Notes for Week 11 of *Confirmation*

11/21/07

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1 Two Reflections on Our Discussions from the Past Few Weeks

1.1 Reflections on Maher on (NC)

As you'll recall, Maher's "counterexample" to (NC) was based on intuitions about what raises the probability of what, relative to "what we (actually) know" (K_α). As I pointed out, this is unfortunate, since he needs an example that holds, relative to *no* background knowledge (or *empty/a priori* background knowledge $K_\top$). Similarly, Maher's argument for the existence of inductive probabilities only establishes the existence of (some) inductive probabilities, relative to *substantive*, *empirical* background conditions [$\Pr(p \mid K_\alpha)$]. Again, this is unfortunate, since his main applications of inductive probability to confirmation theory involve principles like (NC) which are explicitly to be interpreted as involving inductive probabilities, relative to no/empty/*a priori* background evidence [$\Pr(p \mid K_\top)$]. Apparently, he seems to think that some sort of *analogical* argument will allow us to go from facts about $\Pr(p \mid K_\alpha)$ to facts about $\Pr(p \mid K_\top)$. But, the analogical argument will also have to (somehow) establish the *existence* of $\Pr(p \mid K_\top)$ from the *existence* of $\Pr(p \mid K_\alpha)$. Moreover, the analogical argument will also have to (somehow) tell us something about the values (or ranges of values) of $\Pr(p \mid K_\top)$, for some p's. For instance, Maher needs certain p's to have *low* $\Pr(p \mid K_\top)$ values, in order for his "counterexample" to (NC) to have any force. [This adds up to some very heavy lifting for an analogical argument!] This is all review from what I said in my notes a few weeks back. Now, I want to point out another problematic fact about Maher's discussions on inductive probability and confirmation. Let's think about Maher's *explicatum* for $\Pr(p \mid K_\top)$. Maher's λ/γ-continuum has *adjustable parameters* γ_F and γ_G, which correspond to $\Pr(Fa \mid K_\top)$ and $\Pr(Ga \mid K_\top)$, repsectively, for an $\mathscr{L}^{2,2}$ with two predicates F and G and a constant a, which (let's assume) appears in some evidence statement E of interest. Maher tells us that:

> The choice of γ_F and γ_G will depend on what the predicates 'F' and 'G' mean and may require careful deliberation. For example, if 'F' means 'raven' then, since this is a very specific property and there are vast numbers of alternative properties that seem equally likely to be exemplified a priori, γ_F should be very small, surely less than 1/1000. A reasoned choice of a precise value would require careful consideration of what exactly is meant by 'raven' and what the alternatives are.

Later, in his "counterexample" to (NC), Maher sets the values of γ_F and γ_G very low, and shows that this suffices to generate a counterexample to (NC) for his explicatum for "confirmation relative to no background evidence." Maher claims that the values of γ_F and γ_G will depend on "exactly is meant by 'raven' and what the alternatives are". This sounds (to my ear) like the values depend on *contextual* factors, which are *pragmatic* (this is what Carnap said about his later systems). But, recall that Maher also claims that

(1) The probability that the ball is white, given that it is white or black is $\frac{1}{2}$. [$\Pr(Wa \mid Wa \vee Ba) = \frac{1}{2}$.]

is just plain *true*. For Maher, the truth-value of (1) *doesn't* depend on any contextual or pragmatic factors (*e.g.*, on what other alternative colors might be "in play", *etc.*). But, he's now implying that the truth-value of

(2) The probability that the ball is white, given that it is white or non-white is $\frac{1}{2}$. [$\Pr(Wa \mid \top) = \frac{1}{2}$.]

does depend on "what the alternatives are". Presumably, this means that the value of $\Pr(Wa \mid \top)$ depends on how many alternative (non-white) colors for a are "in play" in the context at hand. Let's think about that. By Maher's own axioms for $\Pr(\cdot \mid \cdot)$, we can show (provided only that $\Pr(Wa \vee Ba \mid \top) \neq 0$ and $\Pr(Wa \& Ba \mid \top) = 0$, which both seem to be presupposed by Maher in this context) that the following must be true:

(3) $\Pr(Wa \mid Wa \vee Ba) = \dfrac{\Pr(Wa \,\&\, (Wa \vee Ba) \mid \top)}{\Pr(Wa \vee Ba \mid \top)} = \dfrac{\Pr(Wa \mid \top)}{\Pr(Wa \mid \top) + \Pr(Ba \mid \top)}$.

