Notes for Week 6 of *Confirmation*

10/10/07

Branden Fitelson

1 Hempel's (Rather Odd) Reconstruction of Nicod

We'll discuss Hempel's theory of confirmation in more depth next week. But, it was clearly inspired by some of Nicod's earlier, inchoate remarks about instantial confirmation (which we saw last week), 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.

While Nicod is not very clear on the logical details of his logical-probability-raising account of instantial confirmation, three aspects of Nicod's conception of confirmation are apparent here:

- Instantial confirmation is a relation between singular and general propositions/statements (or, if you will, between "facts" and "laws").
- Confirmation consists in *positive probabilistic relevance*, and disconfirmation consists in *negative probabilistic relevance* (where the salient probabilities are "inductive" or "logical", as discussed last week).
- Universal generalizations are confirmed by their positive instances and disconfirmed by their negative instances (indeed, Nicod also endorses an asymmetry here, as we discussed last week).

Hempel 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 unfortunate decision on Hempel's part 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 seems to be something like the following principle:

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

As Hempel explains, (NC_0) has some rather unfortunate 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. But, I think Hempel's reconstruction of Nicod is uncharitable on this score. I don't see any evidence that Nicod would have defended (NC_0). Hempel's own theory (details next week) has a related consequence that Nicod *would* have defended [in addition to (EQC_H)]. That consequence is:

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

This principle, which is assumed (and defended) by most of the early pioneers of inductive logic, will play an important role in most of the controversies we will discuss in the coming weeks [Only the first conjunct of (NC) is controversial. The second will be satisfied by all the (logical) theories of confirmation we discuss.]

2 The Paradox of Confirmation

2.1 The Canonical Argument for the "Paradoxical Conclusion"

Principles (EQC_H) and (NC) may *seem* rather innocuous, but they run up against intuitions rather quickly. The so-called "paradox of confirmation" (a.k.a., the "raven paradox") arises from just these two principles. Let $Bx \overset{\text{def}}{=} x$ is black, and $Rx \overset{\text{def}}{=} x$ is a raven. The following argument establishes a "paradoxical conclusion":

(1) By (NC), $\sim Ba \,\&\, \sim Ra$ confirms $(\forall x)(\sim Bx \supset \sim Rx)$.

(2) By Logic, $(\forall x)(\sim Bx \supset \sim Rx) \dashv\vDash (\forall x)(Rx \supset Bx)$.

(PC) $\therefore$ By (1), (2), (EQC_H), $\sim Ba \,\&\, \sim Ra$ confirms $(\forall x)(Rx \supset Bx)$.

This conclusion (PC) is supposed to be paradoxical. But, one wonders *why*, exactly. One thought is that (PC) sanctions what Goodman calls "indoor ornithology". It may *seem* that (PC) implies, for instance, that $Wa \,\&\, Sa$ confirms $(\forall x)(Rx \supset Bx)$, where $Wx \overset{\text{def}}{=} x$ is white, and $Sx \overset{\text{def}}{=} x$ is a shoe. But, it does *not*, unless certain other (dubious) assumptions are also made. As it turns out, Hempel (and Goodman) made just such assumptions (although, they have not been widely discussed). Specifically, Hempel's theory entails:

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

(M) is a very powerful (and counter-intuitive — even by Hempel's own lights — see below) assumption. Once you have (M) on board, lots of things start to sound strange. *Assuming* (M), (PC) *does* imply that $Wa \,\&\, Sa$ confirms $(\forall x)(Rx \supset Bx)$, since $Wa \,\&\, Sa$ is equivalent to $\sim Ba \,\&\, \sim Ra \,\&\, \psi a$, for some predicate ψ. In this sense, (M) is the missing premise underlying Goodman's "indoor ornithology" remark. We'll talk more about (M) in the coming weeks — it also plays a central role in Goodman's "grue" paradox.

2.2 The Hempel/Goodman Response to the Paradox

Hempel and Goodman *embraced* (NC), (EC) *and* (PC). They saw no paradox. They *explain away* the paradoxical *appearance*, as follows:

> ... in the seemingly paradoxical cases of confirmation, we are often not judging the relation of the given evidence E *alone* to the hypothesis H ... instead, we tacitly introduce a comparison of H with ... E in conjunction with ... additional ... information we ... have at our disposal.

