Notes for Week 10 of *Confirmation*

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1 Some Preliminary Cautionary Remarks on Formalization

Whenever one applies formal theories, one runs the risk of *over*-formalization (or *mis*-formalization). For instance, consider sentential logic. If all we have to work with is sentential logic, then many "intuitively logically valid" (hereafter, simply *valid*) arguments will "come out invalid". This is because *sentential validity* is a property that many valid arguments do not have. Roughly, an argument is sententially valid if it instantiates a valid sentential form. But, if an argument does not instantiate a valid sentential form, it does not follow that the argument is invalid (for instance, the argument expressed by: "Socrates is wise. Therefore, someone is wise."). So, whenever we use a formal logical theory, we must be careful not to infer too much from what that particular theory says about something informal that we're applying it to.

Probabilistic methods are also subject to this kind of cautionary remark. For instance, I might try to apply sentential probability theory to cases involving relationships that are intuitively non-sentential in nature. This isn't necessarily *verboten*. It just has to be done carefully, and in the right way. Here's a salient example. Let "A" be interpreted as $(\forall x)(Rx \supset Bx)$, "$B$" be interpreted as Ra, "C" be interpreted as Ba, and "K" be interpreted as some "background corpus", which captures what we take ourselves to know about the predicates R and B (as featured in our A, B, and C). To be more precise, we can also specify that "R" stands for ravenhood and "B" stands for blackness. Thus, we are assuming that "A" stands for *the proposition that all ravens are black*. In this sense, "A" (and even "B" and "C", for that matter) stands for something with monadic-predicate-logical (*i.e.*, *non*-sentential) structure. This is OK, as long as we keep in mind that:

☞ The **systematic** logical structure of any sentential probability model (*i.e.*, its *sentential* structure) will not be sensitive to any non-sentential logical relations among its "interpreted atomic sentences". As a result, any such relations must be explicitly represented by extra-systematic constraints on the model.

In our present example, there are **extra-systematic** (*viz.*, *non*-sentential) logical relations among our "atomic sentences" — *under their intended interpretations*. For instance, $(\forall x)(Rx \supset Bx)$ and Ra jointly entail Ba — *in monadic predicate logic*. But, this "entailment" is *extra-systematic*, and it will not be captured by any *systematic* sentential-logical relations between A, B, and C. After all, $A, B \nvDash C$ — *in sentential logic*. This is not necessarily a barrier to sentential formalization. In order to model things properly in this example, we'll need to add (at least) the following extra-systematic constraint to our sentential probability model:

- $\Pr(C \mid A \,\&\, B) = 1$.

Of course, this constraint is not implied by the axioms of sentential probability calculus. But, so long as we add it is an extra-systematic constraint (or, if you prefer, a "meaning postulate"), we'll be able to reason more appropriately about the example, under its intended interpretation. More generally, extra-systematic constraints needn't only come from *logical* relations that are (intuitively) present but "sententially inaccessible". [Note: we could also add extra-systematic constraints that are motivated by epistemic relations that we think hold in the case at hand, *etc.*, depending on the problems to which we intend to *apply* the model.]

One of the mistakes that I think Carnap (and others) have made in this literature is that he thought he needed to have every constraint *explicitly represented — systematically*. This leads to various problems that (to my mind) are of a mere technical nature, but which should not be barriers to probabilistic formalization. For instance, Carnap seems to want all deductive relations in examples to which his theories are applied to be expressible as *systematic* constraints within the object language $\mathcal{L}$ that $\Pr(\cdot \mid \cdot)$ ranges over. This leads to various problems that would be avoided if such constraints were handled instead as *extra-systematic* constraints on the models, which needn't be expressible in $\mathcal{L}$ (*e.g.*, they may involve explicit probabilistic constraints, as above). As we discussed last week (and as we will see again this week), Carnap's systematic approach seems to require a kind of "independence" of the atomic statements of $\mathcal{L}$, which often seems implausible (and which is unnecessary, from a broader "extra-systematic" perspective on the models). It also leads to the problem that all universal generalizations have zero probability on infinite domains. This is because of problems inherent in trying to put probability models on quantified rather than sentential languages. These problems don't arise unless we insist that all logical relations in the systems to which the models are applied are captured by systematic logical relations in $\mathcal{L}$ itself. None of these limitations is essential, it seems to me, to probabilistic formalization of the kinds of cases we have been discussing in the seminar so far. For instance, if we use a sentential *ABCK* model — with proper extra-systematic constraints — then we can model the "raven paradox" in a satisfactory and illuminating way, without having to worry about many of the problems faced by Carnapian monadic-predicate-logical models. This is the approach used in my ravens paradox paper "The Paradox of Confirmation" (which we discussed a few weeks back).