This is somewhat odd. Moreover, (1) and (3) can *both* obtain *only if* we assume a naïve *equiprobability* model for the *a priori* probabilities of the Ca's, for all alternative colors C (*e.g.*, if W and B are *the only alternatives*, then $\Pr(Wa \mid \top) = \Pr(Ba \mid \top) = \frac{1}{2}$ is true, *etc.*). But, what's the motivation for *that*? Looks like we're back to the "Principle of Indifference" for the determination of the γ-values. But, why think indifference makes any more sense here (for the Ca's) than it did for the state descriptions of $\mathscr{L}^{2,2}$, which led to $\mathfrak{m}^\dagger$? I think Carnap's concession that the γ's are only fixable *pragmatically* (*a posteriori*) is telling. But, this seems to be *abandoning* "a priori" inductive probabilities altogether, which Maher doesn't seem to want to do.

1.2 Reflections on A Remark from Goodman (1946)

As Kenny mentioned, Goodman says the following in his (1946) paper:

> The theories of confirmation in question require the primitive predicates to be logically independent. This is perhaps a dubious stipulation since it places a logical requirement upon the informal, extrasystematic explanation of the predicates. Such doubts aside, the requirement would make it impossible for the predicates "R" and "S" to belong to the same system. Hence the conflicting confirmations would not occur in any one system. But this is of little help, since the system containing the predicate "S" alone is quite as admissible as the one containing "R" alone; and in the former system, as we have seen, "$S_{a_{100}}$" will be formally confirmed by the very evidence which intuitively disconfirms it.

As I explained last time, Goodman is (unfairly) "coarse-graining" the predicate S here, which should really be written as a biconditional involving two more primitive predicates R and O, as follows: $Sx \stackrel{\text{def}}{=} Ox \equiv Rx$. Once this is done, we actually *can* have R and O "belong to the same system", and we *can* then talk perfectly well and clearly about, for instance, $Oa \,\&\, Ra$ confirming both Rb and $Ob \equiv Rb$ (*viz.*, Sb). Then, we have:

	Does E_i confirm H?	
Hempel	$H_1 \stackrel{\text{def}}{=} Rb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Rb$
$E_1 \stackrel{\text{def}}{=} Ra$	Always	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Ra$	Always	Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Ra$	Never	Always

	Does E_i confirm H?	
Carnap ($\mathfrak{c}^\dagger$)	$H_1 \stackrel{\text{def}}{=} Rb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Rb$
$E_1 \stackrel{\text{def}}{=} Ra$	Never	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Ra$	Never	Never
$E_3 \stackrel{\text{def}}{=} Oa \equiv Ra$	Never	Never

	Does E_i confirm H?	
Carnap ($\mathfrak{c}^*$)	$H_1 \stackrel{\text{def}}{=} Rb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Rb$
$E_1 \stackrel{\text{def}}{=} Ra$	Always	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Ra$	Always	Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Ra$	Never	Always

	Does E_i confirm H?	
Maher ($\mathfrak{c}^\gamma$)	$H_1 \stackrel{\text{def}}{=} Rb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Rb$
$E_1 \stackrel{\text{def}}{=} Ra$	Always	Not Always
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Ra$	Always	Not Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Ra$	Not Always	Always

Let's think about two languages: the grue-language ($\mathscr{L}_1$) and the red-language ($\mathscr{L}_2$). In $\mathscr{L}_1$, "grue" (S) and "examined prior to t" (O) are the two primitive predicates. In $\mathscr{L}_2$, "red" (R) and "examined prior to t" (O) are the two primitive predicates. These languages are expressively equivalent, since $Sx \approx Ox \equiv Rx$, and $Rx \approx Ox \equiv Sx$. If we operate in $\mathscr{L}_2$, then we get the verdicts I report above. If we operate in $\mathscr{L}_1$, we get:

	Does E_i confirm H?	
Hempel	$H_1 \stackrel{\text{def}}{=} Sb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Sb$
$E_1 \stackrel{\text{def}}{=} Sa$	Always	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Sa$	Always	Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Sa$	Never	Always

	Does E_i confirm H?	
Carnap ($\mathfrak{c}^\dagger$)	$H_1 \stackrel{\text{def}}{=} Sb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Sb$
$E_1 \stackrel{\text{def}}{=} Sa$	Never	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Sa$	Never	Never
$E_3 \stackrel{\text{def}}{=} Oa \equiv Sa$	Never	Never

	Does E_i confirm H?	
Carnap ($\mathfrak{c}^*$)	$H_1 \stackrel{\text{def}}{=} Sb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Sb$
$E_1 \stackrel{\text{def}}{=} Sa$	Always	Never
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Sa$	Always	Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Sa$	Never	Always