The idea here is that E [$\sim Ra \& \sim Ba$] confirms H [$(\forall x)(Rx \supset Bx)$] *relative to some impoverished background corpus* $K_\top$, but E doesn't confirm H *relative to some richer background knowledge* $K \neq K_\top$. In other words, *confirmation is not monotonic in the background corpus* K. OK, let's run with this for a bit (it's actually a very good idea, even though it will turn out to *contradict* Hempel's *theory*!). *Which* $K \neq K_\top$, precisely, is Hempel talking about? The answer to this question can be gleaned from Hempel's discussion of the "salts" example: $K \vDash \sim Ra$. The idea is that if you already know that $\sim Ra$, then observing a's color won't tell you anything about the color of ravens. Hempel (and Goodman) implore us to distinguish the following two claims:

(PC) $\sim Ra \,\&\, \sim Ba$ confirms $(\forall x)(Rx \supset Bx)$, *relative to* $K_\top$.

(PC*) $\sim Ra \,\&\, \sim Ba$ confirms $(\forall x)(Rx \supset Bx)$, *relative to some* $K \neq K_\top$ *such that* $K \vDash \sim Ra$.

The intuition Hempel and Goodman have here can be expressed as follows:

($\mathcal{I}$) (PC) is true, but (PC*) is false. [*Why?* $\sim Ra$ reduces the size of the set of possible *counterexamples* to $(\forall x)(Rx \supset Bx)$. Note: this sounds like *Keynes* in his chapter on analogy! More recently, Patrick Maher has been an eloquent advocate of this reading of Hempel.]

Nice idea! Sadly, ($\mathcal{I}$) is *inconsistent* with the confirmation *theory* of Hempel (and Goodman)! ($\mathcal{I}$) contradicts (M). To see this, note that there is no difference in classical deductive logic between "E entails H, relative to background theory K" [$E \vDash_K H$], and "$E \,\&\, K$ entails H, relative to impoverished (tautological) background theory" $\top$ [$E \,\&\, K \vDash H$]. As we'll see next week, given the way Hempel defines confirmation, this implies both (M) and *not*-($\mathcal{I}$). What's needed here to obtain a response of this kind to the paradox is a *non*-monotonic theory of confirmation (say a *probabilistic* one — like Nicod and Keynes already described!).

2.3 Probabilistic Approaches to the Paradox

There have been a great many probabilistic approaches to the "paradox". In this section, I'll examine several "canonical" probabilistic approaches to the paradox. Most of them are inspired by the work of JHL and I.J. Good. Hempel has a long footnote about JHL's approach in this week's readings (I.J. Good's work came later). There, he seems somewhat sympathetic to it, but he ultimately does not endorse it, for reasons we'll discuss shortly (it is worth noting that, in his later work he reconsidered this position on JHL's approach). Interestingly, Hempel doesn't seem to realize that he needs something like a probabilistic approach in order for his own [(PC)/(PC*)] approach to make sense. By his own lights, he at least needs something *non-monotonic* here, and that's much closer to probabilistic frameworks than his own. In the end, I'll suggest a "Probabilistic/Hempelian synthesis" approach to the paradox, which will improve upon JHL's basic idea.

2.3.1 Qualitative Probabilistic Approaches to the Paradox

One way to respond to the paradox is to try to deny one (or more) of the qualitative principles on which it rests. Since it rests on only two principles, this means rejecting either (NC) or (EQC$_H$). There are some people (*e.g.*, relevance logicians) who deny (EQC$_H$), but I'm going to ignore those approaches (Ken Gemes, for instance, has a probabilistic approach to the paradox, based on relevance logic). This leaves only the rejection of (NC) as a live option. Many people have opted for this sort of approach.

I.J. Good was the first to present a concrete probabilistic-relevance counterexample to (NC). The key to the counterexample is that it exploits the *three*-place nature of probabilistic-relevance confirmation relations. Recall, $\mathfrak{C}(H, E \mid K)$ iff $\Pr(E \mid H \,\&\, K) > \Pr(E \mid {\sim}H \,\&\, K)$. This means that, from a probabilistic-relevance point of view, (NC) is *ambiguous*. It's missing a quantifier over the background corpus K. Four alternatives:

(NC$_w$) $\quad (\forall\phi)(\forall\psi)(\forall x)\boldsymbol{(\exists K)}[\mathfrak{C}((\forall y)(\phi y \supset \psi y), \phi x \,\&\, \psi x \mid \boldsymbol{K})]$

