The Wason Task(s) & The Paradox of Confirmation

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- **Nicod Condition** (NC): For any object x and any properties ϕ and ψ, the proposition that x is both ϕ and ψ confirms the proposition that every ϕ is ψ. For instance,

$$\sim Ba \,\&\, \sim Ra \text{ confirms } (\forall x)(\sim Bx \supset \sim Rx).$$

- **Equivalence Condition** (EC): For any propositions H_1, E, and H_2, if E confirms H_1 and H_1 is (*classically*) logically equivalent to H_2, then E confirms H_2. For instance,

$$E \text{ confirms } (\forall x)(\sim Bx \supset \sim Rx) \Rightarrow E \text{ confirms } (\forall x)(Rx \supset Bx).$$

- **Paradoxical Conclusion** (PC): The proposition that a is both nonblack and a nonraven confirms the proposition that every raven is black. That is, for arbitrary individual a:

$$\sim Ba \,\&\, \sim Ra \text{ confirms } (\forall x)(Rx \supset Bx).$$

Proof. (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) By (1), (2), (EC), $\sim Ba \,\&\, \sim Ra$ confirms $(\forall x)(Rx \supset Bx)$.

Hempel [8] *&* Goodman [7] *embraced* (NC), (EC) *and* (PC). They saw **no paradox**. They *explain away* the paradoxical *appearance*:

> . . . 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.

Idea: E [$\sim Ra \,\&\, \sim Ba$] confirms H [$(\forall x)(Rx \supset Bx)$] *relative to* $\top$, but E doesn't confirm H *relative to some background* $K \neq \top$.

Question: *Which* $K \neq \top$? Answer: $K = \sim Ra$. Idea: If you already know that $\sim Ra$, then observing a's color won't tell you anything about the color of ravens. Distinguish the following two claims:

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

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

Intuition ($\mathcal{I}$). (PC) is true, but (PC*) is false. [*Why?* $\sim Ra$ reduces the size of the set of possible *counterexamples* to $(\forall x)(Rx \supset Bx)$ [12].]

Nice idea! Sadly, ($\mathcal{I}$) is *inconsistent* with their confirmation *theory*!

Specifically, intuition ($\mathcal{I}$) contradicts (evidential) *monotonicity*:

(M) E confirms H relative to $\top \Rightarrow E$ confirms H relative to *any* K.

☞ Hempel's *theory entails* (M) [4]. Good intuition [($\mathcal{I}$)], bad theory.

Unlike Hempel, Bayesians (*e.g.*, Carnap [1]) use *probabilistic relevance* relations to explicate the confirmation relation.

This has several advantages over Hempel's *de*ductive approach:

1. It leads to a *non*-monotonic confirmation relation, which can accommodate Hempelian *anti*-(M) *intuitions* like ($\mathcal{I}$).
2. It gives rise to a confirmation relation which does *not* imply (NC). [See "Extras" and [13] for examples and discussion.]
3. It supplies *comparative* (and quantitative) $\mathfrak{c}$-relations:
   - E_1 confirms H *more strongly than* E_2 does — relative to background corpus K — iff $\Pr(H \mid E_1 \,\&\, K) > \Pr(H \mid E_2 \,\&\, K)$. [$\mathfrak{c}(H, E \mid K) \overset{\text{def}}{=}$ the *degree* to which E confirms H (rel. to K).]

Next, a brief review of the canonical comparative Bayesian response(s) to The Paradox. Then, it's on to Wason's Task(s).

There have been *many* comparative Bayesian approaches to the paradox (see [19]). Here is a canonical characterization:

Assume that our *actual* background corpus K_α is such that:

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

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

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

Theorem. Any Pr satisfying (3), (4) and (5) will also be such that:

($\mathscr{B}$) $\Pr(H \mid Ra \,\&\, Ba \,\&\, K_\alpha) > \Pr(H \mid \sim Ba \,\&\, \sim Ra \,\&\, K_\alpha)$.

$\therefore$ the proposition that a is a black raven (*actually*) confirms that all ravens are black *more strongly than* the proposition that a is a nonblack nonraven, *if* (3)–(5) hold for (*actual*) K_α.

(3) is rather plausible (and it's uncontroversial in the literature).

(4) and (5) are problematic. I'll say more about them below. For now, just note that Hempel, Carnap, *et al.* would reject them.