	Does E_i confirm H?	
Maher ($\mathfrak{c}^\gamma$)	$H_1 \stackrel{\text{def}}{=} Sb$	$H_2 \stackrel{\text{def}}{=} Ob \equiv Sb$
$E_1 \stackrel{\text{def}}{=} Sa$	Always	Not Always
$E_2 \stackrel{\text{def}}{=} Oa \,\&\, Sa$	Always	Not Always
$E_3 \stackrel{\text{def}}{=} Oa \equiv Sa$	Not Always	Always

I don't see how Goodman's remark *applies* to such a proper fine-grained representation. Moreover, there is *no language-variance* here in either Hempel's theory or in Carnap's early ($\mathfrak{c}^\dagger/\mathfrak{c}^*$) theories. So, *Goodman* was *not* giving an example involving language-variance. In the later ($\mathfrak{c}^\gamma$) theories, however, there *is* language variance here. To see this, note that E_2 always $\mathfrak{c}^\gamma$-confirms H_1-in-$\mathscr{L}_1$, but E_2 does not always $\mathfrak{c}^\gamma$-confirm H_2-in-$\mathscr{L}_2$, and H_1-in-$\mathscr{L}_1 \approx H_2$-in-$\mathscr{L}_2$. Thus, we *can* get *reversals* here in the later Carnapian theories.[1]

[1] Thanks to Mike for pointing this out. Mike also suggested last week that deductive logic might differ from inductive logic in that *adding* syntactical structure to a deductive logical language can't *reverse* any previous *valid*-verdicts. Is that true? Here's a neat example (MacFarlane & Kolodny, unpublished). *Modus tollens* is a valid form, in *truth-functional, sentential* languages $\mathscr{L}$. But, if we add *epistemic modals* to $\mathscr{L}$, we seem to get some *"invalid" instances* of *modus tollens*. Imagine this argument, in the mouth of someone who doesn't know whether it's raining outside: "If it's raining outside, then the ground must be wet. The ground might not be wet. ∴ it's not raining outside." This *looks like* a case of "intuitive *in*validity" in *de*ductive logic (in Goodman's *epistemic* sense, since this is surely no way for such a person to come to *reasonably believe/know* the conclusion of this argument!). Examples like these do seem analogous (at some level of abstraction) to Goodman's "grue". Stove (now posted on website) argues that *neither* deductive *nor* inductive logic is "purely formal" (because of similar examples). This seems to presuppose something like MacFarlane's 1-Formality understanding of "purely formal", which has logic providing *constitutive norms* for (*e.g.*) inference. I think *that*'s controversial (as is the claim that the above example is a counterexample to the *validity* of *modus tollens*). I think it is the *epistemic* structure of such examples that are *un*controversial. This is why I prefer to formulate these things in *epistemic* terms.

Epistemological Critiques of "Classical" Logic: Two Case Studies

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Overview of Talk

- This talk is largely historical. It aims to sketch and trace some of consequences of the following (crude) analogy:

  entailment : inference : : confirmation : evidential support
- I will focus on arguments *against* classical deductive and inductive logic ("relevantist" and "grue" arguments).
- The talk is mainly *defensive* in nature, although I'll try to say some positive things (hopefully, also in Q&A — see "Extras").
- I'll begin with Harman's defense of classical deductive logic against certain (epistemological) "relevantist" arguments.
- Then, I will argue that if you like Harman's defensive maneuver in the deductive case, you should like a similar defense of inductive logic (from "grue") *even better*.
- I will indicate how a "Harmanian maneuver" might be used to defend either Hempelian or Carnapian inductive logic.
- I will focus mainly on defending Carnapian IL from "grue".

- Here is a "*reductio*" of classical deductive logic (this is naïve and oversimplified, but I'll re-examine it on the next slide):
  (1) For all sets of statements X and all statements p, if X is inconsistent, then p is a logical consequence of X.
  (2) If an agent S's belief set B entails p (and S *knows* $B \vDash p$), then it would be reasonable for S to infer/believe p.
  (3) *Even if* S knows their belief set B is inconsistent (and, hence, that $B \vDash p$, for *any* p), there are still *some* p's such that it would *not* be reasonable for S to infer/believe p.
  (4) ∴ Since (1)–(3) lead to absurdity, our initial assumption (1) must have been false — *reductio* of the "explosion" rule (1).
- Harman [8] would concede that (1)–(3) are inconsistent, and (as a result) that *something* is wrong with premises (1)–(3).
- But, he would reject the relevantists' diagnosis that (1) must be rejected. I take it he'd say it's (2) that is to blame here.
- ☞ (2) is a *bridge principle* [12] linking *entailment* and *inference*.
- (2) is correct *only* for *consistent* B's. [*Even if* B is consistent, the correct response *may* rather be to *reject* some B_i's in B.]