(NC$_\alpha$) $\quad (\forall\phi)(\forall\psi)(\forall x)[\mathfrak{C}((\forall y)(\phi y \supset \psi y), \phi x \,\&\, \psi x \mid \boldsymbol{K_\alpha})]$

(NC$_\top$) $\quad (\forall\phi)(\forall\psi)(\forall x)[\mathfrak{C}((\forall y)(\phi y \supset \psi y), \phi x \,\&\, \phi x \mid \boldsymbol{K}_\top)]$

(NC$_s$) $\quad \boldsymbol{(\forall K)}(\forall\phi)(\forall\psi)(\forall x)[\mathfrak{C}((\forall y)(\phi y \supset \psi y), \phi x \,\&\, \psi x \mid \boldsymbol{K})]$

(NC$_w$) is *too weak*. For each E and H, it is inevitable that there will exist *some* K such that $\mathfrak{C}(H, E \mid K)$. Just let $K \vDash H \supset E$. (NC$_s$) is *too strong*. It is *demonstrably false*. I.J. Good showed this, with examples like:

Let K be: Exactly one of the following two hypotheses is true: (H) there are 100 black ravens, no nonblack ravens, and 1 million other things in the universe [*viz.*, $(\forall x)(Rx \supset Bx)$], or ($\sim H$) there are 1,000 black ravens, 1 white raven, and 1 million other things.

Let E be $Ra \,\&\, Ba$ (a randomly sampled from the universe). Then:

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

$\therefore$ K, R, B, a are such that *not*-$\mathfrak{C}((\forall x)(Rx \supset Bx), Ra \,\&\, Ba \mid K)$. And so Good's example is indeed a counterexample to (NC$_s$).

That leaves (NC$_\alpha$) and (NC$_\top$) as the two remaining contenders. (NC$_\alpha$) is supposed to be taken relative to some "actual" or "currently scientifically reasonable" background corpus (K_α), and (NC$_\top$) is supposed to be taken relative to some "empty" or "tautological" or "a priori" background corpus $K_\top$.

Hempel responded to Good's counterexample, above, by complaining that, at best, it could be parlayed into a counterexample to (NC$_\alpha$). He insisted that (NC) must be interpreted as (NC$_\top$). As such, Hempel did not see Good's counterexample as a threat to the (NC) entailed by his own theory of confirmation. Two points:

1. Hempel's own theory of confirmation *contradicts* the possibility of there being a counterexample to (NC$_\alpha$) if there is no counterexample to (NC$_\top$). On Hempel's theory, if (NC$_\top$) is true, then so is (NC$_\alpha$), since $K_\alpha \vDash K_\top$. So, Hempel's complaint makes no sense, given his *monotonic* [(M)-ish] theory.

2. Hempel's warning about the perils of conflating (PC) and (PC*) only claims that if $K \vDash \sim Ra$, then this can undermine the confirmation that would otherwise obtain, relative to an "impoverished" corpus $K_\top \nvDash \sim Ra$. Hempel says nothing about the "perils" of K entailing the kind of *statistical* information that Good's background corpus contains. And, it is not at all clear that such information should undermine the confirmation relation here (recall, there was an *argument* for the undermining by $\sim Ra$).

Of course, Good did not point either of these things out. Instead, he responded by trying to describe a counterexample to ($NC_\top$) itself. Here is the example, which is sometimes called "Good's Baby":

> Imagine an infinitely intelligent newborn baby having built-in neural circuits enabling him to deal with formal logic, English syntax, and subjective probability. He might argue, after defining a crow in detail, that it is initially extremely unlikely that there are any crows, and ∴ it is extremely likely that all crows are black ... [but] if there are crows, then there is a reasonable chance they are a variety of colours ... ∴ if he were to discover that a black crow exists he would consider [H] to be less probable than it was initially.

This example is somewhat tongue-in-cheek, of course. But, it's actually not a terrible example. Patrick Maher has recently shown that Carnapian theories of "confirmation relative to $K_\top$" also allow for counterexamples to ($NC_\top$). We'll talk more about that when we get to Carnap. Meanwhile, here's Maher's intuitive example:

> According to standard logic, 'All unicorns are white' is true if there are no unicorns. Given what we know, it is almost certain that there are no unicorns and hence 'All unicorns are white' is almost certainly true. But now imagine that we discover a white unicorn; this astounding discovery would make it no longer so incredible that a non-white unicorn exists and hence would disconfirm 'All unicorns are white.'

