GOODMAN'S "NEW RIDDLE"

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ABSTRACT. First, a brief historical trace of the developments in confirmation theory leading up to Goodman's infamous "grue" paradox is presented. Then, Goodman's argument is analyzed from both Hempelian and Bayesian perspectives. A guiding analogy is drawn between certain arguments against classical deductive logic, and Goodman's "grue" argument against classical inductive logic. The upshot of this analogy is that the "New Riddle" is not as vexing as many commentators have claimed (especially, from a Bayesian *inductive-logical* point of view). Specifically, the analogy reveals an intimate connection between Goodman's problem, and the "problem of old evidence". Several other novel aspects of Goodman's argument are also discussed (mainly, from a Bayesian perspective).

1. PREHISTORY: NICOD & HEMPEL

In order to fully understand Goodman's "New Riddle," it is useful to place it in its proper historical context. As we will see shortly, one of Goodman's central aims (with his "New Riddle") was to uncover an interesting problem that plagues Hempel's [17] theory of confirmation. And, Hempel's theory was inspired by some of Nicod's [25] earlier, inchoate remarks about instantial confirmation, such as:

> Consider the formula or the law: A entails B. How can a particular proposition, [*i.e.*] a fact, affect its probability? If this fact consists of the presence of B in a case of A, it is favourable to the law ... on the contrary, if it consists of the absence of B in a case of A, it is unfavourable to this law.

By "is (un)favorable to," Nicod meant "raises (lowers) the inductive probability of". In other words, Nicod is describing a *probabilistic relevance* conception of confirmation, according to which it is postulated (roughly) that positive instances are probability-raisers of universal generalizations. Here, Nicod has in mind *Keynesian* [21] inductive probability (more on that later). While Nicod is not very clear on the logical details of his probability-raising account of instantial confirmation (stay tuned for both Hempelian and Bayesian precisifications/reconstructions of Nicod's remarks), three aspects of Nicod's conception of confirmation are apparent:

- Instantial confirmation is a relation between singular and general propositions/statements (or, if you will, between "facts" and "laws").

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- Confirmation consists in *positive probabilistic relevance*, and disconfirmation consists in *negative probabilistic relevance* (where the salient probabilities are inductive in the Keynesian [21] sense).
- Universal generalizations are confirmed by their positive instances and disconfirmed by their negative instances.

Hempel [17] offers a precise, logical reconstruction of Nicod's naïve instantial account. There are several peculiar features of Hempel's reconstruction of Nicod. I will focus presently on two such features. First, Hempel's reconstruction is completely *non*-probabilistic (we'll return to that later). Second, Hempel's reconstruction takes the relata of Nicod's confirmation relation to be *objects* and universal statements, as opposed to *singular statements* and universal statements. In modern (first-order) parlance, Hempel's reconstruction of Nicod can be expressed as:

(NC_0) For all objects x (with names $\mathbf{x}$), and for all predicate expressions ϕ and ψ:
x confirms $\ulcorner(\forall y)(\phi y \supset \psi y)\urcorner$ iff $\ulcorner\phi\mathbf{x} \,\&\, \psi\mathbf{x}\urcorner$ is true, and
x disconfirms $\ulcorner(\forall y)(\phi y \supset \psi y)\urcorner$ iff $\ulcorner\phi\mathbf{x} \,\&\, {\sim}\psi\mathbf{x}\urcorner$ is true.[1]

As Hempel explains, (NC_0) has absurd consequences. For one thing, (NC_0) leads to a theory of confirmation that violates the *hypothetical equivalence condition*:

(EQC_H) If x confirms H, then x confirms anything logically equivalent to H.

To see this, note that — according to (NC_0) — both of the following obtain:

- a confirms "$(\forall y)(Fy \supset Gy)$," provided a is such that $Fa \,\&\, Ga$.
- *Nothing* can confirm "$(\forall y)[(Fy \,\&\, {\sim}Gy) \supset (Fy \,\&\, {\sim}Fy)]$," since *no object* a can be such that $Fa \,\&\, {\sim}Fa$.

But, "$(\forall y)(Fy \supset Gy)$" and "$(\forall y)[(Fy \,\&\, {\sim}Gy) \supset (Fy \,\&\, {\sim}Fy)]$" are *logically equivalent*. Thus, (NC_0) implies that a confirms the hypothesis that all Fs are Gs *only if this hypothesis is expressed in a particular way*. I agree with Hempel that this violation of (EQC_H) is a compelling reason to reject (NC_0) as an account of instantial confirmation.[2] But, I think Hempel's reconstruction of Nicod is uncharitable on this score (more on that later). In any event, my main focus here will be on the theory Hempel ultimately ends-up with. For present purposes, there is no need to examine Hempel's [17] ultimate theory of confirmation in its full logical glory. For our subsequent discussion of Goodman's "New Riddle," we'll mainly need just the following two properties of Hempel's confirmation relation, which is a relation between *statements* and statements, as opposed to *objects* and statements.[3]

[1] Traditionally, such conditions are called "Nicod's Criteria", which explains the abbreviations. Historically, however, it would be more accurate to call them "Keynes's Criteria" (KC), since Keynes [21] was really the first to explicitly endorse the three conditions (above) that Nicod later championed.

[2] Not everyone in the literature accepts (EQC_H), especially if (EQC_H) is understood in terms of *classical* logical equivalence. See [28] for discussion. I will simply assume (EQC_H) throughout my discussion. I will not defend this assumption (or its evidential counterpart, which will be discussed below), which I take to be common ground in the present historical dialectic.

[3] (NC) is clearly a more charitable reconstruction of Nicod's (3) than (NC_0) is. So, perhaps it's most charitable to read Hempel as taking (NC) as his reconstruction of Nicod's remarks on instantial confirmation. In any case, Hempel's reconstruction still ignores the *probabilistic* aspect of Nicod's account. This will be important later, when we discuss probabilistic approaches to Goodman's "New Riddle". At that time, we will see an even more charitable (Bayesian) reconstruction of Nicod's remarks.

(NC) For all constants x and for all (independent) predicate expressions ϕ, ψ: $\ulcorner \phi x \,\&\, \psi x \urcorner$ confirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$ and $\ulcorner \phi x \,\&\, {\sim}\psi x \urcorner$ disconfirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$.

(M) For all constants x, for all (consistent) predicate expressions ϕ, ψ, and for all statements H: If $\ulcorner \phi x \urcorner$ confirms H, then $\ulcorner \phi x \,\&\, \psi x \urcorner$ confirms H.

As we will see, it is far from obvious whether (NC) and (M) are (intuitively) correct confirmation-theoretic principles (especially, from a *probabilistic* point of view!). Severally, (NC) and (M) have some rather undesirable consequences, and jointly they face the full brunt of Goodman's "New Riddle," to which I now turn.

2. History: Hempel & Goodman

Generally, Goodman [14] speaks rather highly of Hempel's theory of confirmation. However, Goodman [14, ch. 3] thinks his "New Riddle of Induction" shows that Hempel's theory is in need of rather serious revision/augmentation. Here is an extended quote from Goodman, which describes his "New Riddle" in detail:

> Now let me introduce another predicate less familiar than "green". It is the predicate "grue" and it applies to all things examined before t just in case they are green but to other things just in case they are blue. Then at time t we have, for each evidence statement asserting that a given emerald is green, a parallel evidence statement asserting that that emerald is grue. And the statements that emerald a is grue, that emerald b is grue, and so on, will each confirm the general hypothesis that all emeralds are grue. Thus according to our definition, the prediction that all emeralds subsequently examined will be green and the prediction that all will be grue are alike confirmed by evidence statements describing the same observations. But if an emerald subsequently examined is grue, it is blue and hence not green. Thus although we are well aware which of the two incompatible predictions is genuinely confirmed, they are equally well confirmed according to our present definition. Moreover, it is clear that if we simply choose an appropriate predicate, then on the basis of these same observations we shall have equal confirmation, by our definition, for any prediction whatever about other emeralds—or indeed about anything else. . . . We are left . . . with the intolerable result that anything confirms anything.

Before reconstructing the various (three, to be exact) arguments against Hempel's theory of confirmation that are implicit in this passage (and the rest of the chapter), I will first introduce some notation, to help us keep track of the logic of the arguments. Let $Ox \overset{\text{def}}{=} x$ is examined before t, $Gx \overset{\text{def}}{=} x$ is green, $Ex \overset{\text{def}}{=} x$ is an emerald. Now, Goodman defines "grue" as follows: $\mathfrak{G}x \overset{\text{def}}{=} Ox \equiv Gx$.[4] With these definitions in hand, we can state the two salient universal statements (hypotheses):

(H_1) All emeralds are green. More formally, H_1 is: $(\forall x)(Ex \supset Gx)$.

[4]Here, "$\equiv$" is the material biconditional. I am thus glossing the detail that unexamined-before-t grue emeralds are *blue* on Goodman's official definition. This is okay, since all that's *needed* for Goodman's "New Riddle" (at least, for its most important aspects) is the assumption that unexamined-before-t grue emeralds are *non*-green. Quine [26] would have objected to this reconstruction on the grounds that "non-green" is not a natural kind term (and therefore not "projectible"). Goodman did not agree with Quine on this score. As such, there is no real loss of generality here, from Goodman's point of view. In any event, I could relax this simplification, but that would just (unnecessarily) complicate most of my subsequent arguments. In the final section (on Goodman's "triviality argument"), I will return to this "blue *vs* non-green" issue (since in that context, the difference becomes important).

(H_2) All emeralds are grue. More formally, H_2 is: $(\forall x)(Ex \supset \mathfrak{G}x)$. Even more precisely, H_2 is: $(\forall x)[Ex \supset (Ox \equiv Gx)]$.

