

Comparativism and the Measurement of Belief

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Abstract

According to *comparativism*, degrees of belief are reducible to a system of purely ordinal comparisons of relative confidence. (For example, being more confident that P than that Q , or being equally confident that P and that Q .) In this paper, I raise several general challenges for comparativism, relating to (i) its capacity to illuminate apparently meaningful claims regarding intervals and ratios of strengths of belief, (ii) its capacity to draw enough intuitively meaningful and theoretically relevant distinctions between doxastic states, and (iii) its capacity to handle common instances of irrationality.

1 Introduction

Meet Sally. Like the rest of us, Sally has beliefs, broadly construed: there's some way she takes the world to be that's generally responsive to her evidence, and which along with her desires guides her behaviour. This paper concerns what Sally's beliefs might be like at the most fundamental level, and the relationship between the different types of beliefs she might be taken to have.

To get the ball rolling, we'll assume Sally has at least two kinds of belief: partial and comparative. With respect to the former, Sally is, for instance, *quite certain* there's an external world, *uncertain* about the consequences of global warming, but *doubtful* they'll be good. These are attitudes directed towards individual propositions, each coming with some (possibly vague or imprecise) strength that can at least sometimes be represented numerically. Her comparative beliefs, on the other hand, relate pairs of propositions, and these do not come in degrees. Sally is, for example, *more confident* that she'll find good coffee in Melbourne than she will in Sydney, and *just as confident* that she'll find good coffee in Sydney as that she'll win the lottery next week. If there's a sense in which Sally also has 'all-or-nothing' beliefs, then we'll assume these derive in one way or another from the facts about her partial and/or comparative beliefs.

Taking that for granted, it's natural to wonder about the relationship between Sally's partial and comparative beliefs. It's clear enough they're closely connected. If Sally is $x\%$ confident that P and $y\%$ confident that Q , then she's more confident that P than she is that Q just in case $x > y$. Likewise, if Sally just as confident that P as she is that Q , then she's $x\%$ confident that P just in case she's $x\%$ confident that Q . Moreover, these conditionals have a certain feel of apriority about them, so it's reasonable to think they're underwritten by some interesting conceptual or metaphysical connection.

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According to *comparativism*, the facts about Sally’s partial beliefs supervene on, and indeed hold in virtue of, the facts about her comparative beliefs.¹ Comparativism comes in a range of shapes and sizes, with roots going back to the writings of Keynes (1921) and de Finetti (1931). Works favourable to comparativism include (Koopman 1940), (Savage 1954), (Fine 1973), (Stefánsson 2017; 2018), and (DiBella 2018). Closely related ideas have also been defended in (Joyce 2010; 2015), and (Hawthorne 2016). We’ll talk more about the details in a moment; but the general thought is that an agent like Sally’s system of partial beliefs can be reduced to a ranking of propositions by relative confidence, with numerical strengths of belief thus serving as a ‘theoretical tool’ for representing the relative positions of propositions within that ranking.

In §3, I’ll argue that comparativism presently lacks any adequate account of the measurement of the strengths of our beliefs. Objections and responses to my arguments will be considered in §4; and in §5, I will consider the related idea that the facts about partial beliefs are determined by the facts about comparative beliefs *plus* some further qualitative mental state (e.g. preferences, or qualitative judgements about evidential relationships). But before we discuss problems with comparativism, we should get a clearer idea of what comparativism is.

2 Probabilistic Comparativism

There are two essential components to any comparativist’s theory. The first component, to put it roughly, is an explanation of *where the numbers come from*. As B.O. Koopman once expressed the idea,

... all the axiomatic treatments of intuitive probability current in the literature take as their starting point a number (usually between 0 and 1) corresponding to the ‘degree of rational belief’ or ‘credibility’ of the eventuality in question. Now we hold that such a number is in no wise a self-evident concomitant with or expression of the primordial intuition of probability, but rather a mathematical construct derived from [comparative beliefs] under very special conditions... (1940, p. 269)

The second component is an explanation of *where cardinality comes from*. The numbers we use to represent the strengths of belief seem to encode more-than-merely-ordinal (read: cardinal) information, and this requires explanation. Why, for instance, does it apparently make sense to say such things as Sally believes P much more than Q , or twice as much as Q , or a fraction as much as Q ?

In §2.1, I’ll say more about the first component; in §2.2–§2.3, more about the second. I will not try to describe every possible variety of comparativism on the market. Instead, I’ll focus my exposition on a relatively straightforward and common version, *probabilistic comparativism*.