- Note: the choice of *deductive* contexts in which S's belief set B is (known by S to be) *inconsistent* is intentional here.
- In such contexts, there is a *deep disconnect* between (known) *entailment* relations and (kosher) *inferential* relations.
- One might try a more sophisticated deductive bridge principle (2′) here. But, I conjecture a *dilemma*. *Either*:
  - (2′) will be *too weak* to yield a (classically) *valid* "reductio".

    or
  - (2′) will be *false*. [Our original BP (2) falls under this horn.]
- Let B be S's belief set, and let q be the conjunction of the elements B_i of B. Here are two more candidate BP's:
  ($2'_1$) If S knows that $B \vDash p$, then S should *not* be such that *both*: S believes q, *and* S does not believe p.
  ($2'_2$) If S knows that $B \vDash p$, then S should *not* be such that *both*: S believes each of the $B_i \in B$, *and* S does not believe p.
- ($2'_2$) is *false* (preface paradox) *and* too weak (it's wide scope).
- ($2'_1$) *may* be true, but it is also *too weak*. [It's wide scope, and the agent can reasonably disbelieve *both* q *and* p].

- So, I think Harman is *right* about such "relevantist" arguments.
- Next, I will argue that Goodman's "grue" argument against CIL fails for analogous reasons (indeed, I'll argue it's *even worse*!).
- I'll begin by discussing the IL's of Hempel and Carnap.
- Hempelian IL (confirmation theory) uses *entailment* to explicate "inductive logical support" (confirmation) — a logical relation between statements. [*i.e.*, E confirms H iff $E \vDash \text{dev}_E(H)$]
- Hempel's theory has the following three key consequences:

(EQC) If E confirms H and $E \dashv\vDash E'$, then E' confirms H.

(NC) For all constants x and all (consistent) predicates ϕ and ψ: $\ulcorner \phi x \,\&\, \psi x \urcorner$ confirms $\ulcorner (\forall y)(\phi y \supset \psi y) \urcorner$.

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

- These three properties are the crucial ones needed to reconstruct Goodman's "grue" argument against Hempel.
- Before giving a precise reconstruction of Goodman's "grue" argument, we'll look at the essentials of Carnapian IL/CT.

- Carnapian confirmation (*i.e.*, *later* Carnapian theory [13] — see "Extras") is based on *probabilistic relevance*, not entailment:
  - E confirms H, relative to K iff $\Pr(H \mid E \,\&\, K) > \Pr(H \mid K)$, for some "suitable" conditional probability function $\Pr(\cdot \mid \cdot)$.
    - Note how this is an *explicitly 3*-place relation. Hempel's was only *2*-place. This is because Pr (unlike $\vDash$) is *non-monotonic*.
    - Carnap thought that "suitable Pr" meant "logical Pr" in a rather strong sense (see "Extras"). However, Goodman's argument will work against *any* probability function Pr.

☞ Carnap's theory implies *only 1* of our 3 Hempelian claims: (EQC). It does *not* imply (NC) *or* (M) (see "Extras" & [3]/[13]).

  - This will allow Carnapian IL to avoid facing the full brunt of Goodman's "grue" (but, it will still face a serious challenge).
- For Carnap, confirmation is a *logical* relation (akin to entailment). Like entailment, confirmation can be *applied*, but this requires *epistemic bridge principles* [akin to (2)].
- Carnap [1] discusses various bridge principles. The most well-known of these is the *requirement of total evidence*.

- **The Requirement of Total Evidence**. In the application of IL to a given knowledge situation, the total evidence available must be taken as a basis for determining the degree of confirmation.
- This *sounds* like a plausible principle. But, once it is made more precise, it will actually turn out to be subtly defective.
- More precisely, we have the following *bridge principle* connecting *confirmation* and *evidential support*:

(RTE) E evidentially supports H *for* S *in* C iff E confirms H, relative to K, where K is S's *total evidence in* C.

- The (RTE) has often been (implicitly) presupposed by Bayesian epistemologists (both subjective and objective).
- However, as we will soon see, the (RTE) is not a tenable bridge principle, and for reasons independent of "grue".

☞ Moreover, Goodman's "grue" argument will rely *more heavily* on (RTE) than the relevantists' argument relies on (2). In this sense, Goodman's argument will be *even worse*.

- Before reconstructing the argument, a brief "grue" primer.

