Notes on Accuracy, Coherence and Evidence

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- These lectures are about the relationships between three types of epistemic norms: accuracy, coherence, evidence.
- A simple example that everyone will be familiar with:
  - **The Truth Norm for Belief** (TB). Epistemically rational agents should only believe propositions that are true.
  - **The Consistency Norm for Belief** (CB). Epistemically rational agents should have logically consistent belief sets.
- **Fact.** (TB) entails (CB). Suppose S violates (CB). Then, some of S's beliefs are false. Therefore, S violates (TB). $\square$
- I don't think (TB) is a very interesting "norm". It's of little use in *guiding* our epistemic lives. The fact that (TB) entails (CB) suggests that (CB) isn't a very interesting "norm" either.
- I think (some) *preface cases* are counterexamples to (CB) [and $\therefore$ (TB)], as *norms*. In a preface case, my evidence E will suggest that consistent belief sets *have more incorrect judgments* than my own. And, E ***may not*** *be misleading*!

- Before getting into a new approach to accuracy and coherence (and evidential) norms, I want discuss some historical background regarding full & partial belief.
- Ramsey's watershed essay [22] gives various reasons to be skeptical about substantive epistemic analogies here.
- The main problem involves the following "accuracy analogy"

$$\frac{p \text{ is true}}{B(p)} :: \frac{??}{b(p) = r}$$

- Here, $B(p)$ denotes (epistemically rational) belief that p, and $b(p) = r$ denotes (epistemically rational) credence r that p.
- Ramsey argues that there is no meaningful/epistemically probative way to fill-in the "??" in the above analogy.
- Basically, I agree with Ramsey. In the first part of these lectures, I will review some attempts to fill-in the analogy. Then, I'll explore a different approach (due to Joyce).

- Hájek [9] discusses this analogy in detail. He gives various arguments against certain historical attempts to complete the analogy. I agree with his negative assessments.
- One popular attempt to fill-in the analogy is as follows

$$\frac{p \text{ is true}}{B(p)} :: \frac{\text{"the frequency of } p\text{" is } r}{b(p) = r}$$

- The idea that "credential accuracy" involves matching "the frequencies with which p's are true" has its proponents [26]. Note: (*finite!*) frequencies ***must be*** probabilities.
- Ramsey [22, 174] even *glosses* his pragmatic approach to probabilistic coherence in a "finite frequentist" way:

  > Suppose [S's] degree of belief in p is $\frac{m}{n}$; then [S's] action is such as he would choose it to be if he had to repeat it n times, in m of which p was true, and in the others false.

- There are a great many objections to "calibration to frequency" approaches (see [9]). Here is my own.

- Consider the following simple set of *doxastic indifferences*:
  (1) $Q \sim \neg Q$. [or, if you prefer: $(Q \mid \top) \sim (\neg Q \mid \top)$]
  (2) $(P \& Q) \sim (\neg P \& Q)$. [*viz.*, $(P \& Q \mid \top) \sim (\neg P \& Q \mid \top)$]
  (3) $(Q \mid P) \sim (P \vee Q)$. [*viz.*, $(Q \mid P) \sim (P \vee Q \mid \top)$]
- The *numerical*, probabilistic b-representability of this set of indifference judgments (as well as Ramsey's pragmatic theory of subjective probabilistic indifference) *entails* that:
  (4) $b(P \& \neg Q) = \frac{1}{8}\left(\sqrt{17} - 3\right)$.
- This (coherent!) credence function b *cannot* be "calibrated with respect to *any* finite frequencies" (of *any* kind).
- Moreover, it is difficult to see how we could *operationalize* this credence is Ramsey's terms. "Betting quotients" are normally glossed in terms of ratios of natural numbers.
- "Hypothetical infinite frequencies" would be required. This is deeply problematic for various reasons [9]. Not least of these problems is that HIF's *aren't necessarily probabilities*!

- Hájek proposes that we fill-in the analogy as follows:

$$\frac{p \text{ is true}}{B(p)} :: \frac{\text{“the objective chance of } p\text{” is } r}{b(p) = r}$$

- This avoids my objection, since there is no problem having irrational chances (in fact, these are common in physics).
- But, there are other objections to this approach. Hájek himself discusses the following troubling example:

> the coin that I am about to toss is either two-headed or two-tailed, but you do not know which. What is the probability that it lands heads? Very reasonably, you assign a probability of $\frac{1}{2}$, even though you *know* that the chance of heads is either 1 or 0. So it is rational here to assign a credence that you *know* does *not* match the ... chance.

- This is *dis*analogous to rational belief, since it is never rational to believe something that you *know* is *not* true.
- Anyhow, getting *probabilism* as a coherence norm from this analogy requires arguing that *chances are probabilities* [11].

- To summarize: any attempt to ground probabilism by analogy with the truth norm/consistency norm story for full belief faces a dilemma involving "??". ***Either***
  (a) "??" gets filled-in with something that is *uncontroversially* a *probabilistic structure* (*e.g.*, finite frequencies), ***OR***
  (b) "??" is *controversially* (or just *not*) a *probabilistic structure* (*e.g.*, infinite frequencies, chances, degrees of warrant, *etc.*).
    - I didn't discuss Keynesian [14]/Carnapian [2] ways of filling-in "??". These would involve "partial entailment" or "degrees of warrant/justification". *Even if* such things exist (!), why should we think they (*of necessity*) are *probabilities*?
- If (b), then (at best) an additional (non-trivial) *argument* in M&/∨E will be needed to show that "??" is *probabilistic*.
- If (a), then the analogy faces other (insurmountable) problems — *e.g.*, the fact that sometimes Pr-coherence (of $\succeq$) requires credences that *can't* match finite frequencies.
- ∴ I (like Ramsey) reject the "accuracy analogy" approach. Next: Ramsey's argument for probabilism: The Dutch Book Argument.

- Ramsey's "frequentist gloss" of probabilism is *merely* that. His *official* argument for probabilism is *independent* of any "accuracy analogy". It's a *Dutch Book Argument* (DBA).
- Here's a precise statement of the *modern* DBA:

If an agent S satisfies the following three conditions:

1. S views *individual* bets (β on p) having non-negative "expected payoff" (in a classical sense) within the (monetary) Dutch Book Setup (DBS) as *acceptable*.
2. Condition (1) holds *even if* we replace \$'s with *utiles* in DBS.
3. S views any *collection* of *severally acceptable* bets as *jointly acceptable* [this is called the "package principle"].

then, the following is also true of S:

4. *If (and only if)* S's betting quotients (in a DBS) q are *non*-probabilistic, *then* there exists a sequence of bets (*which S views as both severally and jointly acceptable*) on which S is *guaranteed to lose* (where the loss is in *utiles*).

- Next, I'll go through the modern DBA in some detail.

Here is the Dutch Book Setup (DBS):

- For each proposition $p \in \mathcal{B}$, the agent (Mr. B) must announce a number $q(p)$ — called his *betting quotient* on p — and *then* Ms. A (the "Dutch bookie") will choose *stakes* $\mathfrak{s}$ for all bets.
- $|\mathfrak{s}|$ is small in relation to Mr. B's total wealth [(2): diminishing marginal utility of \$]. $\mathfrak{s}$ can be + or − (Ms. A can "switch sides").

  Mr. B's payoff (in \$) for a bet β regarding $p = \begin{cases} \mathfrak{s} - q(p) \cdot \mathfrak{s} & \text{if } p. \\ -q(p) \cdot \mathfrak{s} & \text{if } \neg p. \end{cases}$
- NOTE: If $\mathfrak{s} > 0$, then the bet is *on* p, if $\mathfrak{s} < 0$, then it's *against* p.
- $q(p)$ is taken to measure Mr. B's *degree of belief* in p (at t).
- Mr. B's *total* payoff for a sequence of multiple bets on multiple propositions is the *sum* of the payoffs for each bet on each proposition. This is "the package principle" (more on it later).

Question: why should a *probabilistically incoherent* agent (with $b = q$) ever enter into a DBS in the first place? Assumption (1)...

- "Acceptability" of an individual bet is (tacitly) assumed to obey:
  - A bet β regarding a proposition p is said to be **acceptable** (to S) iff β has *non-negative* **expected utility** (for S).
- Let $q(\cdot) = b(\cdot)$, $u(\cdot) = S$'s *utility function*, β_p = the outcome of bet β if p is true, and $\beta_{\neg p}$ be the outcome of β if p is false. Then, the *expected utility* (for S) of β is:

  $EU(\beta) \overset{\text{def}}{=} q(p) \cdot u(\beta_p) + q(\neg p) \cdot u(\beta_{\neg p}) = q \cdot u(\beta_p) + \bar{q} \cdot u(\beta_{\neg p})$.
- In the DBS, $u(\beta_p) \overset{\text{def}}{=} \mathfrak{s} - q(p) \cdot \mathfrak{s}$, and $u(\beta_{\neg p}) \overset{\text{def}}{=} -q(p) \cdot \mathfrak{s}$. So:

$$\begin{aligned} EU(\beta) &= q \cdot (\mathfrak{s} - q \cdot \mathfrak{s}) + \bar{q} \cdot (-q \cdot \mathfrak{s}) \\ &= q\mathfrak{s} \cdot (1 - q - \bar{q}) \end{aligned}$$

- Hence, $EU(\beta) \geq 0$ iff $\begin{cases} \mathfrak{s} > 0 \text{ and } \bar{q} \geq 1 - q, \textit{ or} \\ \mathfrak{s} < 0 \text{ and } \bar{q} \leq 1 - q. \end{cases}$
- Now, recall our Question. Why would an agent who has a *non*-probabilistic b ever enter-in to a DBS in the first place? I think they *wouldn't* — *if* their q were such that $\bar{q} \neq 1 - q$!

☞ Assumption (1) *saddles* S (in a DBS) with (some) *probabilism*!

- The DBT has four cases: one for each of the three Pr-axioms, and one for the definition of conditional probability.