Moreover, (3)–(5) are quite strong. They entail *far more than* ($\mathscr{B}$).

Assumptions (3)–(5) *also* entail the following *qualitative* claims:

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

(7)/(PC) $\Pr(H \mid \sim Ba \,\&\, \sim Ra \,\&\, K_\alpha) > \Pr(H \mid K_\alpha)$

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

(9)/(PC*) $\Pr(H \mid \sim Ba \,\&\, \sim Ra \,\&\, K_\alpha) > \Pr(H \mid \sim Ra \,\&\, K_\alpha)$

Hempel's *theory* agrees with (6) & (7), but not (8). And, Hempel's *intuitive* response is to *accept* (PC) [(7)] while *denying* (PC*) [(9)].

These consequences of (3)–(5) are undesirable for two reasons:

- According to *many* commentators on the paradox (both Hempelians and non-Hempelians — see [19], [12] for discussion), *even if* (6) and (7) are plausible, (8) & (9) *aren't*.
- They preclude (3)–(5) from grounding a *purely comparative* approach [*i.e.*, one that's *neutral* on the truth of (6)–(9)].

It would be nice to have a *purely comparative* approach — one which does not *force* the Bayesian to accept *any* of (6)–(9)...

The problematic assumptions are the *independencies*: (4) & (5). Vranas [19] discusses (4) & (5), and their standard rationales.

Comparatively, (4) & (5) can be replaced by the *strictly weaker*:

(‡) $\Pr(H \mid Ra \,\&\, K_\alpha) \gtrapprox \Pr(H \mid \sim Ba \,\&\, K_\alpha)$

☞ (3) & (‡) jointly entail ($\mathscr{B}$) — no independencies required [4].

(‡) says: Ra confirms H *to* $\gtrapprox$ *the same degree* as $\sim Ba$ does. This assumption is far more plausible than the independencies (4) & (5). None of the standard arguments against (4)/(5) apply to (‡).

Moreover, accepting (3) & (‡) is consistent with denying (or accepting) all four of the qualitative claims (6), (7), (8) and/or (9).

Thus, a more plausible, *purely comparative* approach *is* possible.

Hempel's own *intuitive* line on the paradox favors (3) & (‡), which is compatible with *accepting* (PC) while *denying* (PC*).[1]

[1] *Carnapian* $\mathfrak{c}$-theory is also compatible with (PC) & ¬(PC*) [12, 13].

- Wason gives various versions of his "task(s)". *E.g.*,

  Given the sentence: Every card which has a D on one side has a 3 on the other side (and knowledge that each card has a letter on one side and a number on the other), together with four cards showing D, K, 3, 7, hardly any individuals make the correct choice of cards to turn over (D, 7) in order to determine the truth of the sentence. [20, p. 63]

- This characterization is unclear. Here is a precisification:

  Each card (in some set of cards **C**) has one letter on one side and one number on the other side. You will be shown four cards from **C** (with one face down), and you will be asked to turn over one or more of the four cards, with an eye toward determining whether the following hypothesis is true:

  (H) All "D"-cards (in **C**) are "3"-cards.

  Q: Which of the following 4 cards would you turn to test H?

  [D] [K] [3] [7]

  - Empirically, the most frequent answers are (in decreasing order of f): (*i*) [D] [3], (*ii*) [D], (*iii*) [D] [3] [7], (*iv*) [D] [7].
  - For *single-card* strategies, the ordering is: [D] $\succ$ [3] $\succ$ [7].

- Humberstone [10] was the first to draw an explicit analogy between Wason's task(s) and the Paradox of Confirmation.
- Unfortunately, Humberstone seems not to have been read by the cognitive scientists who (later) exploit the analogy.
- Humberstone's key insight is that Wason's original description—which *leaves out* **C**—was *ambiguous*.
- Wason presupposes (without telling his subjects!) that **C** just *is* the set of four cards you are shown. Two readings:
  - (I) $\mathbf{C} = \boxed{\mathrm{D}}\,\boxed{\mathrm{K}}\,\boxed{3}\,\boxed{7}$. This is the reading Wason presupposes.
  - (II) $\boxed{\mathrm{D}}\,\boxed{\mathrm{K}}\,\boxed{3}\,\boxed{7} \subset \mathbf{C}$. This is Humberstone's alternative.
- As Wason notes, (I) implies that there is a *single, definitive, correct answer* to the Question: $\boxed{\mathrm{D}}\,\boxed{7}$ — choice (*iv*)!
- Wason must have had (I) in mind, since he talks as if $\boxed{\mathrm{D}}\,\boxed{7}$ is ***the*** answer. But, (II) is consistent with his descriptions.
- (II) leads to an analogy with the Paradox of Confirmation.
- Humberstone sketches this analogy, in a *Hempelian* way.