Maher slips here by saying "given what we know", which sounds like he's talking about what we *actually* know *empirically*. In this sense, it seems Maher may have a counterexample to (NC_α) here, but not (obviously) to ($NC_\top$). If one takes Carnap's theory of "$\mathfrak{C}(H, E \mid K_\top)$" seriously, though, then Maher's formal Carnapian counterexamples *are* probative in this respect. The crucial question here, however, is what is going to count as a counterexample to (NC), as it is intended in the argument for the "paradoxical conclusion". Similar issues arise in *comparative* probabilistic approaches to the paradox, to which I will now turn.

2.3.2 Comparative Probabilistic Approaches to the Paradox

The I.J. Good approach is to deny the soundness of the argument for (PC). JHL, on the other hand, takes a different tack. She argues that *even if* the argument for (PC) is sound, we can still "soften the impact" of the paradox by arguing for a certain *comparative* confirmation-theoretic claim. Let $E_1 \overset{\text{def}}{=} Ra \,\&\, Ba$, $E_2 \overset{\text{def}}{=} \sim Ba \,\&\, \sim Ra$, and $H \overset{\text{def}}{=} (\forall x)(Rx \supset Bx)$. JHL's strategy is to argue for the following claim:

(†) $\mathfrak{c}(H, E_1 \mid K_\alpha) > \mathfrak{c}(H, E_2 \mid K_\alpha)$

For JHL, however, $\mathfrak{c}$ is just *conditional probability* — it is not a *relevance* measure of degree of confirmation. As it turns out, however, it can be shown that for most relevance measures (at least, the most popular ones in the literature), we have the following relationship between Pr and $\mathfrak{c}$ in examples with this structure:

(‡) If $\Pr(H \mid E_1 \,\&\, K) > \Pr(H \mid E_2 \,\&\, K)$, then $\mathfrak{c}(H, E_1 \mid K_\alpha) > \mathfrak{c}(H, E_2 \mid K_\alpha)$.

This means that, for practical purposes, we can just focus on establishing $\Pr(H \mid E_1 \,\&\, K) > \Pr(H \mid E_2 \,\&\, K)$. For the remainder, I will assume that we are only talking about relevance measures $\mathfrak{c}$, which satisfy (‡).

With that background in place, we are ready to look at the assumptions JHL makes in her argument for (†). Basically, all traditional approaches to the paradox rest on these same three assumptions.

(a) $\Pr(\sim Ba \mid K_\alpha) > \Pr(Ra \mid K_\alpha)$

(b) $\Pr(Ra \mid H \,\&\, K_\alpha) = \Pr(Ra \mid K_\alpha)$ [$\therefore \Pr(\sim Ra \mid H \,\&\, K_\alpha) = \Pr(\sim Ra \mid K_\alpha)$!]

(c) $\Pr(\sim Ba \mid H \,\&\, K_\alpha) = \Pr(\sim Ba \mid K_\alpha)$ [$\therefore \Pr(Ba \mid H \,\&\, K_\alpha) = \Pr(Ba \mid K_\alpha)$!]

It is a theorem of the probability calculus that (a)–(c) jointly entail (†).[1] The idea behind this approach is that, while E_2 may confirm H, it confirms H *less strongly* than E_1 does, given assumptions (a)–(c), which are supposed to be "plausible", relative to what we actually know K_α. While this doesn't show that (PC) fails, or even that the argument for (PC) is unsound, one might take it to "soften the impact" of the paradox.

Hempel, of course, won't be satisfied with this approach, since it (at best) only makes sense, when taken relative to K_α. Specifically, it is difficult to see what could justify an assumption like (a), if it is taken relative to $K_\top$. Perhaps there are far more non-black objects in the universe than there are ravens, but that seems like an empirical claim (like Good's statistical claims, above), which Hempel does not think should be allowed into the salient K. [As I explained above, I don't really see how such a response is open to Hempel, given that his theory is monotonic and that his (PC)/(PC*) distinction doesn't say anything about such "statistical" information being allowed into K.] On the other hand, the independence assumptions (b) and (c) may seem less "empirical", but they also seem much stronger and less plausible as well. Note, specifically, that (b) entails the independence of $\sim Ra$ to H. And, this is something that someone who endorses Hempel's (PC)/(PC*) explanation should *reject*. Recall, this view (especially well articulated by Maher) has it that learning $\sim Ra$ should *indirectly support* H by reducing the number of possible counterexamples to H. But, if (b) is accepted, then it is incompatible with $\sim Ra$ being positively relevant to H (by either direct or indirect means). Similar remarks can be made about Ba and H in connection with (c). As such, this is another reason for a "Hempelian" (in this sense) to reject such a response to the paradox. Finally, while it is true that (a)–(b) entail (†), they also entail various other claims that one (Hempelian or not) might not want to accept. *E.g.*:

(d) $\Pr(H \mid Ra \,\&\, Ba \,\&\, K_\alpha) > \Pr(H \mid K_\alpha)$

(e) $\Pr(H \mid \sim Ba \,\&\, \sim Ra \,\&\, K_\alpha) > \Pr(H \mid K_\alpha)$

(f) $\Pr(H \mid Ba \,\&\, \sim Ra \,\&\, K_\alpha) < \Pr(H \mid K_\alpha)$

Claim (d) is just the salient instance of (NC_α). Thus, (a)–(c) *force* JHL to accept (NC) in this case. While this is not something that will bother a *Hempelian*, it is something that might well bother a *probabilist*. The counterexamples of Good and Maher should make probabilists wary of being *forced* to accept instances of (NC). More generally speaking, a *comparative* account should not be forced to take a stand on such *qualitative* claims. Claim (e) is is the salient instance of (PC_α). It is the "paradoxical conclusion". While a comparative approach should be consistent with the truth of (PC), it shouldn't be *forced* to accept it. Presumably, it would be "even better" if it turns out that (PC_α) is *false*. The comparative account need only *kick-in if it turns out that* (PC_α) is true, but it shouldn't *entail* that (PC_α) is true. Finally, (f) is a very odd consequence that is forced upon the JHL-style (a)–(c) comparative response to the paradox. It says that $Ba \,\&\, \sim Ra$ must *dis*confirm H, relative to K_α. That is a very odd claim, especially in light of Hempelian considerations. After all, $Ba \,\&\, \sim Ra$ rules-out a as a counterexample to the law *in two ways*. And, yet, not only doesn't this lead to (indirect) confirmation of H (as Hempel and Maher would suggest), it instead leads to *disconfirmation* of H, assuming (a)–(c)! This is an undesirable consequence, I would say, for both Hempelians and probabilists (it is often raised as a criticism of JHL-style approaches to the paradox).

In light of these considerations, it would be nice to have a "purely comparative" response to the paradox, which did not have any of these undesirable implications (d)–(f), and which *didn't force any* such qualitative claims. As it turns out, such an approach does exist. The key is to find assumptions weaker than (b)–(c), which preserve the desired comparative claim (†). Jim Hawthorne and I discovered a replacement for (b)–(c):

($\star$) $\Pr(H \mid Ra \,\&\, K_\alpha) \geq \Pr(H \mid \sim Ba \,\&\, K_\alpha)$

It can be shown that (a) & ($\star$) jointly entail (†), but they do *not* entail any of (d)–(f). Moreover, they are consistent with the spirit of the Hempelian (PC)/(PC*) approach, since they are compatible with the following probabilistic rendition of it (this is the claim Maher uses in his Carnapian reconstruction of Hempel):

(g) $\mathfrak{c}(H, \sim Ba \,\&\, \sim Ra \mid K_\alpha) > \mathfrak{c}(H, \sim Ba \,\&\, \sim Ra \mid \sim Ra \,\&\, K_\alpha) = 0$

In this way, we have improved upon JHL's approach to the paradox, while preserving Hempel's (PC)/(PC*) distinction. I would call this a "Hempel/Bayes compromise" approach to the paradox. This illustrates just how powerful and flexible the probabilistic framework for confirmation theory is. It enables us to do things that are impossible for a theory like Hempel's, which is based on entailment (and thus monotonic, and unable to make these sorts of nuanced distinctions). We'll discuss the details of Hempel's theory next week.

[1] We also assume in the background that $\Pr(Ba \mid Ra \,\&\, H) = 1$, and that all salient conditional probabilities are well-defined, *etc.*

2.3.3 Quantitative Approaches to the Paradox

Some probabilistic confirmation-theorists have tried to "strengthen" the comparative approaches, by arguing for the following sort of "quantitative enhancement" of (†):

($\dagger_{\mathfrak{c}}$) $\mathfrak{c}(H, E_1 \mid K_\alpha) > \mathfrak{c}(H, E_2 \mid K_\alpha) \approx 0$

I'm calling this ($\dagger_{\mathfrak{c}}$) because now it depends very sensitively on *which* relevance measure $\mathfrak{c}$ is used to gague degree of confirmation. In order to make this precise we'll also need a precise story about what "$\approx$" means. For simplicity, let's assume that "$x \approx y$" means $|x - y| < \epsilon$, for some pre-specified $\epsilon > 0$.