(I) **If Mr. B violates Axiom 2, then $\exists$ a DB against him.** Proof:

- If Mr. B assigns $q(\top) = a < 1$, then Ms. A sets $\mathfrak{s} < 0$, and Mr. B's payoff is always $\mathfrak{s} - a\mathfrak{s} < 0$, since $\top$ cannot be false.
- If Mr. B assigns $q(\top) = a > 1$, then Ms. A sets $\mathfrak{s} > 0$, and Mr. B's payoff is always $\mathfrak{s} - a\mathfrak{s} < 0$, since $\top$ cannot be false.
  - NOTE: if $q(\top) = 1$, then Mr. B's payoff is $\mathfrak{s} - \mathfrak{s} = 0$.

(II) **If Mr. B violates Axiom 1, then $\exists$ a DB against him.** Proof:

- If $q(p) = a < 0$, then Ms. A sets $\mathfrak{s} < 0$, and Mr. B's payoff is $\mathfrak{s} - a\mathfrak{s} < 0$ if p, and $-a\mathfrak{s} < 0$ if $\neg p$.
  - NOTE: If $q(p) \geq 0$, then Mr. B's payoff is $\mathfrak{s} - q\mathfrak{s} \geq 0$ if $\mathfrak{s} > 0$ and p is true, and $-q\mathfrak{s} \geq 0$ if $\mathfrak{s} < 0$ and $\neg p$.

- Cases (I) and (II) do *not* require a "package principle", since they involve individual bets (no sequences of multiple bets).
- The NOTEs do *not* aid the *converse* DBT. I won't discuss that.

- Recall, Pr-Axiom 3 (additivity) requires that:

$$\Pr(p \vee r) = \Pr(p) + \Pr(r),$$

  if p and r cannot both be true (*i.e.*, *p, q mutually exclusive*).
- The *additivity* case of DBT is the most controversial. The main source of controversy is the "package principle" (PP). We'll just *assume* the (PP) for the proof of the *Theorem*.
- Next: additivity & conditional probability cases of DBT; then a brief discussion of the Maher/Schick objection to the (PP).

(III) **Setup**: Let p and r be mutually exclusive propositions in $\mathcal{B}$. And, suppose that Mr. B announces the following q's:

- $q(p) = a$.
- $q(r) = b$.
- $q(p \vee r) = c$, where $c \neq a + b$.

- This leaves Mr. B open to a *Dutch Book*. Next: the proof of the additivity case of DBT — note how it presupposes (PP).

(III) **Case 1**: $c < a + b$. Ms. A asks Mr. B to make *three* bets ($|\mathfrak{s}| = \$1$):

(i) Bet $\$a$ *on* p to win $\$(1-a)$ if p, and to lose $\$a$ if $\neg p$.

(ii) Bet $\$b$ *on* r to win $\$(1-b)$ if r, and to lose $\$b$ if $\neg r$.

(iii) Bet $\$(1-c)$ *against* $p \vee r$ to win $\$c$ if $\neg(p \vee r)$, lose $\$(1-c)$ o.w.

- Since p and r are mutually exclusive, the conjunction $p \,\&\, r$ cannot be true. Therefore, we have three possibilities:

Possibility	Payoff on (i)	Payoff on (ii)	Payoff on (iii)	Total Payoff
$p \,\&\, \neg r$	$1-a$	$-b$	$-(1-c)$	$c-(a+b)$
$\neg p \,\&\, r$	$-a$	$1-b$	$-(1-c)$	$c-(a+b)$
$\neg p \,\&\, \neg r$	$-a$	$-b$	c	$c-(a+b)$

- Since $c < a + b$, Mr. B loses $\$[c-(a+b)]$ *come what may*.

(III) **Case 2**: $c > a + b$. Ms. A simply *reverses the bets*, and (once again) Mr. B loses $\$[c-(a+b)]$ *come what may*.

- This presupposes (PP), since it assumes S will view the *package* (i)–(iii) as acceptable. [(PP) holds *if* S is assumed to be an *EU*-maximizer. But, as I said above, *that's question-begging*.]

(IV) We also need to show that an agent's *conditional* betting quotients $q(\cdot \mid \cdot)$ must satisfy the definition of $\Pr(\cdot \mid \cdot)$.

- Suppose Mr. B announces: $q(p \,\&\, r) = b$, $q(r) = c > 0$, and $q(p \mid r) = a$. Ms. A poses the following three bets:

(i) Bet $\$(b \cdot c)$ *on* $p \,\&\, r$: win $\$[(1-b)\cdot c]$ if $p \,\&\, r$; $\$-(b \cdot c)$ o.w. [$\mathfrak{s} = c$]

(ii) Bet $\$[(1-c)\cdot b]$ *against* r: win $\$(b \cdot c)$ if r; $\$-[(1-b)\cdot c]$ o.w. [$\mathfrak{s} = b$]

(iii) Bet $\$[(1-a)\cdot c]$ *against* p, *conditional on* r: win $\$(a \cdot c)$ if $r \,\&\, p$; $\$-[(1-a)\cdot c]$ if $r \,\&\, \neg p$. If $\neg r$, bet is *called off* & payoff is \$0. [$\mathfrak{s} = c$]

Possibility	Payoff on (i)	Payoff on (ii)	Payoff on (iii)	Total Payoff
$p \,\&\, r$	$(1-b)\cdot c$	$-[(1-c)\cdot b]$	$-[(1-a)\cdot c]$	$(a \cdot c) - b$
$\neg p \,\&\, r$	$-(b \cdot c)$	$-[(1-c)\cdot b]$	$a \cdot c$	$(a \cdot c) - b$
$\neg r$	$-(b \cdot c)$	$b \cdot c$	0	0

- If $a < \frac{b}{c}$, then Mr. B loses $\$[(a \cdot c) - b]$ *come what may*. If $a > \frac{b}{c}$, then Ms. A just asks Mr. B to *take the other side of all three bets*.
- Notice how this argument also presupposes (PP). Moreover, it requires that the stakes depend on Mr. B's betting quotients.

- **Theorem.** Any Dutch Book against an agent S whose betting quotients q satisfy the following three constraints *must* involve multiple bets, and (therefore) presuppose the (PP).
  1. $q(\top) = 1$.
  2. $q(\bot) = 0$.
  3. $0 \le q(p) \le 1$, for all $p \in \mathcal{B}$.
- This will become important later, since we will be able to give an (elementary) accuracy-dominance justification for the *comparative* constraints $\top \succeq p$ and $p \succeq \bot$.
- Moreover, it reveals that the (PP) is *essential* to *any* Dutch Book that goes beyond weak "normalization" constraints.
- Some (Maher/Schick) argue that a Pr-incoherent agent can reasonably *reject* a *package* of *severally* acceptable bets.
- I see a deeper problem. DBA smuggles-in a big helping of probabilism — *via* "acceptability" and *expectation*. *At best*: ***if*** $q(\neg p) = 1 - q(p)$, ***then*** q should be ***fully*** probabilistic.

- I agree with Ramsey that the "accuracy analogy" is flawed. But, the DBA is also flawed, and anyhow it is *pragmatic*.
- Here are different analogies (which "go the other way")

$$\frac{\mathfrak{B} \text{ is “\textit{accuracy-dominated}”}}{\mathfrak{B} \text{ is “coherent” (??)}} :: \frac{b \text{ is “\textit{accuracy-dominated}”}}{b \text{ is a Pr-function}}$$

$$\frac{\mathfrak{C} \text{ is “\textit{accuracy-dominated}”}}{\mathfrak{C} \text{ is “coherent” (??)}} :: \frac{\mathfrak{B} \text{ is “\textit{accuracy-dominated}”}}{\mathfrak{B} \text{ is “coherent” (??)}}$$

- Here, $\mathfrak{B}$ is an agent's *qualitative judgment set* — over an entire Boolean algebra of (entertainable) propositions $\mathcal{B}$.
- And, $\mathfrak{C}$ is an agent's *comparative confidence judgment set* — over $\mathcal{B} \times \mathcal{B}$, *viz.*, the (entire) set of all pairs $p, q \in \mathcal{B}$.
- The idea is to extend Joyce's idea of justifying probabilism *via* an accuracy dominance norm (for b) to analogous norms for full belief/disbelief and comparative confidence.
- This requires filling-in appropriate notions of "dominance", and the investigating resulting notions of "coherence".

- Thus, we'll apply the basic Joycean idea to *three* types of judgments. And, we'll start with *full belief*, which is *clearest*.
- For simplicity, I'll talk about *finite*, *logically omniscient*, *opinionated* agents who make *definite judgments* regarding all propositions (or pairs thereof) in some algebra $\mathcal{B}$.
- I will consider three kinds of judgments:
  - **Qualitative**. S believes p [$B(p)$]. S disbelieves p [$D(p)$].
  - **Comparative**. S is strictly more confident in p than q [$p \succ q$]. S "doxastically indifferent" btw p, q [$q \sim p$]. [Taking $\succeq$ as primitive is more elegant — thanks, Daniel!]
  - **Quantitative**. S's degree of confidence/credence in p is r.
- For each of these judgments, I'll discuss relationships between (analogous) accuracy & coherence norms.
- The first step is to *define inaccuracy* for each of the three types of judgments. Once we've done that, we'll examine some new relationships between (in)accuracy & coherence.

- The *inaccuracies* of S's three types of *judgment sets* are (these get increasingly controversial/tricky — more below).
  - **Qualitative**. Let $\mathfrak{B}$ be the full set of S's qualitative judgments over $\mathcal{B}$. The *innaccuracy* of $\mathfrak{B}$ at a world w is given by the number of incorrect judgments in $\mathfrak{B}$ at w.
    - $B(p)$ is (in)correct in w iff p is true (false) at w.
    - $D(p)$ is (in)correct in w iff p is false (true) at w.
  - **Comparative**. Let $\mathfrak{C}$ be the full set of S's comparative judgments over $\mathcal{B} \times \mathcal{B}$. The *innaccuracy* of $\mathfrak{C}$ at a world w is given by the number of incorrect judgments in $\mathfrak{C}$ at w.
    - $p \sim q$ is (in)correct at w iff $p \equiv q$ is true (false) at w.
    - $p \succ q$ is (in)correct at w iff $p \,\&\, \neg q$ is false (true) at w.
    - As we'll see, this naive 2-valued scheme (with $\succ, \sim$ primitive) faces serious problems (Christensen's $p \sim \neg p$ puzzle).
    - ☞ Daniel's suggestion: have *acceptance* (A) and *rejection* (R) attitudes (analogous to B, D) toward (primitive) $\succeq$ claims.
  - **Quantitative**. Let b be S's credence function (b is a *function from $\mathcal{B}$ to the real numbers*). The *degree of inaccuracy* of b at a world w [$I(b, w)$] will be given by some *scoring-rule*.