- Humberstone omits two key *dis*analogies with Hempel:
  - (i) Subjects may turn over *multiple* cards, not just one.
  - (ii) Subjects *already know* a property of the sampled object(s).
- To shore-up (i), we'll focus on *single-card* strategies.
- We'll address (ii) by using *two-stage* Bayesian sampling.
- Here's a "Hempelian Task" that's most analogous to Wason:

> For each object in the world, we create a card, which has "R"/"$\sim R$" on one side (depending on whether the object is/is not a raven) and "B"/"$\sim B$" on the other side (depending on whether it is/is not black). Then, we shuffle the resulting deck **C**, and deal these 4 cards from it:
>
> $$\boxed{R}\,\boxed{\sim R}\,\boxed{B}\,\boxed{\sim B}$$
>
> Consider the following hypothesis about the cards in **C**:
>
> (H) All "R"-cards (in **C**) are "B"-cards. (*i.e.*, all ravens are black.)
>
> Which of the 4 cards would you turn, in order to test hypothesis H?[2]

[2] We can word this in various ways — including ways of asking which strategies generate the "best test" of H, *etc.* — without affecting results.

- The analogous empirical ordering is: (*i*) $\boxed{\mathrm{R}}\,\boxed{\mathrm{B}}$, (*ii*) $\boxed{\mathrm{R}}$, (*iii*) $\boxed{\mathrm{R}}\,\boxed{\mathrm{B}}\,\boxed{\sim\mathrm{B}}$, (*iv*) $\boxed{\mathrm{R}}\,\boxed{\sim\mathrm{B}}$ [for *single-cards*: ($\mathbb{O}$) $\boxed{\mathrm{R}} \succ \boxed{\mathrm{B}} \succ \boxed{\sim\mathrm{B}}$].
- If **C** were *identical* to $\boxed{\mathrm{R}}\,\boxed{\sim\mathrm{R}}\,\boxed{\mathrm{B}}\,\boxed{\sim\mathrm{B}}$, then $\boxed{\mathrm{R}}\,\boxed{\sim\mathrm{B}}$ [(*iv*)] would be ***the*** correct answer. But, now, $\boxed{\mathrm{R}}\,\boxed{\sim\mathrm{R}}\,\boxed{\mathrm{B}}\,\boxed{\sim\mathrm{B}} \subset \mathbf{C}$.
- Here, The Task is *similar* to The Paradox, on a *two-stage sampling* model [18]. Consider these *single-card strategies*:
  - $\boxed{\mathrm{R}}$ Sampling an object a from the class of ravens and then checking to see whether a is black.
  - $\boxed{\mathrm{B}}$ Sampling an object a from the class of black things and checking to see whether a is a raven.
  - $\boxed{\sim\mathrm{B}}$ Sampling an object a from the class of non-black things and checking to see whether a is a raven.
- The Bayesians' (3)–(5) imply [($\mathscr{B}$)] that $\boxed{\mathrm{R}}$ generates better evidence than $\boxed{\sim\mathrm{B}}$ — *if* both yield *confirmatory* evidence.
- But, since $\mathfrak{c}(H, E \mid K) \neq \mathfrak{c}(H, \sim E \mid K)$ [3], this doesn't explain why $\boxed{\mathrm{R}}$ should generate better evidence "on average".
- ☞ Moreover, (3)–(5) *don't speak to* $\boxed{\mathrm{B}}$'s place in the ordering.