An important issue that arises immediately here is that different confirmation measures can (intuitively) impose different *numerical scales*. That is, what counts as "small" for one measure might be different than what counts as "small" for another measure — even ones that agree on comparative claims about what is more or less well confirmed than what. As such, we should probably have a relativized notion "$\approx_{\mathfrak{c}}$" of "approximately the same degree of confirmation, *according to* $\mathfrak{c}$." Perhaps this would also mean having different $\epsilon_{\mathfrak{c}}$'s for various $\mathfrak{c}$'s. In any event, I think this is a fundamental problem, and that we should probably stick to the comparative structure imposed by a confirmation measure. Having said that, it is worth noting that many philosophers have nonetheless persisted in trying to argue for ($\dagger_{\mathfrak{c}}$), for some large class of measures $\mathfrak{c}$. This involves specifying a set of conditions that implies ($\dagger_{\mathfrak{c}}$) for many (at least the "popular") $\mathfrak{c}$'s. One set of assumptions that is commonly used is (b)–(c), together with the following amendment of (a):

(a′) $\Pr(\sim Ba \mid K_\alpha) \gg \Pr(Ra \mid K_\alpha)$

To make this concrete, let's assume that we're measuring degree of confirmation according to the difference measure $d(H, E \mid K) = \Pr(H \mid E \,\&\, K) - \Pr(H \mid K)$. Now, assume (b), (c), and the following precisification of (a′):

(a'_d) $\dfrac{\Pr(Ra \mid K_\alpha)}{\Pr(\sim Ba \mid K_\alpha)} = \epsilon_d$

Then, it can be shown that (a'_d), (b), and (c) jointly entail:

$$d(H, E_2 \mid K_\alpha) \leq \frac{\Pr(H \mid K_\alpha) \cdot \epsilon_d}{1 - \epsilon_d}$$

This number will, in general, be "small" if ϵ_d is "small". But, it can be greater than ϵ_d, provided that $\Pr(H \mid K_\alpha)$ is sufficiently large. Nonetheless, for "middling" size $\Pr(H \mid K_\alpha)$, it does follow that $d(H, E_2 \mid K_\alpha)$ will be less than ϵ_d, provided that (a'_d), (b), and (c) are true. Similar results can be proved for other relevance measures of degree of confirmation. Moreover, Peter Vranas has shown that (for a large class of relevance measures $\mathfrak{c}$) if ($\dagger_{\mathfrak{c}}$) holds, then (c) is "approximately true". More accurately, ($\dagger_{\mathfrak{c}}$) implies (for many $\mathfrak{c}$'s):

(c′) $\Pr(\sim Ba \mid H \,\&\, K_\alpha) \approx \Pr(\sim Ba \mid K_\alpha)$

Thus, "approximate independence" of $\sim Ba$ and H is *necessary* for ($\dagger_{\mathfrak{c}}$), for a wide-variety of $\mathfrak{c}$'s. This is an interesting result. But, it is not as un-Hempelian as it might sound. After all, (c′) is compatible with Ba and H *not* being "approximately independent". That is, (c′) is compatible with the following:

- $\Pr(Ba \mid H \,\&\, K_\alpha) \not\approx \Pr(Ba \mid K_\alpha)$

As such, Vranas's result is not as problematic (from a Hempelian point of view) as it might at first appear. Moreover, keep in mind that this sort of "quantitative" approach inherits all the undesirable consequences of the qualitative approach, since its assumptions are strictly stronger. For this reason, I would say that such approaches already face powerful challenges that are independent of the "extra" consequences implied by the quantitative strengthening. And, since I don't really know how to make sense of talk of "small" *vs* "large" degrees of confirmation (in a way that is commensurable across different measures of confirmation), I would prefer to stick to making only qualitative and comparative claims in the context of confirmation theory. Next week, we'll look more carefully at the logical details of Hempel's theory of confirmation, and how it compares (in terms of various properties and *desiderata*) to various probabilistic approaches.