2.1 Where the numbers come from

I like to think that Sally accepts *probabilism*, as well she should. Supposing she does, what is it then that she accepts? According to the usual gloss, probabilism

¹ To emphasise: comparativism, as I’m understanding it, is *not* the idea that partial beliefs depend on *some comparative thing or other*. Rather, it’s a specific claim about the relationship between partial and comparative beliefs. Comparativism can be—and often is—divorced from the more general thesis that beliefs depend on something comparative (e.g., preferences).

says that her beliefs ought to conform to the axioms of the probability calculus. But that can't be quite right: the axioms of the probability calculus are constraints on real-valued functions, and whatever Sally's beliefs are, they aren't literally functions between propositions and real numbers. Obviously, there's no real problem here—not yet, anyway. The usual gloss was only meant to be elliptical. The intention is that Sally's beliefs ought to be such that they're *representable* by a probability function.

We can break the claim down into two parts. The first is a constraint on the set of propositions (call it \mathcal{B}) regarding which Sally has beliefs—specifically, it should have a 'Boolean' structure. A nice and general way to describe that structure is to suppose that propositions are (or can be modelled by) sets of possible worlds.² If we let Ω henceforth denote an appropriately chosen set of such worlds, then probabilists will typically require:

BOOLEAN. \mathcal{B} is non-empty, and if P and Q are in \mathcal{B} , then $\Omega \setminus P$ (i.e., $\neg P$), $P \cup Q$, and $P \cap Q$ are also in \mathcal{B}

The second (and more interesting) part is a constraint on the beliefs themselves. Specifically, Sally's beliefs ought to be such as can be represented by a function $pr : \mathcal{B} \mapsto \mathbb{R}$ satisfying:

NORMALISATION. $pr(\Omega) = 1$

NON-NEGATIVITY. $pr(P) \geq 0$

ADDITIVITY. If $P \cap Q = \emptyset$, then $pr(P \cup Q) = pr(P) + pr(Q)$

But what would Sally's beliefs have to be like, exactly, so as to be probabilistically representable?

Probabilistic comparativism offers an answer. It's not the only possible answer, but it's not an intrinsically implausible one either, and historically it has been extremely influential. The probabilistic comparativist says that Sally's partial beliefs are nothing over and above her comparative beliefs. Consequently, a probability function *represents* Sally's beliefs just when the ordering of the numerical values assigned to the propositions she believes corresponds exactly to the ordering induced over those propositions by her comparative beliefs.

Assume henceforth that \mathcal{B} is Boolean. Next, assume that Sally's full suite of comparative beliefs can be faithfully represented with a single binary relation \succsim defined over \mathcal{B} , where

$$P \succsim Q \text{ iff Sally has at least as much confidence in } P \text{ as in } Q,$$

and where \succ and \sim stand for the *more confident* and *equally confident* comparatives respectively,

$$P \succ Q \text{ iff } (P \succsim Q) \& \neg(Q \succsim P), \quad P \sim Q \text{ iff } (P \succsim Q) \& (Q \succsim P)$$

² By 'possible worlds', I mean nothing more nor less than that the worlds are complete and closed under classical logic. I prefer to think of Ω as a set of 'scenarios' in the sense of (Chalmers 2011), so $\omega \in \Omega$ iff ω cannot be ruled out apriori. You might prefer to model propositions using impossible worlds, sentences in some formal language, or something else. Some of my arguments will depend on how Ω is understood, so I'll say more about this in §4.1.

We'll call \succsim Sally's *confidence ranking*. Given this, say that a real-valued function f (i.e., not necessarily a probability function) *agrees with* \succsim just in case

$$P \succsim Q \text{ iff } f(P) \geq f(Q)$$

Thus, if we suppose that Sally's beliefs ultimately reduce to her comparative beliefs, the notion of *agreement* gives us an unambiguous sense in which those beliefs can be represented by a probability function. It also gives clear meaning to Koopman's assertion above that the numbers used to represent strengths of belief are 'mathematical constructs' designed to help us reason about what, according to comparativism, is ultimately a system of purely ordinal judgements of relative confidence.