While the sort of "logical course-graining" I described above *can* be harmless in some cases, provided suitable extra-systematic constraints can be added, *etc.*, we can also sometimes engage in *too much* "coarse-graining". For instance, if we *are* working *within* the systems of Hempel or Carnap, which operate *systematically* (or "internally"), then it can be a mistake to engage in "coarse-graining". In fact, Goodman does this himself in his "grue" examples, and in a way that obscures some important nuances of logical structure. I will bring out (and try to remedy) this "coarse-graining" in my discussions this week and next. With these preliminary remarks about formalization out of the way, we're ready to discuss Goodman's 1946 paper.

2 Goodman's (1946) "Query on Confirmation"

There are some important differences between Goodman's early (1946) discussion of "grue"–type examples and his later (1955) discussion (in *Fact, Fiction, and Forecast*). I will mention some significant differences as we go along this week, and we will talk about his later discussion next week. Here's our main passage:

> Suppose we had drawn a marble from a certain bowl on each of the ninety-nine days up to and including VE day, and each marble drawn was red. We would expect that the marble drawn on the following day would also be red. So far all is well. Our evidence may be expressed by the conjunction "$Ra_1 \,\&\, Ra_2 \,\&\, \cdots \,\&\, Ra_{99}$", which well confirms the prediction "Ra_{100}". But increase of credibility, projection, "confirmation" in any intuitive sense, does not occur in the case of every predicate under similar circumstances. Let "S" be the predicate "is drawn by VE day and is red, or is drawn later and is non-red." The evidence of the same drawings above assumed may be expressed by the conjunction "$Sa_1 \,\&\, Sa_2 \,\&\, \cdots \,\&\, Sa_{99}$" By the theories of confirmation in question this well confirms the prediction "Sa_{100}"; but actually we do not expect that the hundredth marble will be non-red. "Sa_{100}" gains no whit of credibility from the evidence offered. It is clear that "S" and "R" can not both be projected here, for that would mean that we expect that a_{100} will and will not be red. It is equally clear which predicate is actually projected and which is not. But how can the difference between projectible and non-projectible predicates be generally and rigorously defined?

Before delving into the details, it is important to make a few preliminary remarks about this example. First, here Goodman defines "grue" ("S") using *logical opposites* red and non-red. Later, he (1955) uses green and blue, which are not logical opposites. I think it's better (for him) if logical opposites are used. So, in this sense, I like this earlier version better than the later version. Second, Goodman is talking entirely about *singular predictive induction* — universal induction is not even mentioned here (that also changes in his later 1955 discussion). Third, Goodman is talking, specifically, about the theories of Hempel and Carnap here. And, with respect to Carnap, he is talking about his *early* systems (this restriction turns out to be important here, since some surprises happen again in the later Carnapian systems). Fourth, Goodman's claims about being "*well* confirmed" are *quantitative* claims. But, these are inessential to Goodman's main criticism, which is fundamentally *qualitative* in nature. Goodman thinks there is *no* confirmation (by certain evidence E) for certain "S"–claims, but there is *some* confirmation for the corresponding "R"–claims. As such, we don't need there to be "99 instances" in our evidence E. We only need to talk about evidence E consisting of *one* instance. So, from now on (today), I will only discuss this problem in its *qualitative* form, involving a *single* instance. Finally, I will also limit my discussion to confirmation as *increase in firmness*, on the Carnapian side. But, I will begin from the point of view of Hempel's qualitative theory of confirmation.