- Nickerson's (two-stage) approach involves the adoption of the following simple measure of "confirmational power":

$$\bar{d}(H, E \mid K) \stackrel{\text{def}}{=} |\Pr(H \mid E \,\&\, K) - \Pr(H \mid K)|$$

- Nickerson uses $\bar{d}$ to define the "*expected* confirmational power" [$\mathcal{P}(S)$] of an evidence-gathering strategy (S).
- The three salient (and traditional, Bayesian decision theoretic) definitions are as follows (suppressing K):

$$\mathcal{P}(\boxed{\mathrm{R}}) \stackrel{\text{def}}{=} \Pr(Ba \mid Ra) \cdot \bar{d}(H', Ba \mid Ra) + \Pr(\sim Ba \mid Ra) \cdot \bar{d}(H', \sim Ba \mid Ra).$$

$$\mathcal{P}(\boxed{\mathrm{B}}) \stackrel{\text{def}}{=} \Pr(Ra \mid Ba) \cdot \bar{d}(H', Ra \mid Ba) + \Pr(\sim Ra \mid Ba) \cdot \bar{d}(H', \sim Ra \mid Ba).$$

$$\mathcal{P}(\boxed{\sim\mathrm{B}}) \stackrel{\text{def}}{=} \Pr(Ra \mid \sim Ba) \cdot \bar{d}(H', Ra \mid \sim Ba) + \Pr(\sim Ra \mid \sim Ba) \cdot \bar{d}(H', \sim Ra \mid \sim Ba).$$

- He then writes down a *numerical* Pr-function, which *entails both* the standard Bayesian assumptions (3)–(5), *and*:
  - ($\mathscr{N}$) $\mathcal{P}\left(\boxed{R}\right) > \mathcal{P}\left(\boxed{B}\right) > \mathcal{P}\left(\boxed{\sim B}\right)$. [Note that ($\mathscr{N}$) *matches* ($\mathbb{O}$).]
- Nickerson's ($\mathscr{N}$) is *not entailed by* (3)–(5), and he doesn't identify (general) conditions for his desired $\mathcal{P}$-ordering ($\mathscr{N}$).

- Here are four important (general) new results about Nickersonian models (now joint work with Jim Hawthorne):

① (3)–(5) are *not* sufficient for ($\mathcal{N}$), but (3′)–(5) *are*, where:

(3′) $\Pr(\sim Ba) > \Pr(Ba) > \Pr(Ra)$.

② (3′) + (‡) is *not* sufficient for ($\mathcal{N}$), but (3′) + (‡) *does* entail:

$$\mathcal{P}\left(\boxed{R}\right) > \mathcal{P}\left(\boxed{\sim B}\right)$$

which is the *un*controversial (“Paradox”) fragment of ($\mathcal{N}$).

③ Assuming *only* that $\bar{\mathfrak{c}}(H, Ra \mid \sim Ba) > \bar{\mathfrak{c}}(H, Ra \mid Ba)$, the following is a *necessary* condition for Nickerson’s ($\mathcal{N}$):

$$\Pr(Ra \mid Ba) > \Pr(Ra \mid \sim Ba).$$

☞ *confirmation bias — viz.,* $\boxed{\sim B}$*-refutation is less probable than* $\boxed{B}$*-confirmation* — is a *necessary condition* for ($\mathcal{N}$)!

④ If we assume a *Carnapian* [13]/Nickersonian [15] definition of “expected confirmational power, relative to tautological background corpus” $\mathcal{P}_\top(\cdot)$, then we *must* have:

($\mathscr{W}$) $\mathcal{P}_\top\left(\boxed{\sim B}\right) > \mathcal{P}_\top\left(\boxed{B}\right)$. [*i.e.*, Carnap + Nickerson $\Longrightarrow$ Wason!]

