. This is intentional, as we would prefer to remain as neutral as possible on this score. Having said that, all we really need to presuppose (dialectically) is that our approach is compatible with *some non-deductive* approaches to scientific explanation. If that presupposition is correct, then we will have succeeded in pushing Garber's main ideas farther than previous authors (except for Hartmann 2014) who have written (Garber-style) on the old evidence problem. We think this presupposition is quite plausible. For instance, we think it is clear that our approach is compatible with various (objective) non-deductive theories of scientific explanation, *e.g.*, so-called *inductive-statistical* and *statistical-relevance* approaches (Salmon 1989). One might (still) worry that our approach is not compatible with (subjective) Bayesian accounts of scientific explanation, on the grounds that such an interpretation of our approach would require "higher order subjective probabilities" (since we'd have to assign subjective probabilities to explanatory relations that themselves involve subjective probabilities). This is an interesting worry, but we suspect that our approach can be made compatible *even* with subjective Bayesian accounts of scientific explanation, provided proper care is taken to model higher order credences (Skyrms 1980).

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Munich Center for Mathematical Philosophy, Ludwig Maximilians-Universität, München
E-mail address: `S.Hartmann@lmu.de`
URL: `http://stephanhartmann.org/`

Philosophy Department, Rutgers University, New Brunswick
E-mail address: `branden@fitelson.org`
URL: `http://fitelson.org/`