Let the predicate "R" be interpreted as "is red", let "S" be interpreted as "is observed prior to 01/01/2008 and is red, or is not observed prior to 01/01/2008 and is non-red" (here, I've changed the definition of the "grue"-like predicate, so as to bring it up-to-date a bit). Thus, "S" actually has a finer-grained predicate-logical structure. Let "O" be interpreted as "is observed prior to 01/01/2008". Then, "Sx" can be expressed in a more fine-grained way, as "$Ox \equiv Rx$". Now, let E be an evidential proposition consisting of one "positive instance" a: "$Oa \,\&\, Ra$". Finally, consider the following two hypotheses about an object b: $H_1 \overset{\text{def}}{=} Rb$, and $H_2 \overset{\text{def}}{=} Sb = Ob \equiv Rb$. Now, what does Hempel's theory say about the $\mathfrak{C}$–relations between E and H_1/H_2?

1. E (indirectly) Hempel-confirms H_1. Here is the proof. First, $Oa\&Ra$ directly Hempel-confirms $(\forall x)Rx$, since $Oa \,\&\, Ra \vDash \mathrm{dev}_{Oa\&Ra}[(\forall x)Rx]$. Therefore, since $(\forall x)Rx \vDash Rb$, we have the result that E (indirectly) Hempel-confirms H_1. Note, $E' \overset{\text{def}}{=} Oa \equiv Ra = Sa$ does *not* Hempel-confirm H_1, since there is no set S such that for all $s \in S$, $Oa \equiv Ra \vDash s$, and s directly Hempel-confirms H_1. This is because one way of making $Oa \equiv Ra$ true is to have $\sim Oa \,\&\, \sim Ra$, which clearly won't suit present purposes.

2. E (indirectly) Hempel-confirms H_2. Here is the proof. First, $Oa \,\&\, Ra$ directly Hempel-confirms $(\forall x)(Ox \equiv Rx)$, since $Oa \,\&\, Ra \vDash \mathrm{dev}_{Oa\&Ra}[(\forall x)(Ox \equiv Rx)]$. $\therefore$ Since $(\forall x)(Ox \equiv Rx) \vDash Ob \equiv Rb$, we have the result that E (indirectly) Hempel-confirms H_2. Note, $E' \overset{\text{def}}{=} Oa \equiv Ra$ also (indirectly) Hempel-confirms H_2, by the same argument. This is an interesting *a*symmetry between H_1 and H_2.

I presume that Goodman was talking about E and not E' here (otherwise, his argument doesn't make as much sense). It seems that Goodman *et al* missed this distinction between E and E' because they *coarse-grained $Ox \equiv Rx$ into a single predicate Sx*.[1] However, once the distinction is made, it seems (to me) that Sa is more accurately rendered as $Oa \equiv Ra$, and not $Oa \,\&\, Ra$. But, if that's right, then this is a case in which "coarse-graining" *reverses a salient verdict*. Moreover, the verdict one gets on the proper fine-grained representation of the evidence is not the one Goodman needs for his argument to go through (as stated). One might be tempted to argue that this only makes Goodman's argument *stronger*, since Hempel's theory is giving the "exact opposite" of the "intuitive verdicts" on my rendering of Sa. But, this depends sensitively on the extra-systematic interpretation of "S" and "R". If we interpret "Rx" as "x is either *grue* and observed prior to 01/01/2008 or x is non-*grue* and not observed prior to 01/01/2008", and we take "Sx" $\overset{\text{def}}{=}$ "x is *grue*" as *primitive*, then Hempel's theory (and Carnap's, below) give "intuitively correct" verdicts: that Sb is not confirmed and $Ob \equiv Sb$ is confirmed by $E' \overset{\text{def}}{=} Oa \equiv Sa$. Note: these are *extra-systematically expressively equivalent* interpretations of the language. So, is Goodman offering an objection involving *language-relativity* here? How could that be? Hempel's theory is based on $\vDash$, which is *not* supposed to be language-relative. Is this a systematic/extra-systematic issue, rather than a language-relativity issue?