- Consider this notion of (qualitative) *accuracy-dominance*:
  - One set of qualitative judgments $\mathfrak{B}'$ *accuracy-dominates* another $\mathfrak{B}$ iff (i) $\mathfrak{B}'$ has *strictly fewer* incorrect judgments at *some* possible worlds, and (ii) $\mathfrak{B}'$ contains *at most as many* incorrect judgments as $\mathfrak{B}$ at *every* possible world.
- Next, consider the following qualitative coherence norm:

(QC) S should not have a qualitative judgment set $\mathfrak{B}$ that is (*a priori*) *accuracy-dominated* by some alternative set $\mathfrak{B}'$.

- Why is (QC) compelling? For one thing, it is immune from one analogue of preface cases. Allow me to explain.
- In a (sufficiently bad) preface case, S has a judgment set $\mathfrak{B}$ which is inconsistent, but which is such that no consistent alternative $\mathfrak{B}'$ "looks better" to them, *given their evidence*.
- If we show S an alternative, consistent set $\mathfrak{B}'$, their evidence will suggest — *perhaps **non-misleadingly**!* — that $\mathfrak{B}'$ contains *more incorrect judgments* than their own set $\mathfrak{B}$.

- However, there can be no analogous cases when it comes to violations of (QC). Such cases would be *truly paradoxical*.
  1. Suppose S violates (QC). Then, there exists a $\mathfrak{B}'$ which S can know *a priori* accuracy-dominates their judgment set $\mathfrak{B}$.
  2. If I show S such a $\mathfrak{B}'$, their evidence will (as in the preface case) suggest that $\mathfrak{B}'$ has more incorrect judgments than $\mathfrak{B}$.
  3. But, then, I can run S through an (*a priori*) argument, which shows that $\mathfrak{B}'$ *cannot* have more incorrect judgments than $\mathfrak{B}$ — in *any* possible world. So, now S knows their evidence is misleading — with regard to its suggestion about $\mathfrak{B}$ *vs* $\mathfrak{B}'$.
- In this sense, (QC) seems more appropriate for those who want to maintain that there *are some* coherence norms for B. [That debate has been "stacked" in Kolodny's [15] favor!]
- But, what in the world is this (QC) norm *like*? Are there independent ways to understand it or get a grip on it? Yes.
- I'll give two characterizations of (QC) — in terms of violation and satisfaction — and then I'll mention some applications.

- The following theorem gives *one* way to understand (QC):
  - **Theorem**. *S violates* (QC) *iff* $\mathfrak{B}$ *contains a subset B which has a majority of incorrect judgments in every possible world.*
- Of course, (QC) is *strictly weaker* than (CB). That (QC) does *not* entail (CB) can be seen *via* a simple counterexample.
  - S can believe *both P and $\neg P$ without* violating (QC).

	P	$\neg P$	$B(P)$	$B(\neg P)$	$B(P)$	$D(\neg P)$	$D(P)$	$B(\neg P)$
w_1	**F**	**T**	incorrect	correct	incorrect	incorrect	correct	correct
w_2	**T**	**F**	correct	incorrect	correct	correct	incorrect	incorrect

- Here's a revealing result about *satisfying* (QC).
  - **Theorem**. *S satisfies* (QC) *iff* their $\mathfrak{B}$ can be numerically Pr-represented in the following precise, "Lockean" way:

($\mathcal{L}_{\mathfrak{B}}$) There exists a probability function Pr such that, $\forall p \in \mathcal{B}$:

$$B(p) \textit{ iff } \Pr(p) > \tfrac{1}{2}, \text{ and } D(p) \textit{ iff } \Pr(p) \le \tfrac{1}{2}$$
OR
$$B(p) \textit{ iff } \Pr(p) \ge \tfrac{1}{2}, \text{ and } D(p) \textit{ iff } \Pr(p) < \tfrac{1}{2}.$$

- Next, a concrete example in which (QC) *is* violated.

	$\mathfrak{B}$	$\mathfrak{B}'$	$\mathcal{L}_{\mathfrak{B}'}$
$\neg X \,\&\, \neg Y$	B	D	$\frac{11}{32}$
$X \,\&\, \neg Y$	B	D	$\frac{7}{32}$
$X \,\&\, Y$	B	D	$\frac{13}{32}$
$\neg X \,\&\, Y$	D	D	$\frac{1}{32}$
$\neg Y$	B	B	$\frac{18}{32}$
$X \equiv Y$	B	B	$\frac{24}{32}$
$\neg X$	D	D	$\frac{12}{32}$
X	B	B	$\frac{20}{32}$
$\neg(X \equiv Y)$	D	D	$\frac{8}{32}$
Y	D	D	$\frac{14}{32}$
$X \vee \neg Y$	B	B	$\frac{31}{32}$
$\neg X \vee \neg Y$	B	B	$\frac{19}{32}$
$\neg X \vee Y$	B	B	$\frac{25}{32}$
$X \vee Y$	B	B	$\frac{21}{32}$
$X \vee \neg X$	B	B	1
$X \,\&\, \neg X$	D	D	0

- S's $\mathfrak{B}$ isn't dominated by any *consistent* set, but $\mathfrak{B}$ is — *uniquely* — dominated by the "coherent" $\mathfrak{B}'$.
- As I mentioned, it is *impossible* for S's evidence to *non-misleadingly* make it appear to S that $\mathfrak{B}'$ contains more incorrect judgments than $\mathfrak{B}$.
- But, it is still possible for there to be a weaker sense in which S's evidence non-misleadingly suggests that her violation of (QC) may be "OK".
- Suppose S's evidence *non-misleadingly* supports the truth of the conjunction $X \,\&\, \neg Y$. Then, S may reason in the following way, when they see $\mathfrak{B}'$.
  - Look, I realize that $\mathfrak{B}'$ cannot have more incorrect judgments than my $\mathfrak{B}$ does.
  - But, *I have good evidence for/know* $X \,\&\, \neg Y$, which (if true) *rules-out* $\mathfrak{B}'$. Since *my* violation of (QC) is *equivalent* to my being dominated by $\mathfrak{B}'$, why should I be *moved* by my violation of (QC)? [*Kolodny's revenge*!?]

- Rachael Briggs, Kenny Easwaran and I are investigating various applications of this accuracy-dominance approach.
- One interesting application is to judgment aggregation. *E.g.*,
  - Majority rule aggregations of the judgments of a bunch of agents — each of whom satisfy (CB) — need not satisfy (CB).
- Question: does majority rule preserve *our* "qualitative coherence", *viz.*, is (QC) preserved by MR? Answer: No!
  - There are sets of judges (minimum # = 5) that (severally) satisfy (QC), while their majority profile *violates* (QC).
  - But, if a set of judges is (severally) *consistent* (in the classical sense), then their majority profile *must* satisfy (QC).
- Another application: define an "entailment" relation ($\Vdash$), as a relation between a set of judgments Γ and a p-judgment:
  - $\Gamma \Vdash B(p)$ iff $\Gamma \cup \{D(p)\}$ is "*incoherent*" [in the (QC) sense].
  - $\Gamma \Vdash D(p)$ iff $\Gamma \cup \{B(p)\}$ is "*incoherent*" [in the (QC) sense].
- Next: language dependence, and why it's *not* a problem.

- In these lectures, I will explain why *only one* of Miller's two types of language-dependence-of-verisimilitude problems is a (potential) threat to the sorts of accuracy-dominance approaches to coherence that I'll be discussing.
- The first (Qualitative) LD problem involves dependence on the choice of underlying *logical/formal language* in our representations of the doxastic spaces of epistemic agents.
- Consider an agent S with a language $\mathcal{L}_{HRW}$ having 3 atomic sentences (H, R, W). There will be 256 propositions in S's doxastic algebra $\mathcal{B}$ (as usual, we assume omniscience here).
- Suppose S is such that $D(H)$, $B(R)$, and $B(W)$. And, suppose that the *actual* world $w_@$ is such that $H \,\&\, R \,\&\, W$.
- S's judgment (sub)set $\mathbf{B}_1 = \{D(H), B(R), B(W)\}$ over H, R, and W seems to be "*closer to the truth at $w_@$*" than the set $\mathbf{B}_2 = \{D(H), D(R), D(W)\}$, since $\mathcal{B}_1$ has 1 incorrect judgment (at $w_@$), while $\mathcal{B}_2$ contains 3 incorrect judgments.

- This assessment of the "relative verisimilitudes of $\mathbf{B}_1$ *vs* $\mathbf{B}_2$ *at* $w_@$" depends on our choice of sentential linguistic representation. This is Miller's first LD problem [20, Ch. 11].
- To see this LD problem vividly, consider an alternative representation of S's doxastic space, which uses $\mathscr{L}_{HMA}$.
- $\mathscr{L}_{HMA}$ has atoms H and M, A, where M, A are such that:
  - $M \dashv\vDash H \equiv R$.
  - $A \dashv\vDash H \equiv W$.
- In $\mathscr{L}_{HMA}$, $\mathbf{B}_1$ becomes $\{D(H), D(M), D(A)\}$, and $\mathbf{B}_2$ becomes $\{D(H), B(M), B(A)\}$. And, in $w_@$, we have $H \,\&\, M \,\&\, A$. Thus, using $\mathscr{L}_{HMA}$ leads to a *reversal* of the assessment of "relative verisimilitudes of $\mathbf{B}_1$ *vs* $\mathbf{B}_2$ *at* $w_@$."
- This *seems* to pose a problem for our accuracy-dominance norm for qualitative judgments. But, in fact, it does *not*.
- In order to see why this LD problem is *merely* an *apparent* one, we must return to our definitions from above.