And, we can state three salient singular (evidential) statements, about an object a:

($\mathcal{E}_1$) $Ea \,\&\, Ga$. [a is an emerald and a is green]

($\mathcal{E}_2$) $Ea \,\&\, (Oa \equiv Ga)$. [a is an emerald and a is grue]

($\mathcal{E}$) $Ea \,\&\, Oa \,\&\, Ga$. [a is an emerald and a is both green and grue]

Now, we are in a position to look more carefully at the various claims Goodman makes in the above passage. At the beginning of the passage, Goodman considers an object a which is examined before t, an emerald, and green. He correctly points out that all three of $\mathcal{E}_1$, $\mathcal{E}_2$, and $\mathcal{E}$ are true of such an object a. And, he is also correct when he points out that — according to Hempel's theory of confirmation — $\mathcal{E}_1$ confirms H_1 and $\mathcal{E}_2$ confirms H_2. This just follows from (NC). Then, he seems to suggest that it therefore follows that "the observation of a at t" confirms *both* H_1 *and* H_2 "equally." It is important to note that this claim of Goodman's can only be correct if "the observation of a at t" is a *statement* (since Hempel's relation is a relation between *statements* and statements, *not* between "*observations*" and statements) which bears the Hempelian confirmation relation to *both* universal statements H_1 and H_2. Which instantial, evidential statement can play this role? Neither $\mathcal{E}_1$ nor $\mathcal{E}_2$ can, since Hempel's theory entails *neither* that $\mathcal{E}_1$ confirms H_2 *nor* that $\mathcal{E}_2$ confirms H_1. However, the *stronger* claim $\mathcal{E}$ *can* play this role, since:

(†) $\mathcal{E}$ confirms H_1 and $\mathcal{E}$ confirms H_2.

That Hempel's theory entails (†) is a consequence of (NC), (M), and the following *evidential* equivalence condition, which is also a consequence of Hempel's theory:

(EQC$_E$) If E confirms H, then anything logically equivalent to E also confirms H.

Figure 1, below, contains a visually perspicuous proof of (†) from (NC), (M), and (EQC$_E$). The single arrows in the figure are confirmation relations (annotated on the right with justifications), and the double arrows are entailment relations.

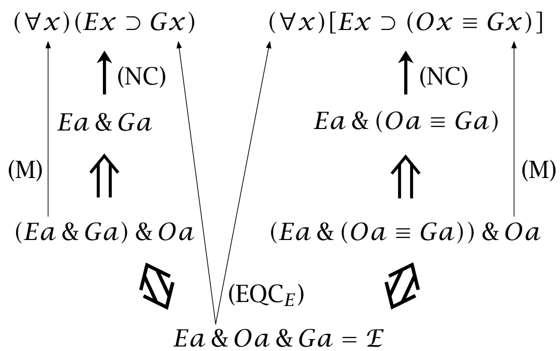

Figure 1. A Proof of (†) from (NC), (M), and (EQC$_E$).

Of course, the following weaker claim follows immediately from (†):

(‡) $\mathcal{E}$ confirms H_1 *if and only if* $\mathcal{E}$ confirms H_2.

Goodman is not only attributing the *qualitative* confirmation claim (‡) to Hempel's theory. He is also attributing the following *quantitative* (or *comparative*) claim:

($\star$) $\mathcal{E}$ confirms H_1 and H_2 *equally*.

Let's call Goodman's argument concerning (‡) his *qualitative* "grue" argument, and the argument concerning ($\star$) his *quantitative* argument. There is also a third argument toward the end of the above passage, which is Goodman's *triviality* argument. I will spend most of the rest of the paper discussing the qualitative argument, which I take to be the most important and interesting argument of the three.[5] Because I am most interested how Goodman's argument bears on *probabilistic* conceptions of confirmation, the remainder of the paper will largely be concerned with Bayesian confirmation theory (of various flavors). After giving careful Hempelian and Bayesian reconstructions of Goodman's qualitative argument in section 3, I will return to the quantiative argument (this time, just from a Bayesian point of view) in section 4. In section 5, I will conclude with a (more brief) discussion of Goodman's triviality argument, from both Hempelian and Bayesian perspectives.

3. Goodman's Qualitative Argument

3.1. **Prelude: Logic *vs* Epistemology in Goodman's Qualitative Argument.** Goodman is right, of course, that (‡) *is* entailed by Hempel's theory of confirmation.[6] And, Goodman *seems* to think that (‡) is intuitively *false*. Specifically, he *seems* to think that $\mathcal{E}$ confirms H_1 but $\mathcal{E}$ does *not* confirm H_2. As such, even this first argument (which comes before the quantitative and triviality arguments) is supposed to show that Hempel's theory of confirmation has some unintuitive consequences. On reflection, however, it's not clear precisely what is supposed to be unintuitive about (‡). After all, Hempel's confirmation relation is a *logical* relation. Is Goodman's worry here really a *logical* one? It seems to me that Goodman's worry here is really *epistemic*, rather than logical.[7] That is, I take it that Goodman's intuition here is *really* that $\mathcal{E}$ *is evidence for* H_1 for epistemic agents S in "grue" contexts, but $\mathcal{E}$ is *not* evidence for H_2 for epistemic agents S in "grue" contexts. Notice that this intuition is about an *epistemic* relation (*evidential support*) in "grue" contexts, *not* a *logical* relation (the Hempelian *confirmation* relation). Understood in this way, Goodman's argument is *enthymematic* — it's missing a crucial premise to serve as

[5]In his initial paper on "grue" [13], Goodman's arguments do not involve universal hypotheses at all (*i.e.*, in [13], Goodman's discussion is *purely instantial*). I will not discuss those early purely instantial arguments here (although, in my discussion of his later "triviality" argument in the final section of this paper, some instantial confirmation-theoretic considerations will arise). I'm focusing here on Goodman's later arguments (which happen to primarily involve universal hypotheses) for two reasons. First, Goodman is much clearer in [14] about his *methodology and aims*, *i.e.*, on what the argument is supposed to show and how it is supposed to show it (see *fn. 7*). Second, the purely instantial arguments in [13] are flawed in some elementary ways that have nothing to do with where I think the deep and important problems with Goodman's underlying methodology and aims lie.

[6]As for ($\star$), Hempel's theory [17] does not have quantitative or comparative consequences, so it is *neutral* on ($\star$). However, it can be shown that Hempel & Oppenheim's quantitative extension of Hempel's qualitative theory [19] does imply ($\star$). I omit the details of this in order to conserve space. But, I will analyze ($\star$) from a Bayesian point of view (in some detail) in section 4, below.

[7]Goodman [14, pp. 70–72] makes it quite clear that what's *at stake* here are the principles of inductive *logic* (in the sense intended by Hempel). But, Goodman's *methodology* for rejecting such principles is one that appeals to *epistemic* intuitions. This is most clear at [14, pp. 72–73], when Goodman discusses an example that is a prelude to "grue". There, he says (concerning an instantial E and a universal H) that E "does not increase the credibility of" H "and so does not confirm" H.

a *bridge principle* connecting the logical relation of confirmation with the epistemic relation of evidential support.[8] Here, it is helpful to consider an analogy.

A (naïve) "relevance logician" might be tempted to offer the classical logician the following "*reductio*"[9] of classical deductive logic (*viz.*, a "paradox of entailment"):

(1) For all sets of statements X and all statements p, if X is logically inconsistent, then p is a logical consequence of X.
(2) If an agent S's belief set B entails a proposition p (and S *knows* that $B \vDash p$), then it would be reasonable for S to infer/believe p.[10]
(3) *Even if* S knows that their belief set B is inconsistent (and, on this basis, they also know that $B \vDash p$, for any p), there are (nonetheless) *some* p's such that it is *not* the case that it would be reasonable for S to infer/believe p.
(4) Therefore, since (1)–(3) lead to absurdity, our initial assumption (1) must have been false — *reductio* of the classical "explosion" rule (1).

Of course, a classical logician need not reject (1) in response to this argument. As Harman's [15] and Macfarlane's [22] discussions suggest, the classical logician may instead choose to reject premise (2). It is premise (2) that plays the role of *bridge principle*, connecting the *logical* concept (entailment) and the *epistemic* concept (inference, or reasonable belief) that feature in the relevantist's argument. Without such a bridge principle, there is no way to get classical logic to have the undesirable "epistemic consequences" that seem to be worrying the relevantist. The Harman-esque strategy here for responding to the relevantist is to argue that (2) is implausible, in precisely the sort of cases that are at issue in the argument. If an agent's beliefs B are inconsistent (and they know this), the proper response *may* be to reject some of the members of B, rather than inferring anything (further) from B. For the remainder of the paper, I will use this Harman-esque approach as a guiding analogy. In fact, I will argue that this strategy is in some ways *even more effective* in the inductive case, for two reasons. First, in the *inductive* case, the worries are even more clearly *epistemic* in nature. And, second, there is a sense (to be explained below) in which Goodman's qualitative argument (against Bayesianism) relies even more heavily on a bridge principle that the Bayesians should reject for independent reasons. Before applying this analogy to the Bayesian case (which will be my main focus here), I will begin with a brief discussion of what form it might

[8]I borrow the term "bridge principle" from John MacFarlane [22], who also takes an epistemic stance with respect to arguments of this kind. In the case of deductive logic (the so-called "paradoxes of entailment"), MacFarlane [22] offers epistemic reconstructions much like the ones I am using here.

[9]Of course, relevantists don't like *reductio* arguments, but the aim of this argument is to *convince a classical logician* who *does* accept *reductio*. I'll assume this is dialectically kosher.

[10]Note: this is just a "*straw man*" bridge principle. The point of the deductive example is *merely to set-up an analogy* with the inductive case. Of course, to make the deductive analysis more interesting, we'd have to consider much more subtle bridge principles [22]. That's not the point of this paper. Where *we* will need to take greater care is in the *inductive* case. That's what we aim to do, below. That said, I should also note that in contexts where S's beliefs are inconsistent, I doubt that *any* deductive bridge principle (no matter how sophisticated) will serve the relevantist's purposes. Specifically, I suspect the relevantist faces a dilemma: Any bridge principle will either be *false* (while perhaps being strong enough to make their *reductio* classically valid), or it will be *too weak* to make their *reductio* classically valid (while perhaps being true). The naïve bridge principle (2) stated above falls under the first horn of this dilemma. More plausible bridge principles will (I bet) not yield a valid *reductio*.

take with respect to Goodman's qualitative argument against Hempelian confirmation theory. Here is an analogous (albeit somewhat sketchy) reconstruction of Goodman's qualitative argument against Hempelian confirmation theory:

(1′) Hempel's theory of confirmation [specifically, property (‡)].

(2′) Some bridge principle connecting Hempelian confirmation and evidential support. Presumably, it will say something like: if E Hempel-confirms H, then (*ceteris paribus*[11]) E is evidence for H for S in C.