So, Hempel's theory entails that E confirms both H_1 and H_2. Goodman seems to think that this is a bad consequence for Hempel's theory, since "that would mean that we expect that a_{100} will and will not be red". But, this is somewhat odd, for two reasons. First, Hempel's theory is not a theory about what we would or should expect to happen or observe (presumably, on the basis of E) — *it is a theory about logical relations between statements*. Second, Hempel's theory implies (CC), and so it can never be the case that any E Hempel-confirms both p and $\sim p$, for any statement p. So, contrary to what Goodman suggests here, we do not have the consequence that Hempel's theory allows both Rb and $\sim Rb$ to be confirmed by E in this case. It is true that both Rb and $Ob \equiv Rb$ are confirmed by E. But, *these are not **systematically** logically incompatible hypotheses*. It is also true that both Rb and $\sim Ob \supset \sim Rb$ are confirmed by E as well. What, then, would happen if we built $\sim Ob$ into our "background corpus" K? Interestingly, this *still* doesn't make E Hempel-confirm both Rb and $\sim Rb$, relative to K (I'll leave that as an exercise). I think the main problem here is that Hempel's theory handles all constraints as *systematic* constraints. As a result, one can have the feeling that Hempel's theory is "missing something" in its failure to be able to incorporate extra-systematic constraints in a certain way. But, it seems to me that this is not an "internal" problem for Hempel's theory. It's more a problem involving the proper application of Hempel's theory in certain contexts. Apparently, Goodman wants Hempel's theory to say that E confirms H_1, but E does *not* confirm H_2. But, the *reason* he wants Hempel's theory to say this is, apparently, because he presupposes something like the following:

(BP) If E confirms H, then E "increases of credibility of" (or "evidentially supports") H (for some agent ϕ).

This is a "bridge principle" that connects the *logical* confirmation relation and some *epistemic* concept Goodman has in mind. But, we have to be careful about principles like (BP). Even in *de*ductive logic, such principles are often dubious. For instance, we might try one of the following deductive "bridge principles":

① If an agent S's belief set (or "acceptance set") B entails p (and S *knows* $B \vDash p$), then it would be reasonable for S to infer/believe p.

② If S knows that $B \vDash p$, then S should *not* be such that *both*: S believes $\bigwedge B$, *and* S does not believe p.

[1] Also, I suspect, because they have a tendency to slide between *objectual* and *propositional* senses of "evidence" — after all, you might say that a red object observed now *is* an S-object, and that an S-object observed now *is* a red object. While these statements *about an object* are true, they are not relevant to the relata of the confirmation relation, since those are *propositions* and not objects.

③ 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.

Principle ① is quite strong, and it is clearly false. There are various cases in which the right thing for S to do is to reject some $B_i \in B$, rather than accepting/inferring/believing p. Principle ② seems more plausible (it is "wide scope", rather than "narrow scope"), but it doesn't yield very interesting constraints on S (especially, regarding p), since S is probably never in a position where it would be reasonable for them to believe $\bigwedge B$ (indeed, they may not even be able to *grasp* the conjunction of everything they now believe/accept). Principle ③ may also seem more plausible than ①, but it seems less plausible than ②. Preface paradox cases seem to be counterexamples to ③ (especially, if we take "global" preface paradox cases in which $p = \bigwedge B$). While there is presumably *some* sort of connection between logic and epistemology, it is rather difficult to say precisely what this connection is — even in the deductive case. Having said that, I think the prospects are rather dim for defending Hempel by rejecting (BP). Hempel's theory seems hopeless to me (since it is monotonic, which I think even Hempel would have recognized as a fatal flaw). Continuing on with our discussion of the singular predictive version of "grue", we now consider what Carnap's theories say about this case.

Carnap's early theories are straightforward as applied to this case. We just need to look here at $\mathcal{L}_Q^{2,2}$ languages, where the two predicates are O and R. In the case of Carnap's first system $\mathfrak{c}^\dagger$, we have:

- According to $\mathfrak{c}^\dagger$, E confirms *neither* H_1 *nor* H_2 (relative to $\top$). This is because (in $\mathcal{L}_Q^{2,2}$) we have

$$\mathfrak{c}^\dagger(H_1 \mid E) = \mathfrak{c}^\dagger(H_1 \mid \top) = 1/2, \text{ and } \mathfrak{c}^\dagger(H_2 \mid E) = \mathfrak{c}^\dagger(H_2 \mid \top) = 1/2.$$

  In this sense, Carnap's $\mathfrak{c}^\dagger$ disagrees with Hempel's theory. Not surprisingly, Carnap's $\mathfrak{c}^\dagger$–theory also entails that $E' = Oa \equiv Ra$ confirms *neither* H_1 *nor* H_2 (relative to $\top$). Of course, the lack of "instantial relevance" (manifested here in the E'/H_2 relationship) is exactly why Carnap *abandoned* $\mathfrak{c}^\dagger$.