- Here are the two salient definitions:
  - $\mathfrak{B}$ is the *full* set of S's qualitative judgments *over the entire algebra of propositions* $\mathcal{B}$. The *innaccuracy* of $\mathfrak{B}$ at a world w is given by the number of incorrect judgments in $\mathfrak{B}$ at w.
  - One (*full*) qualitative judgment set $\mathfrak{B}'$ *accuracy-dominates* another $\mathfrak{B}$ iff (i) $\mathfrak{B}'$ has *strictly fewer* incorrect judgments at *some* possible worlds, and (ii) $\mathfrak{B}'$ contains *at most as many* incorrect judgments as $\mathfrak{B}$ at *every* possible world.
- The crucial point here is that *our* assessments of "relative verisimilitude" are *only* made with respect to *full* judgment sets $\mathfrak{B}/\mathfrak{B}'$ *over S's entire algebra of propositions* $\mathcal{B}$.
- Miller's first LD problem trades on the fact that "the # of incorrect judgments in $\mathbf{B}$ at w" can *vary*, depending on our *linguistic representation* of S — for *proper subsets* $\mathbf{B} \subset \mathfrak{B}$.

☞ *This* language dependence *disappears* when we *restrict our comparisons to full judgment sets* $\mathfrak{B}/\mathfrak{B}'$. "The # of incorrect judgments in $\mathfrak{B}$ at w" is a *language invariant* quantity.

- Once upon a time, *comparative* confidence judgments were thought to be more "secure" or "basic" or "fundamental" than *numerical* confidence/credence judgments [14].
- In his watershed essay, de Finetti [4] begins his story about the foundation of subjective probability theory, as follows:

> Let us consider a well-defined event and suppose that we do not know in advance whether it will occur or not; the doubt about its occurrence to which we are subject lends itself to comparison, and, consequently, to gradation. If we acknowledge only, first, that one uncertain event can only appear to us (a) equally probable, (b) more probable, or (c) less probable than another; second, that an uncertain event always seems to us more probable than an impossible event and less probable than a necessary event; and finally, third, that when we judge an event E more probable than an event E', which is itself judged more probable than an event E'', the event E can only appear more probable than E'' (transitive property), it will suffice to add to these three evidently trivial axioms a fourth, itself of a purely qualitative nature, in order to construct rigorously the whole theory of probability.

- What de Finetti is describing here is a "foundationalist" conception of subjective probability, where the *foundation* consists of *relations of comparative confidence* $(\succ, \sim)$.

- First, de Finetti thought that the following two "axioms" for the "strictly more confident in" relation $\succ$ were *self-evident*:
  (1) $\succ$ is transitive, asymmetric, and irreflexive.
    - More precisely, that $\succ$ imposes a *strict total order* on $\mathcal{B}$. [Note: de Finetti *assumes*: $p \sim q$ *iff* $p \not\succ q \,\&\, q \not\succ p$.]

  (2) For all p, $p \not\succ \top$ and $\bot \not\succ p$.
    (2.1) S should never be strictly more confident in any p than $\top$.
    (2.2) S should never be strictly more confident in $\bot$ than any p.
- Second, de Finetti thought that the following "additivity" axiom — together with axioms (1) and (2) — *suffices* to ensure *numerical probabilistic representatability* of $\succ$.
  (3) If $\langle p, q\rangle$ and $\langle p, r\rangle$ are both mutually exclusive pairs, then:
  $$q \succ r \text{ only if } (p \vee q) \succ (p \vee r).$$
- That is, de Finetti believed that the following was true:
  (†) If the relation $\succ$ satisfies (1)–(3), then there exists a *numerical probability function* b such that:
  $$p \succ q \text{ if and only if } b(p) > b(q).$$

- A few decades later, it was discovered [17] that de Finetti was *wrong* about (†). This was a *crack in the foundation*.
- Scott [25] gave *a much stronger* "additivity" axiom (3′), such that (1)–(3′) *does suffice* for numerical Pr-representability of $\succ$. [I will discuss Scott's Axiom and its implications, below.]
- Here, I'll focus on *justifying de Finetti*'s "intuitive" axioms for $\succ$. *Nobody* seems to offer *any* justifications for (1)–(3) [6, 8]. But, some *do* offer (dominance) justifications of the *numerical* Pr-axioms (see Part II). d.F.'s overall strategy was:
  (i) First, lay down some "*intuitive*" axioms for $\succ$-orderings.
  (ii) Then, show that these axioms suffice to ensure numerical probabilistic representability of any "intuitive" $\succ$-ordering.
  (iii) Finally, justify the axioms of numerical probability theory (*via* the *Brier-dominance approach*, to be discussed Friday).
- If this could be achieved, then one could ground the desired "foundationalist" conception of subjective probability. This project failed at step (ii). But, I think even (i) is dubious...

- Recall our definition of "inaccuracy" of a set of comparative judgments. [Note: other "scoring schemes" are possible for comparative judgments. I will return to this issue below.]
- We can use an analogous notion of "accuracy dominance" to *justify* (some) "axioms" of comparative probability. To wit:
  - $\mathfrak{C}'$ *accuracy-dominates* $\mathfrak{C}$ iff $\mathfrak{C}'$ contains *strictly fewer* incorrect judgments than $\mathfrak{C}$ at *some* w's, and $\mathfrak{C}'$ contains *at most as many* incorrect judgments as $\mathfrak{C}$ at *every* w.

☞ de Finetti's axioms (1) & (2) *can* then (*modulo* a small *caveat* — see below) be given an accuracy-dominance justification.

  - **Theorem**. If S's $\succ$-ordering (*viz.*, S's comparison set $\mathfrak{C}$) violates *either* (1) *or* (2), then (*modulo* a small *caveat* — see below) there exists a $\mathfrak{C}'$ which *accuracy-dominates* $\mathfrak{C}$.
- As far as I know, we (this is joint work with David McCarthy) are the first to offer *any* justification of (1) and/or (2).

  [And, ours is *natural*, given de Finetti's *numerical* approach [3]. I'll explain what I mean by this later on in these lectures.]

- But, *all is not beer and skittles...*
- de Finetti's axiom (3) does *not* have an accuracy-dominance justification [∴ the Scott Axiom (3′), which is required for numerical Pr-representation of $\succ$ doesn't have one either].
- This suggests that a de Finetti-style "foundationalist" approach to epistemic probability contains a *logical gap*.

  ☞ *Why* should we think $\succ$ has a numerical Pr-representation?
- It is very interesting to note that the principles of comparative probability that have been uncontroversial (or "self-evident") in the literature [8] are (basically) those with an accuracy-dominance justification in our sense. *E.g.*,
  (4) If p entails q, then $p \nsucc q$. ["*Monotonicity*" of $\succ$.]
- And, those which have been seen as controversial *fail* to have an accuracy-dominance justification. *E.g.*, "additivity" (3), the Scott Axiom (3′), and other principles, such as:
  (5) If $p \succ q$, then $\neg p \nsucc \neg q$. ["*Complementarity*" of $\succ$.]

- Consider the following weak form of transitivity.
  (WT) If $p \succ q$ and $q \succ r$, then $r \nsucc p$.
- If S violates (WT), then S is accuracy-dominated. Proof:

	P	Q	R	$P \succ Q$	$Q \succ R$	$R \succ P$	$P \sim Q$	$Q \sim R$	$P \sim R$
w_1	**T**	**T**	**T**	incorrect	incorrect	incorrect	correct	correct	correct
w_2	**T**	**T**	**F**	incorrect	correct	incorrect	correct	incorrect	incorrect
w_3	**T**	**F**	**T**	correct	incorrect	incorrect	incorrect	incorrect	correct
w_4	**T**	**F**	**F**	correct	incorrect	incorrect	incorrect	correct	incorrect
w_5	**F**	**T**	**T**	incorrect	incorrect	correct	incorrect	correct	incorrect
w_6	**F**	**T**	**F**	incorrect	correct	incorrect	incorrect	incorrect	correct
w_7	**F**	**F**	**T**	incorrect	incorrect	correct	correct	incorrect	incorrect
w_8	**F**	**F**	**F**	incorrect	incorrect	incorrect	correct	correct	correct

- In fact, this is the *unique* dominating $\mathfrak{C}'$. [*Kolodny's revenge* applies — suppose S *has good reason for/knows* $P \succ Q$.]
- The "small caveat" is that *not all* violations of transitivity are dominated! *E.g.*, an S such that $p \sim q$, $q \sim r$, $p \succ r$.
  - This is welcome (to me), since I think there are permissible examples of this kind (*e.g.*, perceptual indiscriminability).
- Next: other consequences of our 2-valued scoring scheme.

- If S violates *Monotonicity* (4), then S is accuracy-dominated.

  (4) If p entails q, then $p \nsucc q$.

	P	Q	$P \succ Q$	$Q \succ P$
w_1	**T**	**T**	B	B
w_2	**T**	**F**	—	—
w_3	**F**	**T**	C	A
w_4	**F**	**F**	B	B

- In fact, as this table shows, *any scoring scheme with the above structure* (where $A < C$) entails Monotonicity.
- To see that de Finetti's additivity axiom (3) does *not* have a dominance justification, one must look at *all* the possible ways of "fixing" a violation of (3), and show that *none* of these lead to a comparison set that dominates the original.
- There aren't that many cases to check. [I won't show them.]
- Next: an objection to our simple, 2-valued scoring scheme.