(3′) $\mathcal{E}$ is evidence for H_1 for an agent S in a "grue" context, but $\mathcal{E}$ is *not* evidence for H_2 for an agent S in a "grue" context.

(4′) Therefore, since (1′)–(3′) lead to absurdity, our initial assumption (1′) must have been false — *reductio* of Hempelian inductive logic (1′)/(‡).

Perhaps a Hempelian could try to save their confirmation theory by blaming (2′) for the unintuitive (apparent) "epistemic consequences" brought out by Goodman's qualitative argument. In the end, I don't think this Harman-esque strategy is very promising for the Hempelian confirmation theorist.[12] But, I won't dwell on the (dim) Hempelian prospects of responding to Goodman's qualitative argument, Harman-style. Instead, I will focus on Goodman's qualitative argument, as reconstructed from a *Bayesian* point of view. As we will see shortly, *all* Bayesian confirmation-theorists face a powerful (*prima facie*) qualitative challenge from Goodman, but one that can be parried (or so I will argue) Harman-style.

3.2. **The Bayesian Conception of Confirmation — Broadly Construed.** Like Nicod, contemporary Bayesians explicate confirmation in terms of *probabilistic relevance*. But, unlike Nicod, most contemporary Bayesians (a notable exception being Patrick Maher [23]) don't use inductive (or logical) probabilities in their explication of confirmation. Rather, contemporary Bayesians tend to use *credences* or *epistemically rational degrees of belief*, which are assumed to obey the probability calculus. As it turns out, this difference makes no difference for Goodman's *qualitative* argument (properly reconstructed), since the qualitative argument will go through for *any* probability function whatsoever (independently of its origin or interpretation). As such, for the present discussion (of the *qualitative* argument[13]), I will work with the following very broad characterization of Bayesian confirmation theory, which will apply to basically any approach that uses probabilistic relevance of any kind (this includes just about every plausible flavor that's been on the market since Keynes).

> E confirms H, relative to K iff $\Pr(H \mid E \,\&\, K) > \Pr(H \mid K)$, where $\Pr(\cdot \mid \cdot)$ is *some* "confirmation-theory suitable" conditional probability function.

Notice how the Bayesian confirmation relation is explicitly *three*-place (at least, once a "suitable" Pr is *fixed*), whereas Hempel's was two-place. In fact, Hempel's

[11] Depending on one's epistemological scruples, one will require various *ceteris paribus* conditions to be satisfied here. For instance, one might require that S *knows* that E Hempel-confirms H in C, and/or that S *possesses* E as evidence in C, *etc.* I will say more about such *ceteris paribus* conditions on the *Bayesian* side, below. This sketchy Hempelian story is just a prelude to the Bayesian analysis.

[12] This is because (a) Hempel does not even seem to *realize* he *needs* a bridge principle in order to *apply* his confirmation theory, and (as a result) there is little indication as to how such a principle would or should have been formulated, and (b) Hempel's theory entails (M), which makes the formulation of any remotely plausible bridge principles quite difficult (by Hempel's own lights [6]).

[13] As we'll see below, even the *quantitative* argument is less sensitive to the choice of confirmation-theoretic probability function Pr than one might have thought (or than some seem to think).

relation can be understood as three-place as well, but it would not make a substantive difference to his theory. This is because Hempel's theory (which is based on *entailment* properties of E) has no way to distinguish the following two types [6]:

- Conditional Confirmation: E confirms H, relative to K.
- Conjunctive, Unconditional Confirmation: $E \,\&\, K$ confirms H (relative to $\top$).

Bayesian confirmation theory, which — unlike its Hempelian predecessor — is *non*-monotonic in *both E and K* (more on this below), treats these two types of confirmation claims in radically different ways. This is why its confirmation relation must be defined as *explicitly* three-place. Before we give a precise, Bayesian reconstruction of Goodman's qualitative argument, it's worthwhile to reconsider the crucial (controversial) Hempelian conditions (NC) and (M), from a Bayesian perspective.

3.3. **The Hempelian Properties (NC) and (M) from a Bayesian Point of View.** In a Bayesian framework, neither (NC) nor (M) is generally true. Let's focus on (NC) first. I.J. Good [11] was the first to describe background corpora K relative to which (*e.g.*) $Ea \,\&\, Ga$ does *not* confirm $(\forall x)(Ex \supset Gx)$. Here's a Good-style example:

> Let the background corpus K be: Exactly one of the following two hypotheses is true: (H_1) there are 100 green emeralds, no non-green emeralds, and 1 million other things in the universe [*viz.*, $(\forall x)(Ex \supset Gx)$], or ($\sim H_1$) there are 1,000 green emeralds, 1 blue emerald, and 1 million other things in the universe. And, let $\mathcal{E}_1 \overset{\text{def}}{=} Ea \,\&\, Ga$, where a is an object randomly sampled from universe.
>
> In such a case (assuming a standard random sampling model Pr):
>
> $$\Pr(\mathcal{E}_1 \mid H_1 \,\&\, K) = \frac{100}{1000100} < \frac{1000}{1001001} = \Pr(\mathcal{E}_1 \mid \sim H_1 \,\&\, K)$$
>
> So, in such a context, the "positive instance" $Ea\&Ga$ actually *dis*confirms the corresponding universal generalization $(\forall x)(Ex \supset Gx)$.

This *seems* to show that (NC) is false, from a Bayesian point of view. But, in Hempel's response to Good's paper [18], he explains that (NC) was always meant to be interpreted in a way that would rule-out such corpora K as illegitimate. Specifically, Hempel [18] suggests that the proper understanding of (NC) is as follows:

($NC_\top$) For all constants x and for all (independent) predicate expressions ϕ, ψ: $\ulcorner \phi x \,\&\, \psi x \urcorner$ confirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$ *relative to a priori corpus* $K_\top$, and $\ulcorner \phi x \,\&\, \sim\psi x \urcorner$ disconfirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$, *relative to* $K_\top$.

The idea behind ($NC_\top$) is that the salient confirmation relations here are supposed to be relations between evidence and hypothesis *in the absence of any other (empirical) evidence.* And, since Good's example presupposes a substantial amount of other (background) empirical evidence, this is supposed to undermine Good's example as a counterexample to (NC). I have discussed this dialectic between Hempel and Good elsewhere [6], and I won't dwell on it too much here. To make a long story short, Hempel's ($NC_\top$)-challenge to Good has recently been met by Patrick Maher, who shows [23] that even ($NC_\top$) is not generally true from a Bayesian perspective. Maher's counterexample to ($NC_\top$), which makes use of Carnapian inductive probabilities, is similar in structure to an example described by Good [12] in his original response to Hempel. The details of this dispute about (NC) and ($NC_\top$) are not important for our present purposes. What's important for us is that (NC) is not sacrosanct from a Bayesian point of view. Specifically, the two crucial Bayesian morals here are: (i) whether a "positive instance" confirms a universal generalization will depend sensitively on the background corpus relative to which it is taken, and (ii) "positive instances" need not confirm universal generalizations even relative to "tautological" or "*a priori*" background corpora. I will return to these crucial morals shortly, when I discuss Goodman's claim (‡). But, first, I will briefly discuss the (im)plausibility of (M), from a Bayesian point of view.

The Hempelian *monotonicity* property (M) is *even more suspect* than (NC). Indeed, *Hempel himself* had some compelling intuitions about confirmation that ran *counter to* (M)! In his original resolution (or dissolution) of the raven paradox [17] (and in his response to Good [18], discussed above), Hempel appeals to an intuitive feature of confirmation (or inductive support) that *directly contradicts* (M). I have discussed this inconsistency in Hempel's writings on the raven paradox in detail elsewhere [6]. The salient point for our present purposes is that there are reasons to reject (M) that are *independent* of Goodman's "New Riddle" — reasons that *Hempel himself* would have seen as compelling. Moreover, as in the case of (NC), there are also compelling *Bayesian* reasons for rejecting (M). Bayesian counterexamples to (M) are quite easy to come by. And, to my mind, counterexamples to (M) are much simpler and more compelling than counterexamples to instantial principles, such as (NC). Here's one: let $Bx \overset{\text{def}}{=} x$ is a black card, let $Ax \overset{\text{def}}{=} x$ is the ace of spades, and let $Jx \overset{\text{def}}{=} x$ is the jack of clubs. Assuming (K) that we sample a card a at random from a standard deck (thus, we take Pr to be the standard probability function for such a model), we have the following probabilistic facts:

- Ba confirms Aa, relative to K, since $\Pr(Aa \mid Ba \& K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
- $Ba \& Ja$ *dis*confirms (indeed, *refutes*!) Aa, relative to K, since:

$$\Pr(Aa \mid Ba \& Ja \& K) = 0 < \frac{1}{52} = \Pr(Aa \mid K)$$

It seems pretty clear (and nowadays uncontroversial) that (M) is an undesirable property for a confirmation relation to have (and Hempel himself was sensitive to this in his discussions of the raven paradox [6]). Moreover, (M) is highly implausible for reasons that seem to be *independent* of Goodman's "New Riddle".

In the next section, I will discuss Goodman's central qualitative confirmation-theoretic claim (‡), from a Bayesian point of view.

