Judgment Under Uncertainty Revisited: Probability *vs* Confirmation

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- Carnap [1] aims to provide a formal explication of an informal concept (relation) he calls "confirmation".
- He clarifies "E confirms H" in various ways, including:
  - ($*$) E provides some positive evidential support for H.
- His formal explication of "E confirms H" (in [1]) is:
  - (1) E confirms H iff $\Pr(H \mid E) > r$, where Pr is a suitable ("logical") probability function, and r is a threshold value.
- Unfortunately, Carnap [1] is not entirely consistent in his formal analyses and applications of confirmation.
- Popper [13] points out that in some parts of [1], Carnap has a *different* explication of confirmation in mind, namely:
  - (2) E confirms H iff $\Pr(H \mid E) > \Pr(H)$, where Pr is a suitable ("logical") probability function. [*i.e.*, *correlation* under Pr]
- In response to Popper, Carnap [2] postulated an *ambiguity* in the concept of confirmation [(1)- *vs* (2)-confirmation].
- To some modern readers (*e.g.*, *me*), this seems inadequate, since (2) seems to be a *better explication* of the informal concept ($*$) that Carnap aimed to explicate in the first place.

- To see why (2) is more similar to ($*$) than (1) is, note that (1) can be satisfied *even if E* ***lowers*** *the probability of H*.
- Example: Let H be the hypothesis that John does *not* have HIV, and let E be a *positive* test result for HIV from a highly reliable test. Plausibly, in such cases, we could have both:
  - $\Pr(H \mid E) > r$, for just about any threshold value r, but
  - $\Pr(H \mid E) < \Pr(H)$, since E *lowers* the probability of H.
- So, if we adopt Carnap's (1)-explication, then we must say that E confirms H in such cases. But, in ($*$)-terms, this implies E provides some *positive evidential support for H*!
- I take it we don't want to say *that*. Intuitively, what we want to say here is that, while H is (still) *highly probable given E*, (nonetheless) E provides (strong?) evidence ***against*** H.
- Rather than *ambiguity*, I'd say this reflects *confusion* about the nature of the concept ($*$) Carnap was trying to explicate.
- Even Carnap [2] says (2) is "more interesting" than (1).
- Contemporary (Bayesian) confirmation theorists seem to agree. They no longer think of confirmation in (1)-terms …

- Bayesianism assumes that the *epistemic* degrees of belief (that is, the *credences*) of rational agents are *probabilities*.
- Let $\Pr(H)$ be the degree of belief that a rational agent a assigns to H at some time t (call this a's "prior" for H).
- Let $\Pr(H \mid E)$ be the degree of belief that a would assign to H (just after t) were a to learn E at t (a's "posterior" for H).
- Toy Example: Let H be the proposition that a card sampled from some deck is a ♠, and E assert that the card is black.
- Making the standard assumptions about sampling from 52-card decks, $\Pr(H) = \frac{1}{4}$ and $\Pr(H \mid E) = \frac{1}{2}$. So, (learning that) E (or supposing that E) *raises the probability of* H.
- Following Popper [13], Bayesians define confirmation in a way that is *formally* very similar to Carnap's (2)-explication.
- For Bayesians, E confirms H for an agent a at a time t iff $\Pr(H \mid E) > \Pr(H)$, where Pr captures a's credences at t.
- While this is *formally* very similar to Carnap's (2), it does not assume that there are objective, "logical" probabilities.

- There are *many logically equivalent* (but *syntactically* distinct) ways of saying E confirms H, in the Bayesian sense.
- Here are the three most common ways:
  - E confirms H iff $\Pr(H \mid E) > \Pr(H)$. $[\frac{1}{2} > \frac{1}{4}]$
  - E confirms H iff $\Pr(E \mid H) > \Pr(E \mid {\sim}H)$. $[1 > \frac{1}{3}]$
  - E confirms H iff $\Pr(H \mid E) > \Pr(H \mid {\sim}E)$. $[\frac{1}{2} > 0]$
- By taking differences or ratios of the left/right sides of such inequalities, various confirmation *measures* $\mathfrak{c}(H,E)$ emerge.
- A plethora of such confirmation measures have been used in the literature of Bayesian confirmation theory. See my thesis [5] for a survey. Here are the four most popular $\mathfrak{c}$'s:
  - $d(H,E) \overset{\text{def}}{=} \Pr(H \mid E) - \Pr(H)$
  - $r(H,E) \overset{\text{def}}{=} \log\left[\frac{\Pr(H \mid E)}{\Pr(H)}\right] \doteq \frac{\Pr(H \mid E) - \Pr(H)}{\Pr(H \mid E) + \Pr(H)}$
  - $l(H,E) \overset{\text{def}}{=} \log\left[\frac{\Pr(E \mid H)}{\Pr(E \mid {\sim}H)}\right] \doteq \frac{\Pr(E \mid H) - \Pr(E \mid {\sim}H)}{\Pr(E \mid H) + \Pr(E \mid {\sim}H)}$
  - $s(H,E) \overset{\text{def}}{=} \Pr(H \mid E) - \Pr(H \mid {\sim}E)$