In Carnap's second system $\mathfrak{c}^*$, the story is quite different, of course:

- According to $\mathfrak{c}^*$, E confirms *both* H_1 *and* H_2 (relative to $\top$). This is because (in $\mathcal{L}_Q^{2,2}$) we have

$$\mathfrak{c}^*(H_1 \mid E) = 3/5 > \mathfrak{c}^*(H_1 \mid \top) = 1/2, \text{ and } \mathfrak{c}^*(H_2 \mid E) = 3/5 > \mathfrak{c}^*(H_2 \mid \top) = 1/2.$$

  Interestingly, Carnap's $\mathfrak{c}^*$–theory implies that E' does *not* confirm H_1, while it *does* confirm H_2! Thus, $\mathfrak{c}^*$ agrees with Hempel's theory (on all of these verdicts). As a result, my remarks above about the perils of "coarse-graining" in Hempelian theory also apply equally well to Carnap's $\mathfrak{c}^*$–theory.

To sum up so far: Hempel's theory and Carnap's $\mathfrak{c}^*$–theory both imply that E confirms H_1 and H_2 in Goodman's (1946) example. But, they also both imply that $E' = Oa \equiv Ra$ confirms H_2 but does not confirm H_1. As such, we must read Goodman as presupposing that the evidence E is really $Oa \,\&\, Ra$, and not $Sa = Oa \equiv Ra$ (which already makes Goodman's presentation somewhat misleading). But, no matter, we can run the argument for E instead of E', and then the argument does go through. We are, nonetheless, left to wonder *why* it is a bad thing that Hempelian and early Carnapian theories have these consequences.

What about Carnap's later theories? As it turns out, they have even different behavior in this case. Specifically, they entail that E confirms H_1 and E' confirms H_2. But, they do not entail that E confirms H_2 or that E' confirms H_1. See the last section of this handout for all the gory details (using Maher's λ/γ $\mathcal{L}_Q^{2,2}$–systems). However, if we assume that it is "known antecedently" that $\sim Ob$ (*i.e.*, that the background corpus $K \dashv\vDash \sim Ob$), then *all* of Carnap's "instantial relevance" theories (*i.e.*, $\mathfrak{c}^*$ and later) *do* entail that E (and E') confirms both H_1 and H_2, relative to K. As such, if one accepts Carnap's Requirement of Total Evidence:

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

then one will be led to the conclusion that E increases the credibility of H_1 and H_2, for agents ϕ whose total evidence consists of $\sim Ob$. And, *this* is surely a claim that Goodman means to reject. [Note, again, how "coarse-graining" the evidence as "Sa" rather than either "$Oa \,\&\, Ra$" or "$Oa \equiv Ra$" prevents us from seeing further ambiguities and subtleties lurking in the structure of Goodman's problem.] Of course, it could turn out that (RTE) is a bad principle, in which case not very much would follow from Goodman's example here.

Next week, I'll explain why anyone who applies confirmation as increase in firmness will have to reject (RTE) for reasons that have nothing to do with "grue". I'll also use this fact to defend Bayesians against "grue".

For now, I will continue our "early grue" discussion with the following variation on the example that Goodman uses to illustrate that the problem has little to do with "temporal content" of the predicate "S":

> That one predicate used in this example refers explicitly to temporal order is inessential. The same difficulty can be illustrated without the supposition of any order. Using the same letters as before, we need only suppose that the subscripts are merely for identification, having no ordinal significance, and that "S" means "is red and is not a_{100}, or is not red and is a_{100}.".