- Recall our definition of "inaccuracy"/"accuracy dominance" for a (complete) set of comparative judgments $\mathfrak{C}$.
  - **Comparative.** Let $\mathfrak{C}$ be the full set of S's comparative judgments over $\mathcal{B} \times \mathcal{B}$. The *innaccuracy* of $\mathfrak{C}$ at a world w is given by the number of incorrect judgments in $\mathfrak{C}$ at w.
    - $p \sim q$ is (in)correct at w iff $p \equiv q$ is true (false) at w.
    - $p \succ q$ is (in)correct at w iff $p \,\&\, \neg q$ is true (false) at w.
  - $\mathfrak{C}'$ *accuracy-dominates* $\mathfrak{C}$ iff $\mathfrak{C}'$ contains *strictly fewer* incorrect judgments than $\mathfrak{C}$ at *some* w's, and $\mathfrak{C}'$ contains *at most as many* incorrect judgments as $\mathfrak{C}$ at *every* w.
- This simple, 2-valed scoring scheme may seem overly simplistic. It is based on the following underlying norm:

  (†) S should be more confident in truths than falsehoods.
- So, if p is **T** and q is **F**, then the judgments $q \succ p$ and $p \sim q$ are in violation of this basic underlying norm (†).
- But, (†) *alone* does not justify our choice of 2-valued scheme. Indeed, other scoring schemes seem plausible.

- Let's use "+1" to denote *best* epistemic status, "−1" to denote *worst* epistemic status, and "0" to denote "middling" epistemic status. Our simplest, 2-valued scheme is:

	P	Q	$P \succ Q$	$Q \succ P$	$Q \sim P$
w_1	**T**	**T**	−1	−1	+1
w_2	**T**	**F**	+1	−1	−1
w_3	**F**	**T**	−1	+1	−1
w_4	**F**	**F**	−1	−1	+1

- If we're going to use only 2-values ("correct/incorrect"), then it seems to me that this scheme is *forced* on us, by (†).
- But, one might think that a 3-valued scheme makes more sense. David Christensen makes the following observation.

  > Suppose I'm going to flip a coin. Can I rationally be indifferent between heads (H) and tails (T)? It seems that $H \sim T$ would be dominated by $H \succ T$ (or $T \succ H$), since $H \sim T$ is guaranteed to be "incorrect" and the latter aren't.
- Christensen is right. And, he suggests a 3-valued scheme.

	P	Q	$P \succ Q$	$Q \succ P$	$Q \sim P$
w_1	**T**	**T**	0	0	+1
w_2	**T**	**F**	+1	−1	0
w_3	**F**	**T**	−1	+1	0
w_4	**F**	**F**	0	0	+1

- I agree that D.C.'s scheme does seem superior (intuitively) to our simplest 2-valued scoring scheme (in various ways).
- If we use this (or some other) 3-valued scheme, the obvious way to calculate the score of $\mathfrak{C}$ (at w) is to take the *sum* of these 3-valued scores for all the propositions in $\mathfrak{C}$ (at w).
- Then, we would define accuracy-dominance as follows:
  - $\mathfrak{C}'$ *accuracy-dominates* $\mathfrak{C}$ iff $\mathfrak{C}'$ has a *higher* score than $\mathfrak{C}$ at *some* w, and $\mathfrak{C}'$ doesn't have a lower score than $\mathfrak{C}$ at *any* w.
- Anyway, moving to a 3-valued scheme can *not* fill the gap in de Finetti's argument — we have an *impossibility result*.
- [Daniel's alternative (more below) faces a similar challenge.]

Theorem. *No* 2 *or* 3-valued scoring scheme is such that:

(0) S entails (at least *some* instances of) *both* transitivity *and* additivity as (weak) dominance norms.

and, the the following eight (8) scoring *desiderata* are met:

(1) Having a subset of judgments $\{p \succ q, p \succ r, q \sim r\}$ should not — *in and of itself* — entail "incoherence".

(2) *Ditto* for subsets of the form $\{p \succ q, p \succ r, q \succ r\}$.

(3) $p \succ q$ should get a "worst" score when p is **F** and q is **T**.

(4) $p \succ q$ should get the same score when p and q are both **T** as it does when p and q are both **F**.

(5) $p \sim q$ should get the same score when p and q are both **T** as it does when p and q are both **F**.

(6) $p \sim q$ should get the same score when p is **T** and q is **F** as it does when p is **F** and q is **T**.

(7) The score of $p \succ q$ when p is **T** and q is **F** should not be strictly worse than the score of $p \succ q$ when p, q are both **T**.

(8) The score of $p \succ q$ when p is **T** and q is **F** should be strictly better than the score of $p \succ q$ when p is **F** and q is **T**.

- These eight *desiderata* seem to be sacrosanct (Christensen and everyone else I've talked to seems to accept all of them).
- The upshot of our Theorem is that — *it doesn't matter which scoring scheme you use*. *No* scoring scheme can ground *all* of de Finetti's axioms for comparative probability.
  - In fact, our simplest 2-valued scheme *gets as close as any 2 or 3-valued scheme* to grounding all of de Finetti's axioms.
- So, it seems there is no accuracy-dominance justification of all of de Finetti's intuitive axioms (much less the *unintuitive* Scott Axiom — see Extras slides). This *re*-raises a question:

☞ *Why* should we think $\succ$ has a numerical Pr-representation?

- There seems to be no compelling reason to suppose that our comparative confidence orderings are (numerically) probabilistically representable. This is an important *lacuna*.
- Next: de Finetti's Conjecture, Scott's Axiom, and Fine's worries.
- But, first, *Daniel's alternative*, which takes $\succeq$ as primitive.

- Since de Finetti, it has become customary to take $\succeq$ as primitive, and to define $\succ$ and $\sim$ in terms of $\succeq$ as follows:
  - $p \sim q \overset{\text{def}}{=} (p \succeq q) \,\&\, (q \succeq p)$.
  - $p \succ q \overset{\text{def}}{=} (p \succeq q) \,\&\, \neg(q \succeq p)$.
- And, Fine's axiomatization has become more standard. It is:

(A1) $\top \succ \bot$. [*i.e.*, $(\top \succeq \bot) \,\&\, \neg(\bot \succeq \top)$]

(A2) $(p \succeq q) \vee (q \succeq p)$.

(A3) $p \succeq \bot$.

(A4) If $p \succeq q$ and $q \succeq r$, then $p \succeq r$.

(A5) If $\langle p, q\rangle$ and $\langle p, r\rangle$ are mutually exclusive, then:

$$q \succeq r \iff (p \vee q) \succeq (p \vee r).$$

- Daniel's idea is to introduce attitudes of acceptance (A) and rejection (R) toward $\succeq$-relations (analogous to B and D).
- This leads to a more elegant 2-valued scoring scheme.
- Next: Daniel's proposal, and its similarities to Christensen's.

- Daniel's main insight is to introduce attitudes of acceptance (A) and rejection (R) toward primitive $\succeq$ relational claims.
  - $A(p \succeq q)$ is inaccurate at w iff $q \,\&\, \neg p$ is true at w.
  - $R(p \succeq q)$ is accurate at w iff $q \,\&\, \neg p$ is true at w.
- Note how (a) this flows *directly* from the underlying (†)-norm, and (b) it dovetails nicely with the B/D attitudes.
- For each pair $\{p \succeq q, q \succeq p\}$, S must make a pair of R/A judgments. This leads to four possible pairs of judgments:
  - $\{A(p \succeq q), A(q \succeq p)\}$. That is, $A(p \sim q)$.
  - $\{A(p \succeq q), R(q \succeq p)\}$. That is, $A(p \succ q)$.
  - $\{R(p \succeq q), A(q \succeq p)\}$. That is, $A(q \succ p)$.
  - $\{R(p \succeq q), R(q \succeq p)\}$. [This is a new possibility, not recognized on our old scheme! It plays a nice role, later.]
- This scheme is better than the one David and I had. And, it leads to something very similar to Christensen's approach (without having to go "3-valued" + summation of "scores").

- Now, we can re-cast Fine's axioms as *CP-norms for A/R*.

(A1) $A(\top \succ \bot)$. [*i.e.*, $A(\top \succeq \bot)$ & $R(\bot \succeq \top)$.]

(A2) $A(p \succeq q) \vee A(q \succeq p)$. [*i.e.*, *Not*-($R(p \succeq q)$ & $R(q \succeq p)$).]

(A3) $A(p \succeq \bot)$.

(A4) If $A(p \succeq q)$ and $A(q \succeq r)$, then $A(p \succeq r)$.

(A5) If $\langle p, q\rangle$ and $\langle p, r\rangle$ are mutually exclusive, then:

$$A(q \succeq r) \iff A\left((p \vee q) \succeq (p \vee r)\right).$$

- I won't get into all the gory details here, but the following results can be shown for Daniel's scoring regime:
  - S *must satisfy* (A1)–(A3) *on pain of weak domination.*
  - S *can violate* (A4) *without* being weakly dominated.
  - S *can violate* (A5) *without* being weakly dominated.
- Unfortunately, like Christensen's scheme, we *also* have:
  - S *must not* be such that $\{A(p \succ q), A(q \succ r), A(p \succ r)\}$!

☞ Such schemes are *coherent-inadmissible* (Joyce's [12] locution) — they *rule-out* some "*CP-coherent*" sets $\mathfrak{C}$.

- Another impossibility theorem (Daniel) regarding $\sim$-scoring:

Theorem. Given very weak adequacy assumptions (motivated by the fundamental comparative norm), if a scoring scheme for $\sim$ judgments is *extensional* (*i.e.*, depends only on the truth-values assigned to propositions in worlds), then it will either face Christensen's problem (*i.e.*, $p \sim \neg p$ will always dominated) or it will face Ben's problem (*i.e.*, it will have unintuitive implications for small lotteries).

- This result, together with the result(s) above concerning the (inevitable) coherent inadmissibility of (otherwise plausible) schemes for scoring *both* $\sim$ *and* $\succ$ judgments, suggest:

☞ There seems to be no plausible, extensional way to score *both* $\sim$ *and* $\succ$ judgments simultaneously ($\sim$ is problematic).

- This leaves open the possibility of scoring *just* $\succ$ judgments. Indeed, this can be done (in the obvious way), and it leads to a coherent-admissible scheme. But, it leads to very weak dominance constraints (normalization + monotonicity + ??).