- Question: do these (and other) measures disagree only *conventionally*, or do they disagree in substantive ways?
- Note: mere *numerical* differences between measures are not important, since they need not affect *ordinal* judgments of what is more/less well confirmed than what (by what).
- If two measures $\mathfrak{c}_1$ and $\mathfrak{c}_2$ agree on *all comparisons*, then we say that $\mathfrak{c}_1$ and $\mathfrak{c}_2$ are *ordinally equivalent* ($\mathfrak{c}_1 \doteq \mathfrak{c}_2$). That is:

$$\mathfrak{c}_1 \doteq \mathfrak{c}_2 \overset{\text{def}}{=} \mathfrak{c}_1(H,E) \geq \mathfrak{c}_1(H',E') \text{ iff } \mathfrak{c}_2(H,E) \geq \mathfrak{c}_2(H',E')$$

- **Fact.** *No two* of $\{d, r, l, s\}$ are ordinally equivalent.
- OK, but do they disagree on *important* applications or in *important* cases? Unfortunately, they disagree *radically*.
- **Fact.** *Almost every* argument/application in the literature is valid for *only some* choices of d, r, l, s. I have called this *the problem of measure sensitivity*. See my [5] for a survey.
- We need some *normative principles* to narrow the field …

- Consider the following two propositions concerning a card c, drawn at random from a standard deck of playing cards:

  E: c is the ace of spades. H: c is *some* spade.

- I take it as intuitively clear and uncontroversial that:
  - The degree to which E confirms H $\neq$ the degree to which H confirms E, since $E \vDash H$, but $H \nvDash E$. $[\mathfrak{c}(H,E) \neq \mathfrak{c}(E,H)]$
  - The degree to which E confirms H $\neq$ the degree to which ${\sim}E$ *dis*confirms H, since $E \vDash H$, ${\sim}E \nvDash {\sim}H$. $[\mathfrak{c}(H,E) \neq -\mathfrak{c}(H,{\sim}E)]$
- Therefore, *no adequate measure of confirmation* $\mathfrak{c}$ *should be such that either* $\mathfrak{c}(H,E) = \mathfrak{c}(E,H)$ *or* $\mathfrak{c}(H,E) = -\mathfrak{c}(H,{\sim}E)$ for all E and H and for all probability functions Pr. I'll call these two symmetry desiderata S_1 and S_2, respectively.
- Note: for all H, E, and for all Pr, $r(H,E) = r(E,H)$ and $s(H,E) = -s(H,{\sim}E)$. That is, r violates S_1 and s violates S_2.
- *Both* d and l satisfy these S-desiderata. This narrows the field to d and l [4]. We can narrow the field further still …

- If we think of inductive logic as a *quantitative generalization* of deductive logic, then the following *logical* desideratum seems natural (it's also implicit in the previous example):
  - (†) **Quantitative Rendition.** $\mathfrak{c}(H,E)$ should be *maximal* when $E \vDash H$ and $\mathfrak{c}(H,E)$ should be *minimal* when $E \vDash {\sim}H$.
  - (†) **Comparative Rendition.** If $E \vDash H$ but $E' \nvDash H'$, then the following inequality should hold: $\mathfrak{c}(H,E) \geq \mathfrak{c}(H',E')$.
- The measure d violates these desiderata. For, when $E \vDash H$:
  $$d(H,E) = \Pr(H \mid E) - \Pr(H) = 1 - \Pr(H) = \Pr({\sim}H)$$
- So, if the prior probability of H is sufficiently high, then (according to d) E will confirm H *very weakly*, *even if* $E \vDash H$.
- From an inductive-logical point of view, this is absurd, since *the logical strength of a valid argument should not depend on how probable its conclusion is* (or on its truth-value).
- Indeed, of all the Bayesian measures of confirmation that have been used in the literature, only l (or its ordinal equivalents) satisfy all three of our desiderata: S_1, S_2, (†).

- Kahneman and Tversky [9] amassed lots of data, which they claimed indicated *violations* of normative principles for probability judgments (*i.e.*, violations of the Pr-axioms).
- If Carnap was confused (along with many others) about the probability/confirmation distinction, could this confusion also underlie some of these erroneous Pr judgments?
- Two examples from K&T come to mind. First, their experiments on the neglect of "base rate" information.
- When people are asked to assess the probability that John has AIDS, given that he tested positive for AIDS according to a very reliable test protocol, they often report high values.
- This *seems* to violate Bayes's Theorem, since AIDS has such a low base rate (prior?) in the population (and they know this). This does *seem* to be a poor probability judgment [10].
- But, could this also reflect a *good* underlying *confirmation* or *evidential support* judgment? Note: $l(H,E)$ is very close to the value reported by experts in these examples [7].