3.4. Goodman's Claim (‡) from a Bayesian Point of View.

3.4.1. *What **are** "Positive Instances" Anyway?* Before we start thinking about Goodman's (‡) from a Bayesian perspective, it is useful to re-think the notion of "positive instance" that has been at work (historically) in this instantial confirmation literature. So far, we have been talking about *conjunctions* $\ulcorner \phi a \,\&\, \psi a \urcorner$ as "positive instances" of universal generalizations $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$. But, from a *logical* point of view, instances of $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$ are *conditionals* $\ulcorner \phi a \supset \psi a \urcorner$. So, let's add the two salient conditional claims to our stock of singular statements:

($\mathcal{E}_3$) $Ea \supset Ga$. [either a is not an emerald or a is green]

($\mathcal{E}_4$) $Ea \supset (Oa \equiv Ga)$. [either a is not an emerald or a is grue]

From a logical point of view, then, it is $\mathcal{E}_3$ *not* $\mathcal{E}_1$ that is an instance of H_1, and it is $\mathcal{E}_4$ *not* $\mathcal{E}_2$ that is an instance of H_2. Interestingly, Hempel's theory *also* entails that $\mathcal{E}_3$ confirms H_1 and that $\mathcal{E}_4$ confirms H_2. So, from a Hempelian point of view, *both* conjunctions $\ulcorner \phi a \,\&\, \psi a \urcorner$ *and* conditionals $\ulcorner \phi a \supset \psi a \urcorner$ are *confirming instances* of

universal generalizations $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$. From a Bayesian point of view, however, only the *conditionals* are *guaranteed* to be *confirming* instances of universal generalizations, relative to *all* background corpora K and all (regular[14]) probability functions Pr. This is because (*i*) universal claims *entail* their *conditional*-instances, but they do *not* entail their "conjunction-instances", and (*ii*) if H entails E, then H and E will be *positively correlated* under any (regular) probability function. This explains why it is *possible* to generate Good-style counterexamples to (NC). If by "positive instance" the historical commentators on confirmation theory had meant *conditional* statements of the form $\ulcorner \phi a \supset \psi a \urcorner$, then no Bayesian counterexamples to (NC) would have been (logically) possible. This shows just how important (and strong!) assumption (M) is. Assumption (M) is *so* strong that the conjunction of (M) and (NC) is equivalent to the conjunction of (M) and the following, *innocuous* (from a Bayesian stance) instantial principle of confirmational relevance:

($NC_{\supset}$) For all constants x and for all (independent) predicate expressions ϕ, ψ:
$\ulcorner \phi x \supset \psi x \urcorner$ confirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$, and
$\ulcorner \phi x \,\&\, {\sim}\psi x \urcorner$ disconfirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$.[15]

I suspect that some of the perplexity raised by Goodman's discussion arises out of unclarity about precisely what a "positive instance" is. And, I think this unclarity in the writings of Hempel and Goodman is caused mainly by their implicit acceptance of evidential montonicity (M), which obscures the distinction between conditional-instances *vs* conjunctive-instances of universal statements.[16] Be that as it may, let's continue on with our (Bayesian) scrutiny of Goodman's claim (‡).

3.4.2. *Bayesian Scrutiny of Goodman's Claim (‡).* Goodman's claim (‡) says that $\mathcal{E}$ confirms H_1 iff $\mathcal{E}$ confirms H_2. While it is true that *Hempel's* theory entails (‡), it is *not* true that *Bayesian* confirmation theory — broadly construed — entails (‡). In fact, using a technique similar to I.J. Good's [11], we can describe background corpora K (and probability functions Pr) relative to which $\mathcal{E}$ confirms H_1, but $\mathcal{E}$ *dis*confirms H_2. Here is one such Good-style, Bayesian counter-example to (‡):

> Let the background corpus K be: Exactly one of the following two hypotheses is true: (H_1) there are 1000 green emeralds 900 of which have been examined before t, no non-green emeralds, and 1 million other things in the universe [*viz.*, $(\forall x)(Ex \supset Gx)$], or ($H_2$) there are 100 green emeralds that have been examined before t, no green emeralds that have not been examined before t, 900

[14]Here, I am assuming that all evidential and hypothetical statements (all E's and H's) receive non-extreme *unconditional* probability on all salient probability functions (a reasonable assumption here). Hereafter, I will assume that we're talking about *regular* probability functions in this sense. The importance of this regularity assumption will become clear when we get to "old evidence", below.

[15]Notice how Nicod's three principles are *consequences* of Bayesian confirmation theory, if "positive instance" is understood in the *conditional* sense (as opposed to the *conjunction* sense). This provides a much more charitable reading of Nicod (as a *Bayesian*) than Hempel's reconstruction of Nicod (as a deductivist). Moreover, these considerations provide *further* reason to worry about (M).

[16]My conjecture is that this insidious adherence to (M) is a vestige of Hempel's uncharitable "objectual" reconstruction (NC_0) of Nicod's instantial confirmation principle. If one thinks of objects or observations (rather than statements) as doing the confirming, one can easily be led into thinking that (M) makes sense. For instance, one might start saying things like "observations of green emeralds *are* observations of emeralds, therefore anything confirmed by the observation that a is an emerald is also confirmed by the observation that a is a green emerald." Such (monotonic) inferences are (intuitively) not cogent in general. Fallacies like this underlie many errors in confirmation theory.

non-green emeralds that have not been examined before t, and 1 million other things in the universe (*viz.*, $(\forall x)[Ex \supset (Ox \equiv Gx)]$). Let $\mathcal{E} \stackrel{\text{def}}{=} Ea \& Oa \& Ga$, for a randomly sampled from the universe.[17]

In such a case (assuming a suitable sampling model Pr — see *fn.* 17):

$$\Pr(\mathcal{E} \mid H_1 \& K) = \frac{900}{1001000} > \frac{100}{1001000} = \Pr(\mathcal{E} \mid H_2 \& K)$$

Since H_1 and H_2 are mutually exclusive and exhaustive (given K), it follows that $\mathcal{E}$ confirms H_1, but *dis*confirms H_2 (relative to K).

Of course, all this shows is that there is a *possible* background corpus K/probability function Pr relative to which $\mathcal{E}$ confirms H_1 and disconfirms H_2. That is enough to show that (unlike Hempel's theory) Bayesian confirmation theory — broadly construed — does not *entail* (‡). But, it is also true that Bayesian confirmation theory *is consistent with* (‡). As we will see shortly, there are *also* corpora K/probability functions Pr relative to which $\mathcal{E}$ *does* confirm H_1 *if and only if* it confirms H_2.

At this point in the dialectic, a Hempelian might implore the Bayesian (as Hempel [18] did to Good [11], in the context of the raven paradox) to determine whether or not (‡) is true relative to *a priori* background corpus $K_\top$ and "*a priori*" probability function $\Pr_\top$. But, only those Bayesians (like Patrick Maher [23]) who believe in inductive probabilities (as Nicod did) are likely to take such a challenge seriously. Most modern Bayesians do not have a theory of "confirmation relative to *a priori* background" to appeal to here.[18] But, *historically* — *i.e.*, at the time Hempel and Goodman were thinking about "grue" — almost everyone who used probability to explicate the concept of confirmation used inductive or logical probability (as opposed to subjective or epistemic probability) for this explication. So, it is important to try to work out what the Bayesian confirmation relations look like, when *inductive* probabilities are used in "impoverished" Goodmanian contexts (*i.e.*, assuming *a priori* background $K_\top$). Interestingly, like ($\text{NC}_\top$), (‡) is *not* a consequence of (later[19] Carnapian) confirmation theories based on salient accounts of "inductive" or "logical" probability. By emulating the structure of my Good-style counterexample to (‡) above in later Carnapian models (with three families of predicates), it can be shown that (‡) is *not* a consequence even of a "Carnapian" theory of confirmation,

[17] Here, to make things concrete, assume the following. For each object in the universe, there is a slip of paper on which is inscribed a true description of the object in terms of whether it exemplifies the properties E, O, and G. Imagine an urn containing all the slips. Assume a standard random sampling model Pr of the process, and suppose the slip that reads "$Ea \& Oa \& Ga$" is thus sampled.

[18] Although Timothy Williamson does propose something *like* this in his [29, ch. 9]. See below.

[19] The *early* Carnapian systems of [2] and [3] *do* entail ($\text{NC}_\top$) and (‡). However, the *later* Carnapian systems (*e.g.*, those discussed in [23] and [24]) do *not* entail ($\text{NC}_\top$) or (‡). Maher's [23] counterexample shows that ($\text{NC}_\top$) can fail in later Carnapian systems. And, examples similar to my Good-style counterexample to (‡) above can be emulated in later (*e.g.*, [24]) Carnapian systems (details omitted).

based on "inductive" or "logical" probabilities.[20] In this sense, Goodman's argument seems to be *even weaker* than the relevantist's (even if we replace the "*straw man*" bridge principle (2) with a real contender), since it will have to lean more heavily on its "bridge principle". In any event, as we'll see next, *even if* (‡) *were* a consequence of such a theory, Goodman's qualitative argument would remain *unsound* from a "logic-Bayesian" (or any other Bayesian) point of view, because it (still) rests on an *independently* implausible bridge principle. That brings us (finally) to our (Harman-style) Bayesian reconstruction of Goodman's qualitative argument.

3.5. **A Proper Bayesian Reconstruction of Goodman's Qualitative Argument.** Before we give a precise reconstruction of Goodman's qualitative argument against Bayesian confirmation theory (in "*reductio*" form, as above), we need one more ingredient — a bridge principle to connect the formal/logical concept of *confirmation* and the epistemic concept of *evidential support*. The requisite bridge principle can be found in Carnap's work on *applied* inductive logic. Carnap was an advocate of what we might call *logic-Bayesianism*. Like Hempel, Carnap thought that confirmation (now, understood as probabilistic relevance) was a logical relation between statements. Unlike Hempel, Carnap realized that in order to *apply* confirmation in actual epistemic contexts C, we need a *bridge principle* that tells us when *confirmatory* statements constitute *evidence* for hypotheses (for *agents* S *in* C). Carnap [2] discusses several bridge principles of this kind. The most well-known of these (and the one needed for Goodman's argument) is stated by Carnap as follows:

> **The Requirement of Total Evidence**. In the application of inductive logic to a given knowledge situation, the total evidence available must be taken as a basis for determining the degree of confirmation.

Most modern Bayesians have (usually only implicitly, and without argument) accepted the following precisification of Carnap's requirement of total evidence:

(RTE) E evidentially supports H *for an epistemic agent* S *in a context* C if and only if E confirms H, relative to K, where K is S's *total evidence*[21] *in* C.

This principle (RTE) plays a crucial role in Goodman's qualitative argument against Bayesian confirmation theory, which we can now carefully reconstruct, as follows:

(i) E confirms H, relative to K iff $\Pr(H|E\&K) > \Pr(H|K)$, for some "suitable" Pr. [The "*reductio*" assumption: the formal Bayesian account of confirmation.]

(ii) E evidentially supports H *for an epistemic agent* S *in a context* C if and only if E confirms H, relative to K, where K is S's *total evidence in* C. [(RTE)]

(iii) Agents S in Goodmanian "grue" contexts C_G have Oa as part of their total evidence in C_G. [*i.e.*, S's total evidence in C_G (K) *entails* Oa.]