[1] R. Carnap, *Logical Foundations of Probability*, 2nd *ed.*, Chicago U. Press, 1962.
[2] L. Cosmides, *The logic of social exchange: Has natural selection shaped how humans reason? Studies with the Wason selection task*, *Cognition* **31** (1985), 187-276.
[3] E. Eells and B. Fitelson, *Symmetries and Asymmetries in Evidential Support*, *Philosophical Studies*, **107** (2002), 129–142.
[4] B. Fitelson and J. Hawthorne, *How Bayesian confirmation theory handles the paradox of the ravens*, *Probability in Science* (Eells and Fetzer, *eds.*), to appear (2009).
[5] I.J. Good, *The white shoe is a red herring*, *British J. for the Phil. of Sci.* **17** (1967), 322.
[6] ______, *The white shoe qua red herring is pink*, *British J. for the Phil. of Sci.*, **19** (1968), 156–7.
[7] N. Goodman, *Fact, Fiction, and Forecast*, Harvard University Press, 1955.
[8] C. Hempel, *Studies in the logic of confirmation*, *Mind* **54** (1945), 1–26, 97–121.
[9] ______, *The white shoe: no red herring*, *British J. for the Phil. of Sci.* **18** (1967), 239–240.
[10] Humberstone, L., *Hempel Meets Wason*, *Erkenntnis* **41** (1994), 391–402.
[11] J. MacFarlane and N. Kolodny, *Ifs and Oughts*, unpublished manuscript (2008).
[12] P. Maher, *Inductive logic and the ravens paradox*, *Philosophy of Science* **66** (1999), 50–70.
[13] ______, *Probability captures the logic of scientific confirmation*, *Contemporary Debates in the Philosophy of Science* (Christopher Hitchcock, ed.), Blackwell, 2004.
[14] C. McKenzie and L. Mikkelsen, *The Psychological Side of Hempel’s Paradox of Confirmation*, *Psychonomic Bulletin & Review* **7** (2000), 360–66.
[15] Nickerson, R. *Hempel’s Paradox and Wason’s Selection Task: Logical and Psychological Puzzles of Confirmation*, *Thinking and Reasoning* **2** (1996), 1–31.
[16] Oaksford, M. and Chater N. *A Rational Analysis of the Selection Task as Optimal Data Selection*, *Psychological Review* **101** (1994), 608–631.
[17] W.V.O. Quine, *Natural kinds*, *Ontological Relativity and Other Essays*, Columbia U. Press, 1969.
[18] Royall, R. *Statistical Evidence: A Likelihood Paradigm*, CRC Press, 1999.
[19] P. Vranas, *Hempel’s raven paradox: a lacuna in the standard Bayesian solution*, *British Journal for the Philosophy of Science* **55** (2004), 545–560.
[20] Wason, P. and D. Shapiro. *Natural and Contrived Experience in a Reasoning Problem*, *Quarterly Journal of Experimental Psychology* **23** (1971), 63–71.

I.J. Good [5] gave the following Bayesian counterexample to (NC):

> 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 [*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 universe). Then:
>
> $$\Pr(E \mid H \,\&\, K) = \frac{100}{1000100} \ll \frac{1000}{1001001} = \Pr(E \mid \sim H \,\&\, K)$$
>
> $\therefore$ E lowers the probability of (disconfirms) H, relative to K.

$\therefore$ (NC) is false, and *even for “natural kinds”* (*pace* Quine [17]). Similar examples can be used to show that (PC) is also false.

Hempel [9] complains that Good’s example is not probative, since (NC) must be taken relative to *empty background* $K = \top$.

Is this a fair complaint? [No — (M)!] Anyhow, Good responds …

Here’s Good’s [6] attempt to meet Hempel’s $K = \top$ Challenge:

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.

Even Good wasn’t confident about this $K = \top$ counterexample. Maher [12] argues this is not a compelling counterexample.

Maher [13] has recently provided a more compelling (*Carnapian*) counterexample to (NC), which is beyond our scope today.[3]

Most Bayesians don’t *understand* ($NC_{K=\top}$). Unlike Carnap [1], they have *no theory* of “$\Pr_\top$” [or “$\mathfrak{C}(H, E \mid \top)$”]. So, they opt for a different sort of approach, using *epistemic* Pr and *actual* $K = K_\alpha$.

[3] Maher [13] shows that $\Pr_\top(H \mid E) < \Pr_\top(H)$, for some adequate Carnapian $\Pr_\top$ functions. Hence, (NC) is false for a Carnapian theory of “$\mathfrak{C}(H, E \mid \top)$”.

- **Bayesian counterexample(s) to (M).** Let $Bx \overset{\text{def}}{=} x$ is a black card, $Ax \overset{\text{def}}{=} x$ is the A♠, $Jx \overset{\text{def}}{=} x$ is the J♣, and $K \overset{\text{def}}{=}$ a card a is sampled at random from a standard deck:
  - Ja conjoined to foreground evidence:
    - $\Pr(Aa \mid Ba \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
    - $\Pr(Aa \mid Ba \,\&\, Ja \,\&\, K) = 0 < \frac{1}{52} = \Pr(Aa \mid K)$.
  - Ja conjoined to background evidence:
    - $\Pr(Aa \mid Ba \,\&\, K) = \frac{1}{26} > \frac{1}{52} = \Pr(Aa \mid K)$.
    - $\Pr(Aa \mid Ba \,\&\, Ja \,\&\, K) = 0 = \Pr(Aa \mid Ja \,\&\, K)$.