- Let $Gx \overset{\text{def}}{=} x$ is green, $Ox \overset{\text{def}}{=} x$ is examined prior to t, and $Ex \overset{\text{def}}{=} x$ is an emerald. Goodman introduces a predicate "grue"

$$\mathcal{G}x \overset{\text{def}}{=} x \text{ is grue} \overset{\text{def}}{=} Ox \equiv Gx.$$

- Consider the following two universal generalizations

(H_1) All emeralds are green. [$(\forall x)(Ex \supset Gx)$]

(H_2) All emeralds are grue. [$(\forall x)[Ex \supset (Ox \equiv Gx)]$]

- And, consider the following instantial evidential statement

($\mathcal{E}$) $Ea \,\&\, Oa \,\&\, Ga$

- Hempel's confirmation theory [(EQC) & (NC) & (M)] entails:

(†) $\mathcal{E}$ confirms H_1, *and* $\mathcal{E}$ confirms H_2. [proof]

- As a result, his theory entails the following weaker claim

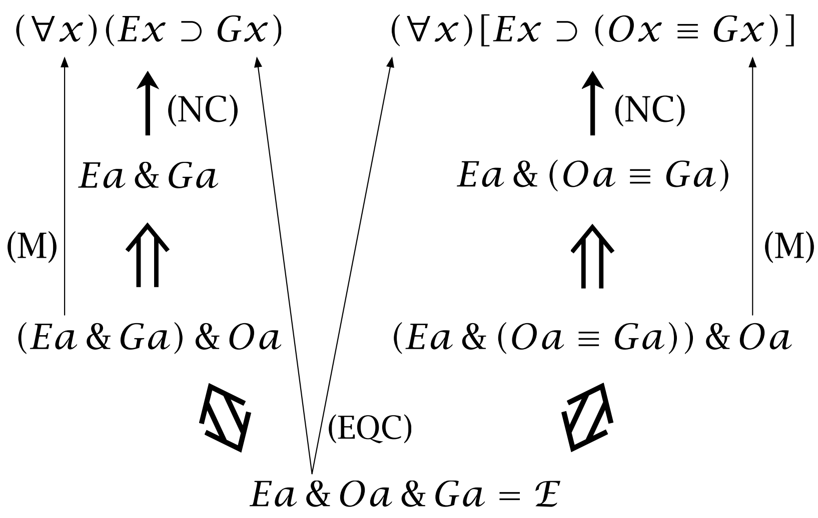
(‡) $\mathcal{E}$ confirms H_1 *if and only if* $\mathcal{E}$ confirms H_2.

- What about (later) Carnapian theory? Does *it* entail even (‡)?

☞ Interestingly, NO! There are (later) Carnapian Pr-models in which $\mathcal{E}$ confirms H_1 but $\mathcal{E}$ *dis*confirms H_2 (see "Extras").

- In this sense, Hempel was an easier target for Goodman than Carnap (Goodman claims to be attacking both).
- Now, we're ready to reconstruct Goodman's argument.

A Proof of (†) From Hempel's (NC), (M), and (EQC)



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- There is just one more ingredient in Goodman's argument:
  - The agent S who is assessing the evidential support that $\mathcal{E}$ provides for H_1 *vs* H_2 in a Goodmanian "grue" context $C_{\mathcal{G}}$ has Oa as part of their total evidence in $C_{\mathcal{G}}$. (*e.g.*, [14].)
- Now, we can run the following Goodmanian *reductio*:
  - (i) E confirms H, relative to K iff $\Pr(H \mid E \,\&\, K) > \Pr(H \mid K)$.
  - (ii) E evidentially supports H *for* S *in* C iff E confirms H, relative to K, where K is S's *total evidence in* C.
  - (iii) The agent S who is assessing the evidential support $\mathcal{E}$ provides for H_1 *vs* H_2 in a Goodmanian "grue" context $C_{\mathcal{G}}$ has Oa as part of their total evidence in $C_{\mathcal{G}}$ [*i.e.*, $K \vDash Oa$].
  - (iv) *If* $K \vDash Oa$, *then—c.p.—*$\mathcal{E}$ confirms H_1 relative to K iff $\mathcal{E}$ confirms H_2 relative to K, for ***any*** Pr [*i.e.*, (‡) holds, $\forall$ Pr's].
  - (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 $C_{\mathcal{G}}$.
- ∴ (i)–(vi) lead to an absurdity. Hence, our initial assumption (i) must have been false. Carnapian inductive logic refuted?