- A second example from K&T that's worth thinking about in this connection is the so-called "conjunction fallacy".
  - (E) Linda is 31, single, outspoken and very bright. She majored in philosophy. As a student, she was deeply concerned with issues of discrimination and social justice and she also participated in antinuclear demonstrations.
- Is it more probable, given E, that Linda is (H_1) a bank teller, or ($H_1 \,\&\, H_2$) a bank teller & active in the feminist movement?
- Most people answer that $H_1 \,\&\, H_2$ is more probable (given E) than H_1 is. This violates Pr-theory, since $H_1 \,\&\, H_2 \vDash H_1$.
- Note: it *is* possible to have $l(H_1 \,\&\, H_2, E) > l(H_1, E)$. Thus, E *could* constitute *better evidence for* $H_1 \,\&\, H_2$ than for H_1 [11]. Indeed, we [3] have recently proven the following:

  Theorem*. For almost all confirmation measures* $\mathfrak{c}$*, if*

  (i) $\mathfrak{c}(H_2, E \mid H_1) > 0$ *and*

  (ii) $\mathfrak{c}(H_1, E) \leq 0$,

  then $\mathfrak{c}(H_1 \,\&\, H_2, E) > \mathfrak{c}(H_1, E)$.
- And, conditions (i) and (ii) do seem to hold in the Linda case.

- Interestingly, until recently there have been almost no psychological studies on how people *actually* make confirmation judgments (in the present, Bayesian sense).
- This was surprising to me, mainly for the following reasons:
  - Because of the long-standing confusion about probability *vs* confirmation in the philosophical literature, I thought that this should be a ripe area for psychological research.
  - I've suspected that confirmation judgments should be more robust than Pr-judgments, since they are (normatively!) less sensitive to subjective factors (in particular, "priors" [6]).
- I am happy to report that this now seems to be evolving into a ripe area for psychological research. Dan Osherson and his colleagues are largely responsible for this change.
- One thing we'd like to know is whether people tend to make *quantitative* judgments of confirmational strength that accord with normatively adequate measures like l.
- A recent study [12] was designed to answer this question . . .

- As far as I know, the forthcoming study by Osherson et al [12] is the first designed explicitly to test Bayesian measures of confirmation against each other for descriptive accuracy.
- Their study involved 24 undergraduates (U. of Trento). They were (individually) faced with the following scenario.
  - They were shown two opaque urns (A, B), where A contains 30/10 black/white balls, and B contains 15/25 B/W balls.
  - A fair coin was tossed, and an urn selected at random. Then, 10 balls were drawn (at random) without replacement.
  - After each draw, they were asked to rank the *evidential impact* of that draw on the hypotheses (a) that A was chosen, and (b) that B was chosen, on a scale with 7 "ticks".
  - Tick 1: "weakens my conviction extremely", tick 7: "strengthens my conviction extremely". Tick 4: "no effect".
  - Then, the subject was asked to estimate *probabilities* $\Pr(A \mid E)$ and $\Pr(B \mid E)$ and *likelihoods* $\Pr(E \mid A)$ and $\Pr(E \mid B)$.
  - Finally, these subjective estimates of probabilities and likelihoods were plugged-in to the various measures of confirmation. And, correlation statistics were calculated.

- The experimenters also plugged-in *objective* probabilities and likelihoods, to see what predictions *those* yielded.
- The results were (to me) somewhat (pleasantly!) surprising:
  - Of the measures d, r, and l, the measure l was significantly better at predicting confirmation judgments, both using the subjective and the objective probabilities and likelihoods.
  - Note: their protocol was unable to test the accuracy of s.
  - Several additional measures from the literature were tested, and l was significantly better than *all* of the other measures, when *objective* probabilities/likelihoods were used.
  - l was not significantly worse than any other measure tested, when *subjective* probabilities/likelihoods were used.
  - The posterior probabilities (either objective or subjective) were *very poor* predictors. This indicates that the subjects distinguished confirmation & probability [Carnap's (1) & (2)].
- This (plus subj $\neq$ obj) confirms what I have long suspected: people are better at making confirmation judgments than probability judgments. Of course, more studies are needed.
- Now, for some research suggestions from the armchair ...

- First, I would suggest looking at *comparative/relational* confirmation judgments, rather than *quantitative* ones. I suspect these will be even more robust and objective [6].
- Second, I would suggest controlling for certain other pragmatic factors that may confound (or create) differences between measures. Jim Joyce has discussed such factors [8].
- Third, the protocol of Osherson *et al* was unable to test the descriptive accuracy of the measure s. It would be nice to generalize their protocol to include s (and others like it).
- Finally, I would also like to see some experiments designed *explicitly* to distinguish *qualitative* confirmation judgments from probability-threshold judgments [Carnapian (1) *vs* (2)].
- I suspect that people's judgments about "what confirms what" come apart *sharply* from their judgments of what is "probable". But, it would be nice to have more data on this.
- *E.g.*: I bet jurors who learn their (guilty) verdict was false will retract "probable" claims, *not* "supported-by-E" claims.

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