[20] This is a subtle issue, since Maher [24] argues that all of the existing Carnapian theories of inductive probability for three families of predicates (*viz.*, for multiple properties) are *inadequate*. As such, Maher probably wouldn't endorse such Carnapian counterexamples to (‡). However, the existence of these Carnapian counterexamples to (‡) seems to have little or nothing to do with the reasons why Maher rejects the latest Carnapian theories of inductive probability for multiple properties. Moreover, there are *intuitive* analogues of these formal Carnapian counterexamples to (‡) [for instance, the Good-style counterexample to (‡) that I describe above]. I think these considerations have the effect of shifting the burden of proof back to the Hempelian/Goodmanian *defender* of (‡).

[21] I will not offer an analysis of "S's total evidence in C" here. One might think of this as S's *background knowledge* in C, where this is *distinct* from the *evidence* S possesses in C. Or, one might follow Williamson [29] and *identify* an agent's knowledge in C with the evidence they possess in C. I'll remain neutral on this question here, but I'll stick with Carnap's locution (for historical reasons).

(iv) If $K \vDash Oa$, then—*ceteris paribus*[22]—$\mathcal{E}$ confirms H_1 relative to K iff $\mathcal{E}$ confirms H_2 relative to K, for *any* Pr-function. [*i.e.*, if $K \vDash Oa$, then (‡).]

(v) Therefore, $\mathcal{E}$ evidentially supports H_1 for S in $C_{\mathcal{G}}$ if and only if $\mathcal{E}$ evidentially supports H_2 for S in $C_{\mathcal{G}}$.

(vi) $\mathcal{E}$ evidentially supports H_1 for S in $C_{\mathcal{G}}$, but $\mathcal{E}$ does *not* evidentially support H_2 for S in $C_{\mathcal{G}}$.

- Therefore, since (i)–(vi) lead to absurdity, our assumption (i) must be false — *reductio* of Bayesian confirmation theory (broadly construed)?

This argument is valid. So, let's examine each of its premises, working backward from (vi) to (ii). Premise (vi) is based on Goodman's *epistemic intuition* that, in "Grue" contexts, $\mathcal{E}$ evidentially supports H_1 but *not* H_2. I will just grant this assumption here (although, I think it could be questioned). Premise (v) follows logically from premises (i)–(iv). Premise (iv) is a theorem of the probability calculus. Premise (iii) is an assumption about the agent's background knowledge that's implicit in Goodman's set-up. Premise (ii) is (RTE). It's the *bridge principle*, akin to (2) in the relevantists' "*reductio*". This is the premise on which I will focus.

Intuitively, *some* precisification of Carnap's requirement of total evidence should be correct. The question at hand, however, is whether (RTE) is correct.[23] While many Bayesians have implicitly assumed (RTE), there are good reasons to think it must be rejected by Bayesian confirmation-theorists — reasons that are *independent* of "grue" considerations. There are various reasons for Bayesian confirmation-theorists to reject (RTE). I will only discuss what I take to be the most important of these here. As Tim Williamson [29, ch. 9] explicitly points out, (RTE) must be rejected by Bayesian confirmation theorists because of the *problem of old evidence* [10, 5]. This (now infamous) problem arises whenever E is *already* part of (entailed by) S's total evidence in C. In such a case, we will have $\Pr(E \mid K) = 1$, for *any* Pr. As a result, E will be powerless to confirm *any* H, relative to K — for *any* flavor of Bayesian confirmation theory. Thus, all Bayesian confirmation theorists must reject (RTE), on pain of having to say something absurd: that nothing that is part of S's total evidence can evidentially support any H for S. From this perspective, it can now be seen that Goodman's (qualitative) "grue" problem and the old evidence problem are related in the following way. In both "grue" and old-evidence contexts C, the agent's total evidence K is such that some undesirable evidential support relation is entailed (assuming only logic and probability theory) by (RTE).[24] Various

[22] Strictly speaking, premise (iv) also requires the following additional *ceteris paribus* condition: $\Pr(Ea \mid H_1 \,\&\, K) = \Pr(Ea \mid H_2 \,\&\, K)$. This assumption is standardly made by Bayesians who analyze "grue" [27]. I will assume it also. I am focusing on the role that $K \vDash Oa$ plays in the argument, since I think that's where the action is. Some Bayesians assume something stronger: $K \vDash Ea \,\&\, Oa$ [27]. I resist making this additional assumption here, since (a) it forces (†) to be true as well, and (b) it also makes Goodman's intuition (vi) seem far less intuitive (indeed, I would say it makes (vi) seem *false*).

[23] It is important to note that *Carnap* only endorsed (RTE) in connection with what he called "confirmation as firmness" — a *different* conception of confirmation, according to which E confirms H, relative to K iff $\Pr(H \mid E \,\&\, K) > r$, for some threshold value r. Carnap never explicitly endorsed (RTE) for "confirmation as increase in firmness" (his name for the probabilistic relevance conception). Indeed, Carnap never explicitly endorsed *any* precise bridge principle for the relevance conception. However, he did say some things that naturally *suggest* a different principle. I discuss this below.

[24] On the surface, it might seem as if old evidence cases and grue cases constitute counterexamples to *different directions* of (RTE). After all, old evidence cases are normally described as cases of support without confirmation, but grue cases are normally described (assuming Goodman is right,

ways of responding to the old evidence problem have been floated in the literature [5]. And, one might hope that some of these responses to old evidence could be applied to "grue". Unfortunately, most of the existing responses to the old evidence problem will not help with "grue". Something more radical is needed. What we need is a reformulation of (RTE) that avoids "grue", old evidence, and other related problems in one fell swoop. One approach that is suggested by the work of Carnap [2, *p*. 472] and Williamson [29, ch. 9] is to *radically* reformulate the (RTE), *and* to revert to *a priori* background/probability function $K_\top/\Pr_\top$ in the definition of confirmation itself. This would bring confirmation theory back to something more like the original (two-place) ideas that Nicod and Hempel had. Carnap called this sort of confirmation relation *initial confirmation*, which I will state as follows.[25]

> E initially-confirms H iff $\Pr_\top(H \mid E \,\&\, K_\top) > \Pr_\top(H \mid K_\top)$, where $\Pr_\top(\cdot \mid \cdot)$ is some "*a priori*" conditional probability function, and $K_\top$ is *a priori*.

Williamson suggests the following radical reformulation of (RTE) to go along with (his flavor of[26]) the Carnapian initial-confirmation relation:

(RTE$_\top$) E evidentially supports H for S in C iff S possesses E (as evidence) in C, and E initially-confirms H.

Richard Fumerton [9] discusses a similar approach, but he (being an epistemological internalist, unlike Williamson) also requires that S have some "epistemic access" to E, and to the fact that E initially-confirms H. These are fascinating recent developments in formal epistemology, but (owing to considerations of space) I won't be able to dwell on them further here.

Another approach (similar to some *subjective* Bayesian responses to the old evidence problem) would be to maintain the original Bayesian explication of confirmation (as *inherently three*-place, where the "suitable" probability function is just the agent's credence function at the time they are assessing the evidential support relations in question), and non-radically reformulate (RTE) so that it doesn't relativize to *all* of the agent's background knowledge K in C. The tricky thing about this sort of response is that it is very difficult to specify a *proper part* $K' \dashv K$ of S's total evidence in C that can avoid *all* the varieties of confirmational indistinguishability that one finds in the literature. One can think of all of these approaches as groping toward something like the following (seemingly *useless*) "bridge principle":

(RTE′) E evidentially supports H for S in C iff E confirms H, relative to K', where K' is the logically strongest statement such that: (a) S's total evidence in C entails K' [$K \vDash K'$], and (b) no undesirable evidential support relations are derivable from (RTE′) using logic and probability theory alone.

of course) as cases of confirmation without support. In fact, however, this *apparent* asymmetry is *merely* apparent, because the grue case can equally legitimately be described (assuming Goodman is right) as a case in which $\sim H_2$ is supported (by $\mathcal{E}$, for S, in $C_{\mathcal{G}}$) but not confirmed (by $\mathcal{E}$, relative to K).

[25] I am writing both $\Pr_\top$ and $K_\top$ here to emphasize that *nothing a posteriori* (other than E) is allowed to be conditionalized on, when we're using $\Pr_\top$ for confirmation-theoretic purposes. Here, I am following Carnap, who used the symbol "t" rather than "$K_\top$" for similar purposes.

[26] It may be somewhat unfair to saddle Williamson with a Carnapian view of the "suitable" probability function. Perhaps Williamson's "evidential probability function" is not exactly the sort of thing Carnap had in mind (*e.g.*, it may be more dynamic and have a slightly more psychologistic flavor). In any event, I think it's more appropriate to lump Williamson in with this sort of approach, as opposed to the sort of approach to old-evidence advocated by *subjective* Bayesians (discussed below).

The project of formulating a *useful* rendition of (RTE′) is one of the "holy grails" of contemporary subjective Bayesian confirmation theory. I'm not optimistic about the prospects for such an (RTE′). I think something more radical is needed. However, I also think that the Carnapian "initial confirmation" approach is unlikely to succeed, because it is unlikely that any adequate theory of "logical" or "inductive" probability is forthcoming. I suspect that we need a different conception of inductive logic altogether. Once inductive logic is properly reformulated, a sensible alternative to (RTE), ($\text{RTE}_\top$) and (RTE′) may be possible. I have some ideas about how to begin such a project, but they are beyond the scope of this paper.[27]

In the next section, I will examine Goodman's *quantitative* argument [for ($\star$)], from a Bayesian perspective.

4. Goodman's Quantitative Argument

4.1. **Goodman's "Equal Confirmation" Claim.** Goodman claims that formal theories of confirmation are stuck not only with (‡), but also with the implication that $\mathcal{E}$ confirms H_1 and H_2 "*equally*." In this section, I will simply *grant* (*arguendo*) that (‡) is true, and I will focus on the (independent) plausibility of the "equally" claim. I will interpret Goodman's "equally" claim as a *comparative* or *relational* confirmation claim. Specifically, I will read this "equal" claim as follows:

($\star$) $\mathfrak{c}(H_1, \mathcal{E} \mid K) = \mathfrak{c}(H_2, \mathcal{E} \mid K)$, where $\mathfrak{c}(H, E \mid K)$ is some Bayesian relevance measure of the degree to which E confirms H, relative to K ($\mathfrak{c}$ will be defined in terms of some "suitable" confirmation-theoretic probability function Pr).