- de Finetti conjectured that axioms (A1)–(A5) *suffice* for:

(†) $(\exists \Pr)(\forall p)(\forall q)[p \succ q$ *if and only if* $\Pr(p) > \Pr(q)]$.

- But, it was later discovered [17] that this was *false*. That is, there exist $\succ$-orderings satisfying de Finetti/Fine's axioms (A1)–(A5), but for which there is no Pr-representation.
- Here is the counterexample reported in [17]. Imagine a Boolean algebra containing 5 states/atoms: $\{s_1, s_2, s_3, s_4, s_5\}$.
- And, suppose we have the following four $\succ$ relations:

(i) $s_4 \succ s_1 \vee s_3$.

(ii) $s_2 \vee s_3 \succ s_1 \vee s_4$.

(iii) $s_1 \vee s_5 \succ s_3 \vee s_4$.

(iv) $s_1 \vee s_3 \vee s_4 \succ s_2 \vee s_5$.

- It can be shown that (i)–(iv) are compatible with de Finetti's axioms (1)–(3), *but* (i)–(iv) have no Pr-representation.
- Exercise: verify that this is a counterexample. [Open question: are there any smaller counterexamples?]

- Before stating the Scott Axiom, we'll need one definition:

Definition. For each state description $\mathfrak{s}$ and each *sequence* (n-tuple) of propositions $\mathbf{Z} = \langle z_1, \ldots, z_n\rangle \in \prod_n \mathcal{B}$, let $\mathfrak{c}(\mathfrak{s}, \mathbf{Z})$ be the number of elements of $\mathbf{Z}$ that are entailed by $\mathfrak{s}$.

- OK, here's the (dreaded) Scott Axiom:

(SA) Let $\mathbf{X}, \mathbf{Y} \in \prod_n \mathcal{B}$ be (arbitrary) sequences of propositions, each having length $n > 0$. Let $\langle x_1, \ldots, x_n\rangle$ denote the members of $\mathbf{X}$, and $\langle y_1, \ldots, y_n\rangle$ denote the members of $\mathbf{Y}$.

If the following two conditions are satisfied

i. For every state description $\mathfrak{s}$, $\mathfrak{c}(\mathfrak{s}, \mathbf{X}) = \mathfrak{c}(\mathfrak{s}, \mathbf{Y})$.

ii. For all $i \in (1, n]$, $x_i \succeq y_i$.

then, the following must also be the case

iii. $y_1 \succeq x_1$.

- Not only is (SA) *unintuitive*, it is also *quite strong*. It entails *both* (A4) and (A5) — *i.e.*, *both* transitivity *and* additivity.
- Next: proofs that (SA) entails both (A4) and (A5). First, (A5).

- Here, I will prove that (SA) entails (A5). Exercise(s): prove some strong theorem(s) — inspired by Pr — from (SA).
- let $\mathbf{X} = \langle p \vee r, q \rangle$ and $\mathbf{Y} = \langle p \vee q, r \rangle$, where $\langle p, q \rangle$ are mutually exclusive and $\langle p, r \rangle$ are mutually exclusive.
- That is, $x_1 = p \vee r$, $y_1 = p \vee q$, $x_2 = q$, and $y_2 = r$.
- Now, suppose (SA). Then, the (⇒) direction of (A5) follows.
- To see why, assume the left hand side of (A5). That is, suppose that $q \succeq r$, *i.e.*, that $x_2 \succeq y_2$. In the case at hand, this is equivalent to condition (ii) in the antecedent of (SA).
- Thus, in order to establish additivity (A5), all we need to do is show that $(p \vee q) \succeq (p \vee r)$, *i.e.*, that (iii) $y_1 \succeq x_1$.
- This will follow from (SA), provided that we can show condition (i) of (SA) must also be true in this case.
- Indeed, (i) must be true in this case, and this can most easily be seen *via* the following *schematic truth-table*.

	p	q	r	$\mathfrak{s}_i \vDash p \vee r$?	$\mathfrak{s}_i \vDash q$?	$\mathfrak{s}_i \vDash p \vee q$?	$\mathfrak{s}_i \vDash r$?
$\mathfrak{s}_1$	**T**	**T**	**T**	—	—	—	—
$\mathfrak{s}_2$	**T**	**T**	**F**	—	—	—	—
$\mathfrak{s}_3$	**T**	**F**	**T**	—	—	—	—
$\mathfrak{s}_4$	**T**	**F**	**F**	YES	NO	YES	NO
$\mathfrak{s}_5$	**F**	**T**	**T**	YES	YES	YES	YES
$\mathfrak{s}_6$	**F**	**T**	**F**	NO	YES	YES	NO
$\mathfrak{s}_7$	**F**	**F**	**T**	YES	NO	NO	YES
$\mathfrak{s}_8$	**F**	**F**	**F**	NO	NO	NO	NO

- Because $\langle p, q \rangle$ and $\langle p, r \rangle$ are mutually exclusive, the (families of) state descriptions $\mathfrak{s}_1$–$\mathfrak{s}_3$ are *impossible*. So, we can ignore those rows of the schematic truth-table.
- Now, in oder to show that (i) holds in this case, we just need to show that each of the five (*possible* families of) state descriptions $\mathfrak{s}_4$–$\mathfrak{s}_8$ satisfies condition (i) of (SA).
- This is easily verified by inspection of the table, since each of these rows contains the same number of "YES"s in both pairs of columns on the right. □ The (⇐) proof is similar.

- I think the best way to grasp the content of (SA) is *via* the following illuminating theorem of Fishburn [8, Ch. 4].

  Theorem (Fishburn). (SA) is true *if and only if* there exists a mass function $\mathfrak{m}$ on $\mathcal{B}$ such that, for all propositions p and q in $\mathcal{B}$, the following *real-valued representation* holds:

  $$(\star) \qquad p \succeq q \text{ if and only if } \sum_{\mathfrak{s}_p \vDash p} \mathfrak{m}(\mathfrak{s}_p) \geq \sum_{\mathfrak{s}_q \vDash q} \mathfrak{m}(\mathfrak{s}_q).$$

  And, given Fine's axioms (A1) & (A3), there will always be a *probability* mass function $\mathfrak{m}$ satisfying ($\star$).
- Fishburn's Theorem reveals that (SA) *alone* ensures a real-valued representation ($\mathcal{R}_{\succeq}$) of the $\succeq$-ordering.
- Not only does this imply de Finetti's additivity axiom (3)/(A5), but it also implies (A2) as well ($\geq_{\mathbb{R}}$ is a *total order*).
- Thus, once (SA) is onboard, the only axiom of de Finetti that can do *any* work is his axiom (2) [*viz.*, Fine's (A1) & (A3)], which just ensures $\mathcal{R}_{\succeq}$ is a *probabilistic* representation of $\succeq$.

- Various other sufficient (but non-necessary) conditions for numerical Pr-representability have been proposed [6, Ch. 2].
- I will discuss a couple of these. But, first, note that *numerical* representability (*per se*) is actually *easy*. In fact:

  Theorem [6, p. 22]. Any comparative probability ordering $\succeq$, on (finite) $\mathcal{B} \times \mathcal{B}$, satisfying Fine's axioms (A1)–(A5) is s.t.: $\exists f : \mathcal{B} \mapsto \mathbb{R}$ satisfying the following two conditions:

  (a) $\forall p, q \in \mathcal{B}$: $p \succeq q$ iff $f(p) \geq f(q)$, and

  (b) $\exists g : \mathcal{B} \times \mathcal{B} \mapsto \mathbb{R}$, where g is *symmetric, strictly increasing, and associative*, such that for all mutually exclusive $p, q \in \mathcal{B}$:

  $$f(p \vee q) = g\left(f(p), f(q)\right).$$
- Thus, the counterexample of Kraft et. al. [17] (above) shows that there is something *invidious* about the choice "$g = +$" that is assumed by classical numerical probability calculus.

☞ Very strong assumptions are required to ensure that the f/g-representation — implied by (A1)–(A5) — is *additive*.

Here, the following passage from Fine [6, *p*. 73] is poignant:

It can be shown [1] that under mild conditions the strictly increasing, symmetric, associative function g can be represented in the form

$$g(x, y) = h\left(h^{-1}(x) + h^{-1}(y)\right),$$

where h is some [monotone] function. From the quantitative f we can derive an additive probability P [*via*] the definition $P(\cdot) \stackrel{\text{def}}{=} h^{-1}(f(\cdot))$. The function g' corresponding to P is then $g'(x, y) = x + y$.

What real considerations make us prefer the particular choice of measurement scale h^{-1} yielding an additive, quantitative probability P?

If we think of probability as a model of relative, frequency, then we seem to be led to accept the choice of $g'(x, y) = x + y$. However, agreement on probability as built upon relative frequency as the quantitative core of chance phenomena does not eliminate the possibility that P may depend on some function of the relative-frequency otber than the identity function.

For example, we might interpret $P(F)$ as $\lim_{n\to\infty}\left(\frac{N_F(n)}{n}\right)^2$, where $N_F(n)$ is the number of occurrences of the event F in n trials.

In this case we would find that F_1 and F_2 disjoint [*viz*., F_1 and F_2 mutually exclusive] imply that

$$P(F_1 \vee F_2) = \left(\sqrt{P(F_1)} + \sqrt{P(F_2)}\right)^2,$$

the scale transformation h^{-1} taking P into an additive probability assignment being $h^{-1}(x) = \sqrt{x}$.

If we accept the fact that there is exactly as much information about a chance event F in $\left(\frac{N_F(n)}{n}\right)^2$ as in $\frac{N_F(n)}{n}$, then it becomes only a matter of habit or convenience as to which of these we prefer to work with — a matter of choice, not necessity.

An analogy to this situation is that of coordinate-free results in analytic and vector geometry. The choice of an additive scale is analogous to choosing a fixed coordinate system and expressing all results in that system, although the results do not depend on the particular system chosen. The danger in this procedure when applied to probability is the ease with which we forget that the results are coordinate-free and then attempt to make something of the choice of system.