In this variation, we need a predicate Bx which is interpreted as "$x \neq b$". Then, I suppose the intended hypotheses in question are H_1: Rb, and H_2: $Rb \equiv Bb$. And, as before, we'll take $E \stackrel{\text{def}}{=} Ra \,\&\, Ba$, and $E' \stackrel{\text{def}}{=} Ra \equiv Ba$. Of course, we'll get the same verdicts as we did above from the Hempelian and Carnapian theories, since the (systematic) logical structure of the E's and the H's is identical in the two variations. This might seem odd, since the intended (extra-systematic) interpretation of B makes $Rb \equiv Bb$ *extra-systematically equivalent* to $\sim Rb$. And, so, it would appear that Hempel's theory gives the verdict that E confirms both Rb and $\sim Rb$. Of course, Hempel's theory gives no such verdict, but (again) only because it has no way of "taking on board" the extra-systematic constraint in question. Carnap's framework is in a similar position on this score.

If, on the other hand, we had a sentential framework of the kind I described at the beginning of these notes, we could easily accommodate these sorts of extra-systematic constraints. Here's how the story might go. We'll let A be interpreted as Ra, B be interpreted as Ba, C be interpreted as Rb, and D be interpreted as Bb. Then, E becomes $A \,\&\, B$, H_1 becomes C, and H_2 becomes $C \equiv D$. Goodman's intuition in this case is that we shouldn't have E confirming both H_1 and H_2. If we add the following extra-systematic constraint:

- $\Pr(D \mid \top) = 0$. [This makes perfect sense, since $D \stackrel{\text{def}}{=} Bb$ is extra-systematically equivalent to $b \neq b$.]

then $\Pr(H_1 \equiv \sim H_2 \mid \top) = 1$, and anything that confirms H_1 will disconfirm H_2 (and *vice versa*), just as Goodman wants. So, this rendition of Goodman's early "grue" problem poses no fundamental barrier to formalization of confirmation-theoretic relations. It does, however, suggest that adequate "fully systematic" formalizations are not always achievable. This shouldn't be terribly surprising, since we can't always give adequate "fully systematic" formalizations (in the required sense) in the deductive context either. Whatever logical framework one chooses, one can always give "extra-systematic" interpretations of some of the symbols of the framework, so as to cause trouble for any purportedly "fully systematic" formalization. For instance, in first-order logic, one could give extra-systematic second-order interpretations to some of the predicates and/or sentence letters, and this will induce "logical relations" that won't be explicitly captured in the systematic structure of the framework either. *This* "problem" is not peculiar to inductive logic. What does seem to be peculiar to inductive logic here is that no amount of fiddling with the *syntax* (or "logical grain") of the object language seems to allow these "systematic" confirmation theories to generate the "intuitively correct" verdicts. [Presumably, in the deductive case, we can always move to a richer logical theory that *does* systematize the desired logical relations? Although, examples like "bachelor ∴ unmarried" might give one pause even here.] All of the examples of this early (1946) paper seem to point toward the following:

☞ No adequate "fully systematic" formalization of inductive logic is forthcoming. That is, confirmation relations between propositions expressible in $\mathcal{L}$ do not supervene on the syntactical structure of $\mathcal{L}$.

While I'm inclined to agree with this point (partly, because I think the need for extra-systematic constraints is not a barrier to adequate formalization, and so I don't see this as having bad consequences), I suspect something similar can be said about *de*ductive logic. I don't say this because I think deductive and inductive logic are on the same footing with respect to what relations can be captured *syntactically*, in object languages (although, it's still not *completely* clear to me that they are not). Rather, I say this because, for Goodman, "adequacy" seems to require the ability to undergird a plausible (and non-vacuous) "bridge principle" between the logical concept in question and some salient epistemic concept. But, can this "desideratum" be met even in the *de*ductive case? Since inductive logic is supposed to *generalize* deductive logic, it will already have to presuppose a solution to what I will call "the bridge principle problem" for deductive logic. Perhaps Goodman thinks deductive logic *doesn't have* a "bridge principle problem"? Or, perhaps Goodman would have been swayed by an analogous "relevance-logic-based epistemological critique" of classical deductive logic? I'll bring this analogy more into focus at our next meeting. Meanwhile, some Carnapian details.