Of course, what's *really* at issue here (as in the qualitative case above) is whether

(✡) $\mathcal{E}$ *evidentially supports* H_1 and H_2 equally strongly, *for* S *in* $C_{\mathcal{G}}$.

As in the qualitative case, (✡) cannot be established by confirmation theory alone. We also need a quantitative bridge principle, connecting quantitative confirmation, and quantitative evidential support. Presumably, the naïve principle would be:

($\text{RTE}_\mathfrak{c}$) The degree to which E evidentially supports H for S in C is $\mathfrak{c}(H, E \mid K)$, where $\mathfrak{c}$ is some (appropriate) relevance measure of degree of confirmation (defined in terms of a "suitable" confirmation-theoretic probability function Pr), and K is S's total evidence in C.

As above, it is *only if* we assume ($\text{RTE}_\mathfrak{c}$) that (✡) can be established. Moreover, whether (✡) can be so established will *also* depend on *which* measure $\mathfrak{c}$ is used.

As it turns out, there are various Bayesian relevance measures $\mathfrak{c}$ of degree of confirmation, and they tend to disagree radically on comparative (or relational) claims of the form $\mathfrak{c}(H_1, \mathcal{E} \mid K) \geq \mathfrak{c}(H_2, \mathcal{E} \mid K)$. I have discussed this disagreement in detail elsewhere [7]. Rather than attempting a general survey here, I will just say something about how the two most popular Bayesian relevance measures (for comparisons of this kind) behave with respect to Goodman's "New Riddle." The first measure I will discuss is the *ratio* measure $r(H, E \mid K) = \frac{\Pr(H \mid E \& K)}{\Pr(H \mid K)}$. When it comes to relational confirmation claims of this kind, the behavior of r is very simple, because r satisfies the so-called "Law of Likelihood" (see [7] for discussion):

(LL) $\mathfrak{c}(H_1, E \mid K) \geq \mathfrak{c}(H_2, E \mid K)$ iff $\Pr(E \mid H_1 \,\&\, K) \geq \Pr(E \mid H_2 \,\&\, K)$.

[27] In my [8], I sketch some preliminary ideas about how one might proceed here. I plan a much more extensive treatment of my preferred reformulation of inductive logic in a future monograph.

For this reason, I will refer to the r-perspective on (✡) as the *Likelihoodist* perspective. From a Likelihoodist perspective, the question about Goodman's ($\star$) reduces to a question about the *likelihoods* $\Pr(\mathcal{E} \mid H_1 \,\&\, K)$ and $\Pr(\mathcal{E} \mid H_2 \,\&\, K)$. And, once we assume ($\text{RTE}_\mathfrak{c}$), (✡) is guaranteed — from a Likelihoodist perspective — by the assumption that S's total evidence in C entails Oa. This was premise (iii) in the qualitative argument, which I have already granted to Goodman as part of the set-up of his example. It is easy to show that ($\text{RTE}_\mathfrak{c}$), (LL), and (iii) jointly entail (✡). This is because, if K entails Oa, then (*ceteris paribus*[28]) $\Pr(\mathcal{E} \mid H_1 \,\&\, K) = \Pr(\mathcal{E} \mid H_2 \,\&\, K)$. So, Likelihoodists who adopt a naïve bridge principle such as ($\text{RTE}_\mathfrak{c}$) are stuck with Goodman's quantitative claim (✡). Indeed, this is just the position that Sober [27] — a Likelihoodist who also endorses ($\text{RTE}_\mathfrak{c}$) — finds himself in. At this point, I could rehearse the various *independent* reasons (*e.g.*, the old evidence problem) why Bayesians should reject naïve bridge principles such as ($\text{RTE}_\mathfrak{c}$). But, since I have already covered that issue in the qualitative case, I won't (in the interest of space) bother going over it again here (although, there are some other issues that arise here, which are somewhat more subtle than the qualitative case). Instead, I will simply assume (for the rest of this section) that ($\text{RTE}_\mathfrak{c}$) is adopted, and I will focus on the *measure-sensitivity* of the above derivation of (✡), *given* ($\text{RTE}_\mathfrak{c}$).

Interestingly, not all Bayesian confirmation theorists are Likelihoodists. As a result, even if one accepts ($\text{RTE}_\mathfrak{c}$) and (iii), one need not accept (✡). Moreover, as I have argued elsewhere [7], Likelihoodism (LL) has some undesirable consequences. For instance, (LL) is incompatible with the following condition, which seems to me to be almost *constitutive* of the "provides better evidence for" (or "favors") relation:

($\mathfrak{f}$) If E provides conclusive evidential support for H_1 for S in C, but E provides non-conclusive evidential support for H_2 for S in C, then E *favors* H_1 over H_2 for S in C.[29]

I discuss this and other reasons for worrying about (LL) at length in my [7]. To make a long story short, it turns out that, among all the historical Bayesian accounts of the favoring relation, only one is compatible with ($\mathfrak{f}$), assuming ($\text{RTE}_\mathfrak{c}$). This leading rival to Likelihoodism is based on the *likelihood-ratio* measure $l(H, E \mid K) = \frac{\Pr(E \mid H \& K)}{\Pr(E \mid \sim H \& K)}$, rather than the measure r. Of course, the likelihood-ratio approach is *non-Likelihoodist*, because it does *not* satisfy the "Law of Likelihood" (LL). But, the l-based approach to confirmation satisfies the following, closely related condition:

($\text{LL}_\mathfrak{f}$) *If* $\Pr(E \mid H_1 \& K) \geq \Pr(E \mid H_2 \& K)$ *and* $\Pr(E \mid {\sim}H_1 \& K) < \Pr(E \mid {\sim}H_2 \& K)$, *then* $\mathfrak{c}(H_1, E \mid K) > \mathfrak{c}(H_2, E \mid K)$.[30]

What the l-based approach to confirmation reveals, therefore, is that [assuming ($\text{RTE}_\mathfrak{c}$)] favoring relations depend not only on the likelihoods of the hypotheses being contrasted $\Pr(\mathcal{E} \mid H_1 \& K)$ and $\Pr(\mathcal{E} \mid H_2 \& K)$, but also on the likelihoods of the *negations* of the hypotheses being contrasted $\Pr(\mathcal{E} \mid {\sim}H_1 \,\&\, K)$ and $\Pr(\mathcal{E} \mid {\sim}H_2 \,\&\, K)$.

[28] As I explained above (*fn.* 22), this (strictly speaking) also requires the *ceteris paribus* condition: $\Pr(Ea \mid H_1 \& K) = \Pr(Ea \mid H_2 \& K)$, which is assumed by Bayesians who analyze "grue". Many Bayesians assume something stronger: $K \vDash Ea \,\&\, Oa$, which entails $\Pr(\mathcal{E} \mid H_1 \,\&\, K) = \Pr(\mathcal{E} \mid H_2 \,\&\, K) = 1$ [27].

[29] In the background, here, I am assuming that if E entails H (and S knows this in C, and S possesses E in C, *etc.*), then E provides conclusive evidence for H for S in C. See [7] for discussion. In light of my present "Harmanian attitude," I need to be very careful here about this sort of principle (which looks like a bridge principle Harman would reject!). For the purposes of the present discussion, let's assume that we're not talking about contexts in which "paradox of entailment" type issues arise.

[30] Note: this is only a *sufficient* condition for favoring [assuming ($\text{RTE}_\mathfrak{c}$)], *not* a *necessary* condition.

I will call this second set of likelihoods *catch-alls*. Because of the dependence of favoring relations on catch-alls, a proper Bayesian analysis of Goodman's claim (✡) requires not only checking to see if $\Pr(\mathcal{E} \mid H_1 \& K) = \Pr(\mathcal{E} \mid H_2 \& K)$, but also looking at the relationship between the catch-alls $\Pr(\mathcal{E} \mid \sim H_1 \& K)$ and $\Pr(\mathcal{E} \mid \sim H_2 \& K)$ in Goodmanian contexts. This means we must think about our negations:

($\sim H_1$) Some emeralds are non-green.

($\sim H_2$) Some emeralds are non-grue. That is, some emeralds are *either* (*i*) examined prior to t and non-green *or* (*ii*) unexamined prior to t and green.

What if we could somehow motivate the claim that $\Pr(\mathcal{E} \mid \sim H_1 \& K) < \Pr(\mathcal{E} \mid \sim H_2 \& K)$, for a "suitable" confirmation-theoretic probability function Pr? Then, so long as we're not Likelihoodists, we could argue that — *even if* Goodman's qualitative claim (†) is granted — $\mathcal{E}$ *favors* the green hypothesis over the "grue" hypothesis. While this wouldn't undermine the main thrust of Goodman's argument (which is qualitative), it might be seen to "soften the impact" of the paradox by showing that $\mathcal{E}$ provides *better evidence* for H_1 than it does for H_2 in "grue" contexts. At this point in the dialectic, those sympathetic to Goodman's quantitative argument [27] will be quick to point out that the only way we can have *both*

- $\Pr(\mathcal{E} \mid H_1 \& K) = \Pr(\mathcal{E} \mid H_2 \& K)$, *and*
- $\Pr(\mathcal{E} \mid \sim H_1 \& K) < \Pr(\mathcal{E} \mid \sim H_2 \& K)$

is if $\Pr(H_1 \mid K) > \Pr(H_2 \mid K)$ is also true. Indeed, this is just a theorem of the probability calculus, and so it holds for *all* Pr. What this means is that any such non-Likelihoodist attempt to deny Goodman's quantitative claim (in the "intuitive" direction) will boil-down to a claim that H_1 is more probable "*a priori*" (prior to $\mathcal{E}$) than H_2. As Sober [27] explains, this does not seem like a very satisfying way of denying Goodman's quantitative claim (✡). On such a story, the evidence $\mathcal{E}$ is not playing any real role in the differential confirmation that $\mathcal{E}$ provides for H_1 *vs* H_2. What we seem to have here is simply some sort of "*a priori*" prejudice against "grue" hypotheses enshrined in Bayesian formalism.[31] This wouldn't be much different (in spirit) than Goodman's own "entrenchment" line on the paradox, and it wouldn't be very illuminating either.