- Fine makes another point about "calibration" and $\succeq$.
  - You might think that additivity is ensured by thinking in terms of "calibration". But, consider the following frequency data (E) concerning a partition of 5 events $\{a, b, c, d, e\}$

    (‡) $\frac{\#_a(n)}{n} = \frac{1}{16}$, $\frac{\#_b(n)}{n} = \frac{2}{16}$, $\frac{\#_c(n)}{n} = \frac{3}{16}$, $\frac{\#_d(n)}{n} = \frac{4}{16}$, $\frac{\#_e(n)}{n} = \frac{6}{16}$.
  - Suppose that $\succeq$ is "calibrated with E" in the following sense:

    ($\star$) If $\frac{\#_p(n)}{n} > \frac{\#_q(n)}{n}$, then $p \succeq q$.
  - This does *not* ensure that $\succeq$ has an additive representation.
  - (‡) + ($\star$) do *not rule-out* the Kraft *et. al.* structure [17]:

    (i) $d \succ a \vee c$.

    (ii) $b \vee c \succ a \vee d$.

    (iii) $a \vee e \succ c \vee d$.

    (iv) $a \vee c \vee d \succ b \vee e$.
  - Note: plausibly — for small n — one *needn't* accept: $p \sim q$ *iff* $\frac{\#_p(n)}{n} = \frac{\#_q(n)}{n}$ as a precondition for "$\succeq$-calibration".

- Other sufficient (but non-necessary) conditions for Pr-representability of $\succeq$ (*viz*., for $g = +$) have been floated. The most popular of these (Savage [24], Koopman [16]) is:

  (**S**) For infinitely many n, $\exists$ an *n-fold uniform partition* of $\mathcal{B}$.
- Where, an *n-fold uniform partition* $\mathcal{P}_n$ of $\mathcal{B}$ is an n-set partition of $\mathcal{B}$ such that: the union of no k sets in $\mathcal{P}_n$ is more probable than the union of any $k + 1$ sets in $\mathcal{P}_n$.
- It can be shown that de Finetti's (1)–(3) + (**S**) entail the existence of a *probabilistic* numerical representation of $\succeq$.
  - But, (1)–(3) + (**S**) also entail that $\mathcal{B}$ is *infinite*.
- Generally, sufficient (but non-necessary) conditions tend to force $\mathcal{B}$ to be infinite. Some do not. *E.g.*, Luce's condition:

  (**L**) $\forall p, q, r, s \in \mathcal{B}$ s.t. p, q are mutually exclusive, $p \succeq r$, and $q \succeq s$, $\exists r', s', t \in \mathcal{B}$ s.t. (i) r', s' are mutually exclusive, (ii) $t \sim p \vee q$, (iii) $r' \sim r$, (iv) $s' \sim s$, and (v) $t \vDash r' \vee s'$.
- (**L**) doesn't entail infinite $\mathcal{B}$. But, it's still quite strong. The question is: are such conditions *rational* $\succeq$*-requirements*?

- Standard arguments for *probabilism* are of the form [10]:
  - An agent S has a non-probabilistic partial belief function b iff ($\iff$) S has some "bad" property B (*in virtue of* the fact that their c.f. b has a certain "bad" *formal* property F).
- These *arguments* rest on *Theorems* ($\Rightarrow$) and *Converse Theorems* ($\Leftarrow$): b is non-Pr $\iff$ b has formal property F.
  - **Dutch Book Arguments** [22, 3]. B is *susceptibility to sure monetary loss* (in a certain betting set-up), and F is the formal role played by non-Pr b's in the DBT/Converse DBT.
  - **Representation Theorem Arguments** [24]. B is *having preferences that violate some of Savage's axioms* (and/or *being unrepresentable as an expected utility maximizer*), and F is the formal role played by non-Pr b's in the RT.
- To the extent that we have reasons to avoid these B's, these arguments provide reasons (not) to have a(n) (in)coherent b.
- Joycean [13] arguments for probabilism also fit this pattern.

- According to Joyce [13], if we view credences as "estimates" of (suitable) "numerical representations of truth-values" of propositions, then we can give an argument for probabilism that is based on the "accuracy" of these "estimates".
- Consider a very simple, logically omniscient, opinionated agent S who has only one atomic sentence P in his language.
- All that matters concerning S's *coherence* is whether S's credences $b(P)$, $b(\neg P)$ *sum to one* (*and are non-negative*).
- Following Joyce, let's associate the truth-value **T** (at each world w) with the number 1 and the truth-value **F** with 0.
- The idea will be that $b(p)$ represents the agent S's "estimate" of the truth-value of p. These "estimates" will be subject to an accuracy norm, which will, in turn, give rise to a coherence norm (*viz.*, *probabilism*) for credences.
- Next, measuring the "accuracy" of Joycean "estimates" (b).

- The *inaccuracy* of $b(p)$ at world w will be b's "distance (d) from the number associated with p's truth-value" at w.
- **Example**. Suppose S has just two (contingent) propositions $\{P, \neg P\}$ in their doxastic space. Then, there are two salient possible worlds (w_1 in which P is **T**, and w_2 in which P is **F**). And, the *overall inaccuracy* of b at w [$I(b, w)$] is given by:
  - $I(b, w_1) = d(b(P), 1) + d(b(\neg P), 0)$.
  - $I(b, w_2) = d(b(P), 0) + d(b(\neg P), 1)$.
- Various measures (d) of "distance from 0/1-truth-value" have been proposed/defended in the historical literature.
- de Finetti [4] endorsed the following measure of "distance from truth-value" (in one argument for probabilism):
  - $\mathfrak{s}(x, y) = (x - y)^2$.
- The distance measure $\mathfrak{s}$ gives rise to a measure of *overall* inaccuracy ($I_\mathfrak{s}$), which is known as the *Brier Score*. In our toy

  - $I_\mathfrak{s}(b, w_1) = \mathfrak{s}(b(P), 1) + \mathfrak{s}(b(\neg P), 0) = (b(P) - 1)^2 + b(\neg P)^2$.
  - $I_\mathfrak{s}(b, w_2) = \mathfrak{s}(b(P), 0) + \mathfrak{s}(b(\neg P), 1) = b(P)^2 + (b(\neg P) - 1)^2$.
- If one adopts the Brier Score as one's measure of b's inaccuracy, then one can give an "accuracy-dominance argument" for the axioms of the probability calculus.
- de Finetti [3] was first to prove this sort of theorem. Joyce [13, 12] sees this as *accuracy*-dominance (also [23]).
  - **Theorem** (de Finetti). b is *non*-probabilistic *if and only if* there exists a *probabilistic* credence function b' such that (a) *b' has a strictly lower Brier Score than b at some worlds*, and (b) *b' never has a greater Brier Score than b at any world.*
- ☞ The "bad" B is: *being dominated in accuracy*; and, the "bad" F is: the c.f. b is *Brier-dominated* by some coherent c.f. b'.
- Finalé: three worries about Joyce-style arguments for probabilism. (1) sensitivity to choice of "accuracy measure", (2) an "evidentialist" worry, (3) language-dependence.

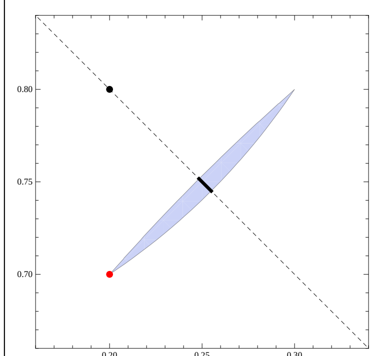
- Recall how inaccuracy measures $I(b, w)$ are defined in terms of measures d of "distance from truth-value".
  - $I(b, w_1) = d(b(P), 1) + d(b(\neg P), 0)$.
  - $I(b, w_2) = d(b(P), 0) + d(b(\neg P), 1)$.
- The Brier score uses the square-of-the-difference measure:
  - $\mathfrak{s}(x, y) = (x - y)^2$.
- And, this choice supports de Finetti/Joyce arguments.
- Interestingly, there are many other (plausible) measures of "distance" that do *not* support de Finetti/Joyce arguments.
- For instance, if we adopt:
  - $\mathfrak{a}(x, y) = \sqrt{(x - y)^2} = |x - y|$.

  as our measure of "distance", then the resulting inaccuracy measure does *not* satisfy the de Finetti/Joyce theorem [18].
- Next: some arguments in favor of (measures like) $\mathfrak{s}$ *vs* $\mathfrak{a}$.

- One difference between $\mathfrak{s}$ and $\mathfrak{a}$: $\mathfrak{s}$ is "Proper." A scoring rule is said to be *proper* just in case it entails that S's current b has — by its own lights — minimum *expected* inaccuracy.
  - Joyce does *not* appeal to "propriety", since this involves appealing to *expectation*, which he thinks (rightly) *builds-in* a lot of probabilism (recall our objection to the DBA).
- Another difference: $\mathfrak{a}$ is "*probabilistically inadmissible*." That is, $\mathfrak{a}$ entails that *some probabilistic credence functions are accuracy-dominated* (of course, $\mathfrak{s}$ does not).
  - Joyce: the *Principal Principle* undergirds Pr-admissibility.
- I agree with Joyce about "propriety". Appeals to expectation are (a) question-begging (as in the DBA), and (b) they defeat the purpose of introducing Joyce's (new) cognitive act of "estimation." I'll return to this point later in the lectures.
- Joyce's use of (PP) presupposes: (a) chances are Pr's [11], (b) all coherent credence functions *could be* chance functions [10], and (c) (PP) interacts benignly with Brier-dominance [5].