3 Details Concerning Carnap's Later Theories

Patrick Maher ("Probability Captures the Logic of Scientific Confirmation") gives a rendition of Carnap's later theories for the $\mathcal{L}_Q^{2,2}$ case. His system has the following four parameters (his notation): λ, γ_F, γ_G, and $\Pr(I)$. Maher recommends that we (always) set $\lambda = 2$ and $\Pr(I) = 1/2$.[2] This leaves only two parameters for $\mathcal{L}_Q^{2,2}$: $\gamma_F, \gamma_G \in (0, 1)$. Thus, we can represent Maher's $\mathcal{L}_Q^{2,2}$–system with the following stochastic truth-table:

Fa	Ga	Fb	Gb	State Descriptions (s_i)	$\mathfrak{m}^\dagger(s_i)$	$\mathfrak{m}^*(s_i)$	Maher's $\mathfrak{m}^\gamma(s_i)$
$\top$	$\top$	$\top$	$\top$	$Fa \,\&\, Ga \,\&\, Fb \,\&\, Gb$	1/16	1/10	$\frac{1}{9}\gamma_F\gamma_G\left(\gamma_G + \gamma_F(5\gamma_G+1)+2\right)$
$\top$	$\top$	$\top$	$\bot$	$Fa \,\&\, Ga \,\&\, Fb \,\&\, {\sim}Gb$	1/16	1/20	$-\frac{1}{9}\gamma_F(5\gamma_F+1)(\gamma_G-1)\gamma_G$
$\top$	$\top$	$\bot$	$\top$	$Fa \,\&\, Ga \,\&\, {\sim}Fb \,\&\, Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)\gamma_F\gamma_G(5\gamma_G+1)$
$\top$	$\top$	$\bot$	$\bot$	$Fa \,\&\, Ga \,\&\, {\sim}Fb \,\&\, {\sim}Gb$	1/16	1/20	$\frac{5}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)\gamma_G$
$\top$	$\bot$	$\top$	$\top$	$Fa \,\&\, {\sim}Ga \,\&\, Fb \,\&\, Gb$	1/16	1/20	$-\frac{1}{9}\gamma_F(5\gamma_F+1)(\gamma_G-1)\gamma_G$
$\top$	$\bot$	$\top$	$\bot$	$Fa \,\&\, {\sim}Ga \,\&\, Fb \,\&\, {\sim}Gb$	1/16	1/10	$\frac{1}{9}\gamma_F(\gamma_G-1)\left(\gamma_G+\gamma_F(5\gamma_G-6)-3\right)$
$\top$	$\bot$	$\bot$	$\top$	$Fa \,\&\, {\sim}Ga \,\&\, {\sim}Fb \,\&\, Gb$	1/16	1/20	$\frac{5}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)\gamma_G$
$\top$	$\bot$	$\bot$	$\bot$	$Fa \,\&\, {\sim}Ga \,\&\, {\sim}Fb \,\&\, {\sim}Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)(5\gamma_G-6)$
$\bot$	$\top$	$\top$	$\top$	${\sim}Fa \,\&\, Ga \,\&\, Fb \,\&\, Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)\gamma_F\gamma_G(5\gamma_G+1)$
$\bot$	$\top$	$\top$	$\bot$	${\sim}Fa \,\&\, Ga \,\&\, Fb \,\&\, {\sim}Gb$	1/16	1/20	$\frac{5}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)\gamma_G$
$\bot$	$\top$	$\bot$	$\top$	${\sim}Fa \,\&\, Ga \,\&\, {\sim}Fb \,\&\, Gb$	1/16	1/10	$\frac{1}{9}(\gamma_F-1)\gamma_G\left(\gamma_F+(5\gamma_F-6)\gamma_G-3\right)$
$\bot$	$\top$	$\bot$	$\bot$	${\sim}Fa \,\&\, Ga \,\&\, {\sim}Fb \,\&\, {\sim}Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)(5\gamma_F-6)(\gamma_G-1)\gamma_G$
$\bot$	$\bot$	$\top$	$\top$	${\sim}Fa \,\&\, {\sim}Ga \,\&\, Fb \,\&\, Gb$	1/16	1/20	$\frac{5}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)\gamma_G$
$\bot$	$\bot$	$\top$	$\bot$	${\sim}Fa \,\&\, {\sim}Ga \,\&\, Fb \,\&\, {\sim}Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)\gamma_F(\gamma_G-1)(5\gamma_G-6)$
$\bot$	$\bot$	$\bot$	$\top$	${\sim}Fa \,\&\, {\sim}Ga \,\&\, {\sim}Fb \,\&\, Gb$	1/16	1/20	$-\frac{1}{9}(\gamma_F-1)(5\gamma_F-6)(\gamma_G-1)\gamma_G$
$\bot$	$\bot$	$\bot$	$\bot$	${\sim}Fa \,\&\, {\sim}Ga \,\&\, {\sim}Fb \,\&\, {\sim}Gb$	1/16	1/10	$\frac{1}{9}(\gamma_F-1)(\gamma_G-1)\left(-6\gamma_G+\gamma_F(5\gamma_G-6)+9\right)$