Having said all that, I should remind you that I have already argued that Bayesians should reject the (RTE) — in both its qualitative and quantitative forms[32] — for reasons that are *independent* of "grue". As such, while the above discussion of the quantitative argument has some interesting wrinkles, I don't think it takes anything away from the diagnosis/resolution I've already offered Bayesians above. In the next section, I will briefly discuss Goodman's "triviality" argument — the third and last argument implicit in that infamous passage from *Fact, Fiction & Forecast*.

5. Goodman's "Triviality" Argument

Goodman does not stop with the two confirmation-theoretic claims (†) and (⋆). He goes on (in the passage quoted above) to claim that, in the context of Hempel's

[31]In this sense, Goodman's quantitative problem is far more difficult for the Bayesian to solve than the quantitative versions of the ravens paradox. See [6] for a novel quantiative approach to the ravens paradox that is much more promising than anything one can say (quantitatively) about "grue".

[32]If one rejects (RTE), then one must also reject ($\text{RTE}_{\mathfrak{c}}$). After all, the qualitative (positive) confirmation relation just corresponds to some inequality constraint on $\mathfrak{c}$. Thus, ($\text{RTE}_{\mathfrak{c}}$) entails (RTE).

theory of confirmation, (†) leads to the absurd result that "anything confirms anything". The most charitable rendition of Goodman's "triviality argument" that I have been able to come up with has the following (high-level) form:

① $\mathcal{E}$ confirms *both* H_1 *and* H_2. [*i.e.*, (†)]

② ∴ $\mathcal{E}$ confirms *both* Gb *and* Bb. [where b = "the first emerald examined after t" (assuming one exists), and G and B are incompatible properties.]

③ ∴ $\mathcal{E}$ confirms *anything*.

④ ∴ *Mutatis mutandis*, *anything* confirms *anything*.

First, I will analyze this argument from a Hempelian point of view, and then from a Bayesian point of view. From a Hempelian point of view, premise (①) must be true, since it is a logical consequence of Hempel's theory. Having established that $\mathcal{E}$ confirms *both* that all emeralds are green *and* that all emeralds are grue, Goodman says that this implies that $\mathcal{E}$ confirms *both* that "the first emerald examined after t will be green" *and* that "the first emerald examined after t will be blue". Let's just assume (*arguendo*) that there *will be* a first emerald observed after t (and that it was *not* observed *before* t). Let's call this emerald b.[33] So, Goodman's claim *seems* to be that it is a consequence of Hempel's theory that (②) $\mathcal{E}$ confirms both that Gb *and* that Bb, where G and B are incompatible properties (say, Bx is equivalent to $\sim Gx \& \phi x$, for some ϕ). This step from (①) to (②) cannot be correct, however, since (as Hempel *proves* in [16]) Hempel's theory satisfies the *consistency condition*:

(CC) If E confirms H_1 and E confirms H_2, then H_1 and H_2 are logically consistent (provided only that E is self-consistent).

So, if isn't true that Hempel's theory implies that $\mathcal{E}$ confirms *both* Gb *and* Bb (where these are *incompatible* statements about an *individual*), then what *is* true? One "nearby" truth is that Hempel's theory entails $\mathcal{E}$ confirms both of the *conditional claims* $\sim Ob \supset Gb$ and $\sim Ob \supset Bb$. Indeed, Hempel's theory even entails that $\mathcal{E}$ confirms $(\forall x)Ox$![34] And, while it might be tempting to infer from this that the *conjunction* $\mathcal{E} \& \sim Ob$ must therefore confirm both Gb *and* Bb, *even this* is not a

[33]If we stick with a *definite description* "the first emerald examined after t" rather than a *constant* "b", then this just makes Goodman's "triviality argument" *worse*. This is because "the first emerald examined after t is green" and "the first emerald examined after t is non-green" *aren't even mutually exclusive* (much less *logical opposites*). And, some sort of *logical conflict* is assumed here. See below.

[34]It is surely an *odd* consequence of Hempel's theory that $\mathcal{E}$ confirms $(\forall x)Ox$. But, it is *not* the *triviality* goodman wants. Note: it would *not* be odd to say that Oa confirms $(\forall x)Ox$, since Oa *is entailed by* $(\forall x)Ox$. The problem is that Hempel's theory implies (M), which forces him to say this same thing about *any* observation statement (*i.e.*, any statement that *entails* Oa). (M) strikes again! Furthermore, because Hempel's theory also implies (SCC) (see below) we can derive another "odd consequence" from this one — that $\mathcal{E}$ confirms *any universal claim about unexamined objects*. Moreover, this will apply to *any* observation statement of the form $\ulcorner Oa \& \phi a \urcorner$, for any predicate expression ϕ. And, so, we have the ultimate "odd consequence" of Hempel's theory of confirmation: (OC) *any observation statement confirms any universal generalization about unexamined objects*. However, (OC) does *not* imply (nor is it true) that we can get simultaneous Hempel-confirmation of *inconsistent* claims. So, we *still* do *not* get the *triviality* Goodman wanted. Note, also, that we can derive (OC) from the three properties (NC), (M), and (SCC) of Hempel's theory. We do *not* need to fiddle with "grue-ifications" of the *antecedents* of universal claims, such as Goodman's "emerose" construction — see *fn.* 36.

consequence of Hempel's theory.[35] Hence, Goodman's "triviality" argument against Hempel's theory of confirmation seems to fall short of its mark right here.[36]

Interestingly, however, this seems to be the *only* gap in Goodman's "triviality" argument against Hempel's theory. That is, all of the other steps in the argument would go through, if only this one did. Specifically, the step from (②) to (③) is OK (for a Hempelian), since Hempel's theory entails both of the following principles:

(**&**) If E confirms H_1 and E confirms H_2, then E confirms $H_1 \,\&\, H_2$.

(SCC) If E confirms H_1, and H_1 entails H_2, then E confirms H_2.

It is easy to see that (②), (**&**), and (SCC) jointly entail (③). And, once we have (③) in place, we can then get something very close to (④) in the following way. For *any* observation statement $\mathcal{E}$ of the form $\ulcorner Oa \,\&\, \phi a \urcorner$, for any predicate expression ϕ, it will follow from Hempel's theory that $\mathcal{E}$ confirms *both* $\ulcorner(\forall x)\phi x\urcorner$ *and* $\ulcorner(\forall x)(\phi x \equiv Ox)\urcorner$. Then, by (①)–(③) *mutatis mutandis*, $\mathcal{E}$ will confirm *anything*. Thus, we have the claim that any *observation* statement confirms any hypothesis, which is (basically) what Goodman meant by (④).

To sum up: there are two senses in which Goodman's triviality argument "almost" works, when aimed against Hempel's theory of confirmation. First, except for Goodman's (false) suggestion (②) that Hempel's theory allows for simultaneous confirmation of inconsistent claims, the rest of his triviality argument concerning Hempel's theory goes through. Second, while his triviality argument fails to establish the desired conclusion that every observation statement Hempel-confirms *every* statement, it can be shown that every observation statement Hempel-confirms every *universal statement about unexamined objects*. However, this can be shown in a more direct way than Goodman's remarks suggest, simply by appealing to properties (NC), (M), and (SCC) of Hempel's theory (see *fn.* 34 and *fn.* 36).

From a Bayesian point of view, however, the "triviality" argument is a non-starter. First, as we have already discussed above, (①) is not a logical consequence of Bayesian confirmation theory (even in its "Carnapian" flavors). But, *even if* (①) *were* true, premise (②) would not follow (for any Bayesian). Interestingly, however, this step is not blocked by (CC), since Bayesian confirmation theory does *not* satisfy (CC).[37] The following example shows that (CC) is false from a Bayesian point of view. Let $Rx \overset{\text{def}}{=} x$ is a red card, let $Ax \overset{\text{def}}{=} x$ is the ace of diamonds, and let $Jx \overset{\text{def}}{=} x$

[35] See Hooker's excellent paper [20] for a very careful analysis, which explains why even this does not follow in Hempel's theory. I discovered Hooker's paper after I had completed my own analysis of Goodman's "triviality argument". I highly recommend Hooker's paper, which goes into great detail about Goodman's argument (and which seems not to be as widely read as it should be). I have omitted lots of technincal details from my discussion here, since Hooker's paper covers them all beautifully.

[36] Some of Goodman's other remarks seem to suggest a *different* way of getting to (③), without going through (②). In a footnote, Goodman introduces the predicate "emerose" ($\mathbb{E}$). Let $Rx \overset{\text{def}}{=} x$ is a rose, and let our new predicate "emerose" be defined as follows: $\mathbb{E}x \overset{\text{def}}{=} (Ex \,\&\, Ox) \vee (Rx \,\&\, {\sim}Ox)$. Now, let H' be the hypothesis that all emeroses are grue $[(\forall x)(\mathbb{E}x \supset \mathfrak{G}x)]$. It *is* a consequence of Hempel's theory that $\mathcal{E}$ also confirms H'. And, by fiddling with our antecedent and consequent predicates in clever ways such as these, we can get $\mathcal{E}$ to Hempel-confirm all kinds of weird *universal generalizations*. Specifically, we can get $\mathcal{E}$ to Hempel-confirm *any universal statement about unexamined objects*. But, since this does *not* imply Hempel-confirmation of incompatible claims about *individual* unexamined objects, this just boils down to the "odd consequence" (OC) already discussed in *fn.* 34.

[37] For a Bayesian, the step from (①) to (②) is blocked because Bayesian confirmation theory does not imply (SCC). See below. Since Hempel's theory implies (SCC), he needs (CC) to block this step.

is the jack of hearts. Assuming (K) a card a is sampled at random from a standard deck (and assuming that Pr obeys the standard random card draw model):

- Ra confirms Aa, relative to K, since $\Pr(Aa \mid Ra \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
- Ra confirms Ja, relative to K, since $\Pr(Ja \mid Ra \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Ja \mid K)$.