- Suppose S adopts the Brier Score as their I-measure, and that S's b is non-probabilistic. Then, there are alternative (coherent) credence functions b' that accuracy-dominate b.
- Intuitively, these b' functions should "look epistemically better" (in a precise sense) than S's current credences b.
- But, a possible "evidentialist" worry remains.
- Consider a very simple toy agent S with one sentence P in their language. And, suppose S's credence function assigns $b(P) = 0.2$ and $b(\neg P) = 0.7$. So, S's b is *non*-probabilistic.
- It follows from de Finetti/Joyce's theorems that there is *a specific set of* credence functions b' that *Brier-dominate* b.
- It seems that this alternative credence function b' should *inevitably* "look epistemically better" to S than her current credence function b. Our worry is that this *needn't* be so.
- Consider the following (toy) illustration of our worry.

- The red dot in the figure is S's credence function b. The shaded region depicts the functions b' that *Brier-dominate* b. [The black dot at $\langle 0.2, 0.8 \rangle$ depicts the *only probabilistic* credence function that is compatible with $b(P) = 0.2$.]



- Suppose that S *has good reason* to assign $b(P) = 0.2$ (*i.e.*, S's total evidence E *supports* $b(P) = 0.2$).
- Here, *all* the Brier-dominating functions b' are s.t. $b'(p) \neq 0.2$.
- So, *all the Brier-dominating functions b' may be "ruled-out" by S's evidence.*
- Then, b' *needn't* "look better" than b.

☞ This is analogous to what happens with (bad) preface cases. Evidential norms can sometimes "trump" coherence norms.

- Next: accuracy, language-dependence, and de Finetti/Joyce.

- Suppose we have two *numerical* quantities ϕ and ψ. These might be, for instance, the velocities (in some common units) of two objects, at some time (or some other physical property, like temperature, of two objects at a time).
- Suppose further that we have two sets of (false) predictions concerning the values of ϕ and ψ, which are entailed by two (false) competing hypotheses H_1 and H_2.
- Finally, let's use "T" to denote *the truth* about the values of ϕ and ψ (or, if you prefer, the true hypothesis about their values) — in our standard units. And, let H_1, H_2, and T be:

	ϕ	ψ	α	β
H_1	0.150	1.225	0.925	2.000
H_2	0.100	1.000	0.800	1.700
T	0.000	1.000	1.000	2.000

- It seems clear that the predictions of H_2 are "closer to the truth T about ϕ and ψ" than the predictions of H_1 are.

- However, as Popper [21, Appendix 2] showed (using a recipe invented by David Miller [19]), there exist quantities α and β (as in the table) satisfying both of the following conditions.
  1. α and β are symmetrically inter-definable with respect to ϕ and ψ in the following (linear) way:

$$\alpha = \psi - 2\phi \qquad \beta = 2\psi - 3\phi$$
$$\phi = \beta - 2\alpha \qquad \psi = 2\beta - 3\alpha$$

  2. The values for α and β entailed by H_2 are strictly "farther from the truth T about α and β" than those entailed by H_1.
- As Miller [19] explains (see [20, Chapter 11] for a recent survey), there is a much more general result in the vicinity.
- For *any* pair of false theories H_1 and H_2 about ϕ and ψ, *many* relations of "closer to the truth" can be *reversed* by looking at what the estimates provided by H_1 and H_2 for ϕ and ψ entail about quantities α and β, which are given by:

$$\alpha = \mathsf{a}\psi + \mathsf{b}\phi \qquad \beta = \mathsf{c}\psi + \mathsf{d}\phi$$
$$\phi = \mathsf{a}\beta + \mathsf{b}\alpha \qquad \psi = \mathsf{c}\beta + \mathsf{d}\alpha$$

- This is Miller's 2^{nd} (Quantitative) LDP. It *is* a (potential) threat.

- The easiest way to see the (potential) threat posed by Miller's 2^{nd} LD problem is to consider a simple numerical example of a toy agent S of the type we've been discussing.

	ϕ	ψ
b	$\frac{1}{2}$	$\frac{1}{4}$

- Here, S's "estimates" (b) of ϕ and ψ do not sum to one. As a result, there exist alternative "estimates" b' of ϕ/ψ that Brier-dominate b in both of the salient possible worlds.

	ϕ	ψ
b	$\frac{1}{2}$	$\frac{1}{4}$
b'	$\frac{5}{8}$	$\frac{3}{8}$
w_1	0	1
w_2	1	0

- b' is the *Euclidean-closest* (to b) set of "estimates" of ϕ and ψ that Brier-dominate b — with respect to ϕ/ψ "estimation".

- Does Joycean "numerical estimation" face a (*prima facie*) problem analogous to Miller's second LD problem? Yes!

	ϕ	ψ	α	β
b	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{9}{16}$	$\frac{3}{16}$
b'	$\frac{5}{8}$	$\frac{3}{8}$	$\frac{3}{4}$	$\frac{1}{4}$
w_1	0	1	$\frac{7}{16}$	$\frac{9}{16}$
w_2	1	0	$\frac{9}{16}$	$\frac{7}{16}$

- Here, we have numerical quantities α and β, such that:
  (i) α/β are symmetrically inter-definable w.r.t ϕ/ψ, *via* ($f^\star$):

$$\alpha = \frac{1}{2}\phi + \frac{1}{2}\psi + \frac{1}{16}\left(\frac{\phi+\psi}{\phi-\psi}\right) \qquad \beta = \frac{1}{2}\phi + \frac{1}{2}\psi - \frac{1}{16}\left(\frac{\phi+\psi}{\phi-\psi}\right)$$

$$\phi = \frac{1}{2}\alpha + \frac{1}{2}\beta + \frac{1}{16}\left(\frac{\alpha+\beta}{\alpha-\beta}\right) \qquad \psi = \frac{1}{2}\alpha + \frac{1}{2}\beta - \frac{1}{16}\left(\frac{\alpha+\beta}{\alpha-\beta}\right)$$

  (ii) The "estimates" of α/β entailed (*via* $f^\star$) by b *Brier dominate* the "estimates" of α/β entailed by b' (*via* $f^\star$).

☞ So, we have a Miller-style *reversal* of Brier-domination here!

- Here is a *more general theorem* about our toy agent S.

  Theorem. For *any* coherent b' that Brier-dominates S's credence function b with respect to ϕ and ψ, there exist quantities α and β that are symmetrically inter-definable with respect to ϕ and ψ, *via* the transformation $f^\star$ above, such that b *Brier-dominates* b' with respect to α and β.

- It is also noteworthy that the *true* values of α and β "behave like truth-values", in the sense that (a) the true value of α (β) in w_1 (w_2) is identical to the true value of β (α) in w_2 (w_1), and (b) the true values of α and β always *sum to one*.
- Indeed, this transformation $f^\star$ is guaranteed to *preserve coherence* of *all* dominating b''s, *and* the "truth-vectors".
- It is not a coincidence that $f^\star$ is *non*-linear. It can be shown that *no linear* f can play the role that $f^\star$ plays here. There are several reasons for this (some of which I'll mention below).
- Next, two possible responses to this (*prima facie*) threat.

- **Response #1: Naturalness.** The first response is to argue that the ϕ/ψ "numerical representation of the truth-values of $P/\neg P$" is somehow "more natural" than the α/β "numerical representation of the truth-values of $P/\neg P$".
- I'm not sure how such a "naturalness argument" would go.
- After all, the truth-values of $P/\neg P$ are *disanalogous* to *numerical* physical quantities like velocity or temperature.
- In the case of temperature, for instance, the *numerical level* of description is (arguably) the most fundamental/scientific.
- But, in the case of truth-values, their theoretical role seems to be given *fundamentally* at the level of their *algebraic* and *meta-logical* (*viz.*, *logico-structural*) properties.
- The "numerical properties" of the truth-values (*if there be such*) do not seem to be theoretically fundamental. So, it's not clear to me how probative "naturalness" is here.
- I think there is a more promising line of response…

- **Response #2: Disanalogies between "Estimation" & Prediction**.
- The second line of response (which I prefer) is to argue that there are crucial disanalogies between "estimation" (in Joyce's sense) and prediction (in Miller's sense).
- Let ⌜$\mathcal{E}(x,y) = \langle p,q\rangle$⌝ express the claim that ⌜S is committed to the values $\langle p,q\rangle$ as their "estimates" of the quantities $\langle x,y\rangle$⌝. Our "reversal argument" presupposes the following (as applied to our toy, numerical S, above):

  (†) If $\mathcal{E}(\phi,\psi) = \langle p,q\rangle$, then $\mathcal{E}(\alpha,\beta) = f^\star(p,q)$, where $f^\star$ is the symmetric inter-translation function that maps values of $\langle\phi,\psi\rangle$ to/from values of $\langle\alpha,\beta\rangle$ in our Theorem.

- Ultimately, what Joyce needs to argue is that (†) is *false*.
- In order to do this, Joyce needs to tell us more about what "estimation" is. Ideally, he needs to give us a *theory* of $\mathcal{E}$.
- Unfortunately, what Joyce *explicitly says* about $\mathcal{E}$ is insufficient to explain why (†) should come out false.

- The only *explicit* commitments Joyce has re $\mathcal{E}$ seem to be:

  (1) Estimates are *not guesses.* Joyce [13, 587] explicitly distinguishes estimation and guessing.
  - This doesn't help me assess (†), so I won't discuss it.

  (2) Estimates are *not expectations.* Joyce [13, 587–8] explicitly *disavows* thinking of estimates as expectations. Indeed, Joyce (rightly) thinks it would be *question-begging* to think of "estimation" as expectation (*builds-in* too much probabilistic structure). Also, this is *not* a new idea [23].
  - If estimates *were* expectations, then this would entail that (†) is *false*, since this would rule-out *all non-linear* transformation functions. [A reason to *like* expectation?]

  (3) Estimates are *not assertions that* the values of the parameters *are such-and-so.* This is clear, since it's *not* a good idea to assert things that you know (*a priori*) *must be false.* And, this happens whenever you offer "estimates" of "numerical correlates of truth-values" that are *non-extreme.*
  - If estimates *were* assertions, then this would entail that (†) is *true* — assuming a truth/closure norm for assertions.

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Stage-Setting (background) Qualitative (belief) Comparative (confidence: ≻, ∼, ⪰) Quantitative (credence) Refs

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