I have posted a *Mathematica* notebook, which (a) verifies claims about arbitrary $\mathfrak{m}^\gamma$-models, and also (b) finds $\mathfrak{m}^\gamma$-models meeting user-specified constraints (if they are satisfiable). This allowed me to show that $E \overset{\text{def}}{=} Oa \,\&\, Ra$ needn't confirm $H_2 \overset{\text{def}}{=} Ob \equiv Rb$, and $E' \overset{\text{def}}{=} Oa \equiv Ra$ needn't confirm $H_1 \overset{\text{def}}{=} Rb$. The notebook also verifies that E must confirm H_1, and E' must confirm H_2. In any event, Goodman's central claims are no longer true for the later Carnapian systems. This is despite what Maher suggests in his paper (where he "coarse-grains" the "grue" predicate, and is thus unable to see these nuances that are implicit in his own models). Here's a summary of the confirmational facts that obtain in the four theories we've seen above:

	Does E confirm H?	
Hempel	$H_1 \overset{\text{def}}{=} Rb$	$H_2 \overset{\text{def}}{=} Ob \equiv Rb$
$E \overset{\text{def}}{=} Oa \,\&\, Ra$	Always	Always
$E' \overset{\text{def}}{=} Oa \equiv Ra$	Never	Always

	Does E confirm H?	
Carnap ($\mathfrak{c}^\dagger$)	$H_1 \overset{\text{def}}{=} Rb$	$H_2 \overset{\text{def}}{=} Ob \equiv Rb$
$E \overset{\text{def}}{=} Oa \,\&\, Ra$	Never	Never
$E' \overset{\text{def}}{=} Oa \equiv Ra$	Never	Never

	Does E confirm H?	
Carnap ($\mathfrak{c}^*$)	$H_1 \overset{\text{def}}{=} Rb$	$H_2 \overset{\text{def}}{=} Ob \equiv Rb$
$E \overset{\text{def}}{=} Oa \,\&\, Ra$	Always	Always
$E' \overset{\text{def}}{=} Oa \equiv Ra$	Never	Always

	Does E confirm H?	
Maher ($\mathfrak{c}^\gamma$)	$H_1 \overset{\text{def}}{=} Rb$	$H_2 \overset{\text{def}}{=} Ob \equiv Rb$
$E \overset{\text{def}}{=} Oa \,\&\, Ra$	Always	Not Always
$E' \overset{\text{def}}{=} Oa \equiv Ra$	Not Always	Always

If we conditionalize $\mathfrak{c}$ on ${\sim}Ob$, then all the cells in the last two tables (for $\mathfrak{c}^*$ and $\mathfrak{c}^\gamma$) become "Always". This is why all of these theories of confirmation are still susceptible to an "epistemic critique" *via* the (RTE).

[2] Allowing λ and $\Pr(I)$ to be adjust*able* parameters as well does not change any of the salient verdicts below. So, it isn't too important what values (if any) one insists on for these parameters. It's the other two parameters [γ_F and γ_G, which are the "a priori" probabilities of Fa and Ga: $\mathfrak{m}^\gamma(Fa)$ and $\mathfrak{m}^\gamma(Ga)$] that are doing the work here. See my *Mathematica* notebook for all the details.