Is this a *bad* thing? That is, is it a bad thing for a theory of confirmation to have the consequence that some evidential propositions confirm each of a pair of contradictory hypotheses? It seems clear to me that the answer is "No!". After all, we are often faced with multiple contradictory competing scientific hypotheses, and in at least some of these cases we think some of our evidence confirms (even *provides some positive evidential support for*) more than one of these competing hypotheses at the same time. So, intuitively, it seems to me that (CC) is *false*. I think it *would* be a bad thing if our confirmation theory allowed there to be cases in which E confirms both H and ${\sim}H$. But, neither the Hempelian nor the Bayesian theory of confirmation violates the following *weaker* version of the consistency condition:

(CC′) If E confirms both H_1 and H_2, then H_1 and H_2 are *not logical opposites.*

Moving on through the argument now, *even if* premise (②) were true in Goodmanian contexts (from a Bayesian point of view), (③) would not follow. But how can Bayesian confirmation theory manage to avoid the absurd consequence that some evidential statements will confirm *every other statement*? After all, if it's possible for E to confirm both members of an inconsistent pair of statements, then won't it follow that E will confirm *anything*, since anything *follows logically* from a logically inconsistent set? No. This reasoning presupposes the two principles (**&**) and (SCC) above. While (**&**) and (SCC) are true from a *Hempelian* point of view, they are *false*, from a *Bayesian* point of view. Here's a simple Bayesian counterexample to (SCC). Let $Rx \overset{\text{def}}{=} x$ is a red card, let $Ax \overset{\text{def}}{=} x$ is the ace of diamonds, and let $Sx \overset{\text{def}}{=} x$ is *some* ace. Assuming (K) that we sample a card a at random from a standard deck:

- Ra confirms Aa, relative to K, since $\Pr(Aa \mid Ra \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
- Ra *doesn't* confirm Sa, rel. to K, since $\Pr(Sa \mid Ra \& K) = \frac{2}{26} = \frac{4}{52} = \Pr(Sa \mid K)$.

The ingredients of our counterexample to (CC) above can be used to construct a counterexample to (**&**) in the following way. Let $Rx \overset{\text{def}}{=} x$ is a red card, let $Ax \overset{\text{def}}{=} x$ is the ace of diamonds, and let $Jx \overset{\text{def}}{=} x$ is the jack of hearts. Then, we have:

- Ra confirms Aa, relative to K, since $\Pr(Aa \mid Ra \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
- Ra confirms Ja, relative to K, since $\Pr(Ja \mid Ra \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Ja \mid K)$.
- Ra does not confirm $Aa \,\&\, Ja$, relative to K, since:

$$\Pr(Aa \,\&\, Ja \mid Ra \,\&\, K) = 0 = \Pr(Aa \,\&\, Ja \mid K)$$

Thus, while Goodman's triviality argument does reveal several "odd conseqeunces" of Hempel's theory of confirmation, it is powerless, when aimed against Bayesian confirmation theory. Indeed, Bayesian confirmation theory does not have any of the odd consequences we saw (*fns.* 34 & 36) in connection with Hempel's theory.[38]

[38] For instance, it is *not* a consequence of Bayesian confirmation theory that $\mathcal{E}$ (or any other observation statement) confirms arbitrary universal generalizations about unexamined objects [it does not even follow from Bayesian confirmation theory that $\mathcal{E}$ confirms $(\forall x)Ox$]. Moreover, using Goodman's "emerose" construction (*fn.* 36) does not reveal any odd consequences of Bayesian confirmation theory either, since Bayesian theory (unlike Hempel's) *does not entail* (NC), (M), *or* (SCC).

6. Conclusion

Goodman's "New Riddle" does reveal some "odd" consequences of Hempel's confirmation theory (especially, in the "triviality" part of the argument — see *fn.* 34). But, one does not need fancy "grue" predicates to cast serious doubt on the adequacy of Hempel's theory.[39] As we have seen, almost all of the "bad" things Goodman (correctly) demonstrates concerning Hempel's theory of confirmation can be established more directly, using only properties like (NC), (M), and (SCC). Indeed, I think (M) in particular is the ultimate cause of many of the problems Goodman and others have had with Hempel's theory. Even Hempel himself would have recognized (M) as a shortcoming of his theory — if only it had been brought to his attention (see my [6] for more on this lacuna). Moreover, even though Goodman's arguments do reveal some "oddities" in Hempelian confirmation theory, they do *not* reduce Hempel's theory to *triviality* as Goodman suggested (see also [20]).

On the other hand, if we adopt a proper Bayesian theory of confirmation — which is superior to Hempel's theory in many ways that are independent of Goodmanian considerations — then Goodman's "New Riddle" loses much of its force. Our analogy with the relevantists' "*reductio*" of classical deductive logic suggests that what Goodman's qualitative and quantitative arguments *really* reveal is that Bayesians should not accept very naïve versions of Carnap's requirement of total evidence like (RTE). This won't come as a surprise to contemporary Bayesians, most of whom have already seen independent reasons to reject (RTE), such as the problem of old evidence. Moreover, alternatives to (RTE) that can avoid both the old evidence problem and the "grue" problem have been suggested both in Carnap's work [2], and (more explicitly) in the recent formal epistemology literature (*e.g.*, by Tim Williamson [29, ch. 9]). Finally, while Goodman's "triviality" argument reveals some rather odd properties of Hempel's theory of confirmation (see *fn.* 34 and *fn.* 36), it is simply a *non*-starter from a Bayesian (inductive-logical) point of view.

In this paper, I have left open (specifically) what I think Bayesian *inductive logic* (*viz.*, *confirmation theory*) should look like, in light of Goodman's "grue" problem. I have also left open (specifically) what I think Bayesians should say about the (epistemic) *evidential support* relations in "grue" contexts. I suspect that these omissions (especially, the *epistemic* ones) will be disappointing to some readers. My main aim in this paper has been merely to try to re-orient the dialectic concerning the bearing of Goodman's "New Riddle" on the very possibility of (classical) *inductive logic*. Here, I have made use of an analogy with the "paradoxes of entailment", and a "Harman-style" response to them. I have shown how an analogous Harman-style response to Goodman's "grue" paradox (on behalf of *Bayesian* inductive logic/confirmation theory) might go. Not only does this analogy reveal a novel Bayesian response to Goodman's arguments (as arguments against Bayesian *inductive logic*), it also suggests avenues for further elaboration and clarification of both the inductive-logical and the epistemic sides of Bayesianism. I plan to address both the inductive-logical and the epistemic issues raised by Goodman's "New Riddle" more fully in a future monograph on pure and applied confirmation theory.

[39] "Grue" predicates are also not needed to establish cases of evidential underdetermination or confirmational indistinguishability of the kind supposedly embodied in Goodman's (†) and (⋆). Of course, I'm not the first to make this point. A.J. Ayer [1, *p.* 84], and others, made this point long ago.

References

[1] A.J. Ayer, *Probability & Evidence*, Macmillan, London, 1972.
[2] R. Carnap, *Logical Foundations of Probability*, Chicago University Press, Chicago, 1950 (second edition 1962).
[3] ______, *The Continuum of Inductive Methods*, Chicago University Press, Chicago, 1952.
[4] J. Earman, *Bayes or bust? A critical examination of Bayesian confirmation theory*, *MIT Press*, Cambridge, MA, 1992.
[5] E. Eells, *Bayesian problems of old evidence*, in C. Wade Savage (*ed.*) *Scientific theories*, *Minnesota Studies in the Philosophy of Science, Vol. X*, University of Minnesota Press, Minneapolis, 205-223.
[6] B. Fitelson, *The paradox of confirmation*, *Philosophy Compass* (B. Weatherson and C. Callender, *eds.*), Blacwkell (online publication), Oxford, 2006, `http://fitelson.org/ravens.htm`.
[7] ______, *Likelihoodism, Bayesianism, and relational confirmation*, *Synthese*, 2007. `http://fitelson.org/synthese.pdf`.
[8] ______, *Logical foundations of evidential support*, *Philosophy of Science*, 2007. `http://fitelson.org/psa2004.pdf`.
[9] R. Fumerton, *Metaepistemology and Skepticism*, Rowman & Littlefield, 1995.
[10] C. Glymour, *Theory and Evidence*, Princeton University Press, Princeton, 1980.
[11] I.J. Good, *The white shoe is a red herring*, *British J. for the Phil. of Sci.* **17** (1967), 322.
[12] ______, *The white shoe* qua *red herring is pink*, *British J. for the Phil. of Sci.* **19** (1968), 156–157.
[13] N. Goodman, *A Query on Confirmation*, *Journal of Philosophy*, **XLIII** (1946), 383–385.
[14] ______, *Fact, Fiction, and Forecast*, Harvard University Press, Cambridge, Mass., 1955.
[15] G. Harman, *Change in View*, MIT Press, Cambridge: MA, 1986.
[16] C. Hempel, *A purely syntactical definition of confirmation*, *J. of Symbolic Logic* **8** (1943), 122–143.
[17] ______, *Studies in the logic of confirmation*, *Mind* **54** (1945), 1–26, 97–121.
[18] ______, *The white shoe: no red herring*, *British J. for the Phil. of Sci.* **18** (1967), 239–240.
[19] C. Hempel and P. Oppenheim, *A definition of "degree of confirmation"*, *Philosophy of Science*, **12** (1945), 98–115.
[20] C. Hooker, *Goodman, 'grue' and Hempel*, *Philosophy of Science*, **35** (1968), 232–247.
[21] J. Keynes, *A Treatise on Probability*, Macmillan, London, 1921.
[22] J. MacFarlane, *In what sense (if any) is logic normative for thought?*, manuscript, 2004.
[23] P. Maher, *Probability captures the logic of scientific confirmation*, in *Contemporary Debates in the Philosophy of Science* (C. Hitchcock, *ed.*), Blackwell, Oxford, 2004.
[24] ______, *Probabilities for multiple properties: the models of Hesse and Carnap and Kemeny*, *Erkenntnis*, **55** (2001), 183–216.
[25] J. Nicod, *The logical problem of induction*, (1923) in *Geometry and Induction*, University of California Press, Berkeley, California, 1970.
[26] W.V.O. Quine, *Natural kinds*, *Ontological Relativity and Other Essays*, Columbia U. Press, 1969.
[27] E. Sober, *No model, no inference: A Bayesian primer on the grue problem*, in *Grue! The New Riddle of Induction* (D. Stalker *ed.*), Open Court, Chicago, 1994.
[28] R. Sylvan and R. Nola, *Confirmation without paradoxes*, in *Advances in Scientific Philosophy* (G. Schurz and G. Dorn, *eds.*), Rodopi, Amsterdam/Atlanta, 1991.
[29] T. Williamson, *Knowledge and its Limits*, Oxford University Press, Oxford, 2000.

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