- Premise (vi) is based on Goodman's *epistemic intuition* that, in "grue" contexts, $\mathcal{E}$ evidentially supports H_1 but *not* H_2.
- Premise (v) follows logically from premises (i)–(iv).
- Premise (iv) is a theorem of probability calculus (***any*** Pr!).
  - The *c.p.* clause needed is $\Pr(Ea \mid H_1 \,\&\, K) = \Pr(Ea \mid H_2 \,\&\, K)$, which is assumed in all probabilistic renditions of "grue".
- Premise (iii) is an assumption about the agent's background knowledge K that's implicit in Goodman's set-up. See [14].
- Premise (ii) is (RTE). It's the *bridge principle*, akin to (2) in the relevantists' *reductio*. This is the premise I will focus on.
- Here are my two main points about Goodman's argument:
  - (ii) must be rejected by Bayesians for independent reasons.
  - Carnapian confirmation theory *doesn't even entail* (‡). [Hempel's theory does, just as deductive logic entails (1).]
- This suggests Goodman's argument is *even less* a *reductio* of (i) than the relevantists' argument is a *reductio* of (1).
- Next, I will explain why Carnapians/Bayesians should reject (ii) on *independent* grounds: The Problem of Old Evidence.

- As Tim Willimson points out [16, ch. 9], Carnap's (RTE) must be rejected, because of the problem of old evidence [2].
- If S's total evidence in C (K) entails E, then, according to (RTE), E cannot evidentially support *any* H for S in C.
- As a result, one cannot (in all contexts) use $\Pr(\cdot \mid K)$ — for *any* Pr — when assessing the *evidential import of* E.
- There are (basically) two kinds of strategies for revising (RTE). Carnap [1, p. 472] & Williamson [16, ch. 9] propose:

(RTE$_\top$) E evidentially supports H for S in C iff S possesses E as evidence in C and $\Pr_\top(H \mid E \,\&\, K_\top) > \Pr_\top(H \mid K_\top)$. [$K_\top$ is *a priori*, $\Pr_\top$ is "inductive" [13]/"evidential" [16]/"logical" [1].]

- Note: Hempel explicitly *required* that confirmation be taken "*relative to* $K_\top$" in all treatments of the paradoxes [9, 10]. (RTE$_\top$) is a charitable Carnapian reconstruction of Hempel.
- A more "standard" way to revise (RTE) is [(RTE′)] to use $\Pr_{S'}(\cdot \mid K')$, where $K \vDash K' \nvDash E$, and $\Pr_{S'}$ is the credence function of a "counterpart" S' of S with total evidence K'.

- Carnap never re-wrote the part of LFP [1] that discusses the (RTE), in light of a probabilistic *relevance* ("increase in firmness" [1]) notion of confirmation. This is too bad.
- If Carnap had discussed this ("old evidence") issue, I suspect he would have used something like Williamson's ($\text{RTE}_\top$) as his bridge principle connecting confirmation and evidence.
- Various other philosophers have proposed similar accounts of "support" as some probabilistic relation, taken relative to an "informationless" or "*a priori*" background/probability.
  - Richard Fumerton (who, unlike Williamson, is an epistemological *internalist*) proposes such a view in his [4].
  - Patrick Maher [13] applies such relations extensively in his recent (neo-Carnapian) work on confirmation theory.
  - Brian Weatherson [15] uses a similar, "Keynesian" [11] inductive-probability approach to evidential support.
- So, many Bayesians *already* reject (RTE). [Of course, "grue" gives Bayesians another important reason to reject (RTE).]

- So far, I have left open (precisely) what I think Bayesian confirmation theorists *should* say (*logically* & *epistemically*) in light of Goodman's "grue" paradox (but, see "Extras").
- Clearly, BCTs will need to revise (RTE) in light of "grue". But, the standard (RTE′) way of doing this to cope with "old evidence" isn't powerful enough to avoid *both* problems.
- Williamson's ($\text{RTE}_\top$) revision of (RTE) — also suggested by Carnap — avoids both problems, from a *logical* point of view (*if* "inductive"/"logical"/"evidential" probabilities *exist*!). But, what should BCTs say on the *epistemic* side?
- I don't have a fully satisfactory answer to this question (yet). But, I remain unconvinced that the epistemic problem (if there is one) is caused by the "non-naturalness" of "grue".
- The problem, I suspect, may involve an *observation selection effect*: we know something about the "grue" observation process that *undermines* (or *defeats*) evidence it produces.
- I hope we can discuss this (and IL) in the Q&A (see "Extras").

[1] R. Carnap, *Logical Foundations of Probability*, 2nd *ed.*, Chicago Univ. Press, 1962.

[2] E. Eells, *Bayesian problems of old evidence*, in C. Wade Savage (*ed.*) *Scientific theories*, *Minnesota Studies in the Philosophy of Science* (Vol. X), 205-223, 1990.

[3] B. Fitelson, *The Paradox of Confirmation*, *Philosophy Compass* (online publication), Blackwell, 2006. URL: `http://fitelson.org/ravens.htm`.