- **More on our assumption (‡).** Recall:

  (‡) $\Pr(H \mid Ra \,\&\, K_\alpha) \gtrapprox \Pr(H \mid {\sim}Ba \,\&\, K_\alpha)$

- It is helpful to note that the following *alternative* suffices:

  $$(\star) \quad \frac{\Pr({\sim}Ba \mid K_\alpha)}{\Pr(Ra \mid K_\alpha)} \gtrapprox \frac{\Pr({\sim}Ba \mid H \,\&\, K_\alpha)}{\Pr(Ra \mid H \,\&\, K_\alpha)}$$

- In words, $(\star)$ says that *learning H doesn't vastly increase one's estimate of the ratio of non-black objects to ravens.*
- Note: the closensess of $\approx$ required (in the $\gtrapprox$) for (‡)/$(\star)$ depends on how large the inequality $\Pr({\sim}Ba) > \Pr(Ra)$ is.

McKenzie & Mikkelsen (M&M) [14] report Ψ-experiments involving a variety of "Hempel-like" hypothesis-testing problems.

Their data show that changes in the "rarity assumption" [(3′)] are correlated with changes in agents' responses as to the degree to which (E_2) ${\sim}Xa \,\&\, {\sim}Ya$ is comparatively probative [*vs* (E_1) $Xa \,\&\, Ya$], concerning (H) All X's are Y's (for *many* X's and Y's).

Three comments on the models of M&M:

- Like Nickerson & typical Bayesians, M&M assume (4) & (5).
- M&M *try* to draw the Hempel/Wason analogy, but they seem insensitive to the fact that explaining the *Wason* data requires explaining $\boxed{\text{R}} \succ \boxed{\text{B}} \succ \boxed{{\sim}\text{B}}$, and *not merely* $\boxed{\text{R}} \succ \boxed{{\sim}\text{B}}$.
- Unlike Hempel/Bayesians who assume agents test H against ${\sim}H$, M&M suppose that agents test H against (a "null") H' asserting that $X \perp\!\!\!\perp Y$. This is a "Likelihoodist" approach [18].
- Our result ④ shows that M&M must *reject* Nickerson's ordering $(\mathcal{N})$ — on pain of *a priori rejection* of their "null"!

- Oaksford & Chater [16] give yet another "rationalization" (Bayesian-style) of the responses to Wason's Task(s).
- In some respects, their approach is similar to that of M & M:
  - O & C do not test H' against ${\sim}H'$ (or, "in isolation", as they put it). Rather, they test H' against H'', which is the hypothesis that R and B are *probabilistically independent*.
  - Like M&M, this is more of a "Likelihoodist" [18] approach.
- In other respects, O&C's approach is similar to Nickerson's:
  - O & C define their "$\mathcal{P}(\cdot)$" in terms of *expected information gain* (expected *entropy decrease*), which is more similar (than M&M) to Nickerson's *expected degree of confirmation.*
- In still other respects, their approach is dissimilar to all other Bayesian approaches, as they do *not* assume *independencies* (4)/(5), or even our [4] weaker (‡).
  - They (now) seem to think their account is more similar to Nickerson's. I need to examine their models more closely before rendering an opinion. But, if they are like Nickerson, they will inherit some of his problems (explained above).

Cosmides [2] reports "Wason-like" experiments in which agents seem to do "better" — *assuming Wason's normative model.*

Her examples involve conditionals with deontic and/or modal content in their consequents. *E.g.*, she asks subjects to test:

If a person is drinking beer (D), then he must be over 20 years old (O).

by turning one or more of these 4 cards [where one side has a person's drinking behavior $D/{\sim}D$ and the other has their age $O/{\sim}O$]:

$$\boxed{\text{D}}\ \boxed{{\sim}\text{D}}\ \boxed{\text{O}}\ \boxed{{\sim}\text{O}}$$

The data for Cosmides's "Wason-like" tasks fit Wason's normative $\boxed{\text{D}} \succeq \boxed{{\sim}\text{O}} \succ \boxed{\text{O}}$ much better than Wason's data did.

Cosmides thinks this is "*good* news" for actual subjects, and evidence that evolution has made us "better" at testing certain types of deontically/modally loaded hypotheses/conditionals.

☞ Recent work in the semantics of such conditionals [11] suggests contraposition is *invalid* for them! Is [2] *Grist for Wason's Mill*?