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"Carnapian" Counterexamples to (NC) and (M)

(K) Either: (H) there are 100 black ravens, no nonblack ravens, and 1 million other things, or ($\sim H$) there are 1,000 black ravens, 1 white raven, and 1 million other things.

- Let $E \overset{\text{def}}{=} Ra \,\&\, Ba$ (a randomly sampled from universe). Then:

$$\Pr(E \mid H \,\&\, K) = \frac{100}{1000100} \ll \frac{1000}{1001001} = \Pr(E \mid {\sim}H \,\&\, K)$$

- $\therefore$ This K/Pr constitute a counterexample to (NC), assuming a "Carnapian" theory of confirmation. This model can be emulated in the later Carnapian λ/γ-systems [13].

- Let $Bx \overset{\text{def}}{=} x$ is a black card, $Ax \overset{\text{def}}{=} x$ is the ace of spades, $Jx \overset{\text{def}}{=} x$ is the jack of clubs, and $K \overset{\text{def}}{=}$ a card a is sampled at random from a standard deck (where Pr is also standard):
  - $\Pr(Aa \mid Ba \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
  - $\Pr(Aa \mid Ba \,\&\, Ja \,\&\, K) = 0 < \frac{1}{52} = \Pr(Aa \mid K)$.

A "Carnapian" Counterexample to (‡)

(*K*) Either: (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, 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 non-green emeralds that have not been examined before t, and 1 million other things.

- Imagine an urn containing true descriptions of each object in the universe (Pr $\overset{\text{def}}{=}$ urn model). Let $\mathcal{E} \overset{\text{def}}{=}$ "$Ea \,\&\, Oa \,\&\, Ga$" be drawn. $\mathcal{E}$ confirms H_1 but $\mathcal{E}$ *dis*confirms H_2, relative to K:

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

- This K/Pr constitute a counterexample to (‡), assuming a "Carnapian" theory of confirmation. This probability model can be emulated in the later Carnapian λ/γ-systems [13].

Is "Grue" an Observation Selection Effect? Part I

- **Canonical Example of an OSE**: I use a fishing net to capture samples of fish from various (randomly selected) parts of a lake. Let E be the claim that all of the sampled fish were over one foot in length. Let H be the hypothesis that all the fish in the lake are over one foot [$(\forall x)((Fx \,\&\, Lx) \supset Ox))$].
- Intuitively, one might think E should evidentially support H. This may be so for an agent who knows *only* the above information (K) about the observation process. That is, it seems plausible that $\Pr(E \mid H \,\&\, K) > \Pr(E \mid {\sim}H \,\&\, K)$, where Pr is taken to be "evidential" (or "epistemic") probability.
- But, what if I *also* tell you that (D) the net I used to sample the fish from the lake (which generated E) has holes that are all over one foot in diameter? It seems that D *defeats* the support E provides for H (relative to K), because D *ensures* O. Thus, intuitively, $\Pr(E \mid H \,\&\, D \,\&\, K) = \Pr(E \mid {\sim}H \,\&\, D \,\&\, K)$.

Is "Grue" an Observation Selection Effect? Part II

- Note: the "grue" hypothesis (H_2) entails the following claim, which is not entailed by the green hypothesis (H_1):

  (H') All green emeralds have been (or will have been) examined prior to t. [$(\forall x)((Ex \,\&\, Gx) \supset Ox))$].
- Now, consider the following two observation processes:
  - **Process 1**. For each green emerald in the universe, a slip of paper is created, on which is written a true description of that object as to whether it has property O. All the slips are placed in an urn, and one slip is sampled at random from the urn. By *this* process, we learn ($\mathcal{E}$) that $Ea \,\&\, Ga \,\&\, Oa$.
  - **Process 2**. Suppose all the green emeralds in the universe are placed in an urn. We sample an emerald (a) at random from this urn, and we examine it. [We know *antecedently* that the examination of a will take place prior to t, *i.e.*, that Oa is true.] By *this* process, we learn ($\mathcal{E}$) that $Ea \,\&\, Ga \,\&\, Oa$.
- Goodman seems to presuppose Process 2 in his set-up.

What Could "Carnapian" Inductive Logic Be? Part I

- The early Carnap dreamt that probabilistic inductive logic (confirmation theory) could be formulated in such a way that it *supervenes* on deductive logic in a *very strong* sense.
  - **Strong Supervenience** (SS). All confirmation relations involving sentences of a first-order language $\mathcal{L}$ supervene on the deductive relations involving sentences ***of*** $\boldsymbol{\mathcal{L}}$.
- Hempel clearly saw (SS) as a *desideratum* for confirmation theory. The early Carnap also seems to have (SS) in mind.
- I think it is fair to say that Carnap's project — understood as requiring (SS) — was unsuccessful. [I think *this* is true for reasons that are *independent* of "grue" considerations.]
- The later Carnap seems to be aware of this. Most commentators interpret this shift as the later Carnap simply *giving up* on inductive logic (*qua logic*) altogether.
- I want to resist this "standard" reading of the history.

What Could "Carnapian" Inductive Logic Be? Part II

- I propose a different reading of the later Carnap, which makes him much more coherent with the early Carnap.
- I propose *weakening* the supervenience requirement in such a way that it (a) ensures this coherence, and (b) maintains the "logicality" of confirmation relations in Carnap's sense.
- Let $\mathscr{L}$ be a formal language strong enough to express the fragment of probability theory Carnap needs for his later, more sophisticated confirmation-theoretic framework.
  - **Weak Supervenience** (WS). All confirmation relations involving sentences of a first-order language $\mathcal{L}$ supervene on the deductive relations involving sentences ***of*** $\mathscr{L}$.
- As it turns out, $\mathscr{L}$ needn't be very strong (in fact, one can get away with PRA!). So, even by early (*logicist*) Carnapian lights, satisfying (WS) is all that is *really* required for "logicality".
- The specific (WS) approach I propose takes confirmation to be a *four*-place relation: between E, H, K, *and a function* Pr.

What Could "Carnapian" Inductive Logic Be? Part III

- Consequences of moving to a 4-place confirmation relation:
  - We need not try to "construct" "logical" probability functions from the syntax of $\mathcal{L}$. This is a dead-end anyhow.
  - Indeed, on this view, inductive logic has nothing to say about the *interpretation/origin* of Pr. That is *not* a *logical* question, but a question about the *application* of logic.
    - Analogy: Deductive logicians don't owe us a "logical interpretation" of the truth value assignment function v.
  - Moreover, this leads to a vast increase in the *generality* of inductive logic. Carnap was stuck with an impoverished set of "logical" probability functions (in his λ/γ-continuum).
    - On my approach, *any* probability function can be part of a confirmation relation. Which functions are "suitable" or "appropriate" or "interesting" will depend on *applications*.
    - So, some confirmation relations will not be "interesting", *etc.* But, this is (already) true of *entailments*, as Harman showed.
  - Questions: Now, what *is* the job of the inductive logician, and how (if at all) do they interact with *epistemologists*?

What Could "Carnapian" Inductive Logic Be? Part IV

- The inductive logician must explain how it is that inductive logic can satisfy the following Carnapian *desiderata*.
  - The confirmation function $\mathfrak{c}(H, E \mid K)$ quantifies a *logical* (in a Carnapian sense) relation among statements E, H, and K.

  ($\mathcal{D}_1$) One aspect of "logicality" is ensured by moving from (SS) to (WS) [from an $\mathcal{L}$-determinate to an $\mathscr{L}$-determinate concept].

  ($\mathcal{D}_2$) Another aspect of "logicality" insisted upon by Carnap is that $\mathfrak{c}(H, E \mid K)$ should *generalize* the entailment relation.
    - This means (at least) that we need $\mathfrak{c}(H, E \mid K)$ to take a maximum (minimum) value when $E \,\&\, K \vDash H$ ($E \,\&\, K \vDash \sim H$).
    - Very few *relevance* measures $\mathfrak{c}$ satisfy this "generalizing $\vDash$" requirement. That's another job for the inductive logician.

($\mathcal{D}_3$) There must be *some* interesting "bridge principles" linking $\mathfrak{c}$ and *some* relations of evidential support, in *some* contexts.
  - ($\mathcal{D}_2$) implies that *if* there are any such bridge principles linking *entailment* and *conclusive evidence*, these should be *inherited by* $\mathfrak{c}$. This brings us back to Harman's problem!

Three Salient Quotes from Goodman [7]

☞ The "new riddle" is *about* inductive *logic* (*not epistemology*).

Quote #1 (page 67): "Just as deductive logic is concerned primarily with a relation between statements — namely the consequence relation — that is independent of their truth or falsity, so inductive logic ... is concerned primarily with a comparable relation of confirmation between statements. Thus the problem is to define the relation that obtains between any statement S_1 and another S_2 if and only if S_1 may properly be said to confirm S_2 in any degree."

Quote #2 (73): "Confirmation of a hypothesis by an instance depends ... upon features of the hypothesis other than its syntactical form".

☞ But, Goodman's *methodology* appeals to *epistemic* intuitions.

Quote #3 (page 73): "... the fact that a given man now in this room is a third son *does not increase the credibility of* statements asserting that other men now in this room are third sons, *and so does not confirm* the hypothesis that all men now in this room are third sons."