Supposing we accept all this, then an important benefit of probabilistic comparativism is that we're able to supply an alternative and unambiguous formulation of exactly what it is that probabilism requires. Since (de Finetti 1931), we've known that probabilistic agreement requires Sally's confidence ranking to satisfy (for all $P, Q, R \in \mathcal{B}$):

WEAK ORDER. \succsim is transitive and complete

NON-TRIVIALITY. $\Omega \succ \emptyset$

MINIMALITY. $P \succ \emptyset$

MONOTONICITY. If $R \cap (P \cup Q) = \emptyset$, then $P \succsim Q$ iff $(P \cup R) \succsim (Q \cup R)$

We need something a little stronger than MONOTONICITY if we want necessary *and* sufficient conditions for probabilistic agreement (Kraft et al. 1959; Scott 1964), and we need something even stronger still if we want there to be only *one* probability function that agrees with the confidence ranking (see Fishburn 1986). But the details of those further (and more complicated) conditions need not concern us—what's listed is more than enough to get a handle on the kind of shape a confidence ranking has to have to be probabilistically representable.³

2.2 Where cardinality comes from: the transformation argument

You might worry that something's missing from the picture I've described: probabilistic comparativism still owes us an explanation of *where cardinality comes from*. Imagine for example that Sally is about to roll an ordinary six-sided die. Now consider:

ORDINAL. Sally is *more* confident of rolling ≥ 2 than of rolling a 1

INTERVAL. Sally is *much more* confident of rolling ≥ 2 than of rolling a 1

RATIO. Sally is *five times* as confident of rolling ≥ 2 than of rolling a 1

The apparent meaningfulness of claims like INTERVAL and/or RATIO is sometimes raised as a challenge for comparativism (e.g. Meacham and Weisberg 2011, p. 659), and it will be the focus of my critical discussion in §3 as well.

³ One important way in which comparativists might diverge from the view I've been describing is worth flagging here. What we might call *quarternary probabilistic comparativism* replaces the *binary* confidence rankings with a *quarternary* relation: $P, Q \succsim R, S$ iff Sally has at least as much confidence in P given R as she does in R given S . Conditions similar to WEAK ORDER, NON-TRIVIALITY, etc., are then used to ensure that conditional probabilities agree with \succsim , in the sense that $P, Q \succsim R, S$ iff $pr(P|Q) \geq pr(R|S)$. See, e.g., (Koopman 1940), (Fine 1973, pp. 28–32), (DiBella 2018). Each of the main points that I raise below with regards to binary comparativism can also be raised for quarternary comparativism, *mutatis mutandis*. To keep the discussion to a reasonable length, however, I won't spell out the details.

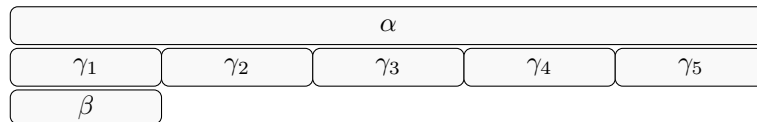
To get an initial sense of why one might think there's a problem here, suppose we have a probability function, pr , which agrees with \succsim , such that

$$pr(\text{rolling } \geq 2) = 5/6, \quad pr(\text{rolling } 1) = 1/6$$

So pr also seems to appropriately represent the claims INTERVAL and RATIO. However, any order-preserving transformation of pr will also agree with \succsim , and only information that's common to each of these functions—the ordering—can be taken to represent something doxastically 'real'. We know that neither ratios nor ratios of intervals need be preserved across arbitrary order-preserving transformations. Therefore, it seems, only claims like ORDINAL can have genuine doxastic meaning, whereas claims like INTERVAL and RATIO seem to depend on an arbitrary choice of scale. Call this the *transformation argument*.

The transformation argument is flawed. Comparativists have long-standing explanatory strategy for dealing with cardinal information that's entirely consistent with their theory, one based on an analogy with the measurement of basic extensive quantities like length and mass, and the transformation argument does nothing to undermine it. (This explanation can be found in numerous locations, though it's discussed in particular depth in Fine 1973, pp. 68ff.) So, in the remainder of this section I'll first explain why the transformation argument is flawed by showing why a parallel argument fails in the case of length; following that I'll describe the standard comparativist's explanation of cardinality.

We'll focus on defining ratios of lengths, since once they're defined it's straightforward to define ratios of intervals. Hence, consider 'α is five times longer than β'. Our task is to provide truth conditions for this without presupposing any cardinal information. Towards that end, suppose we have at hand five further objects, γ_1 – γ_5 , each of which is exactly as long as β , and which share no parts with one another. You might refer to these as β 's *length-duplicates*. Glossing over a few complications, it's plausible that 'α is five times longer than β' would be true if, and only if, were you to take these five length-duplicates of β and lay them end-to-end then the result would be as long as α :



This suggests that it's possible to give at least rough-and-ready truth conditions for a claim about ratios of lengths in relational terms: 'α is five times longer than β' just means *α is as long as 5 disjoint length-duplicates of β laid end-to-end*.

That's the general idea, and simple variations on the same strategy can be used to provide truth conditions for any other rational ratio comparison as well. (For example, what we've said already entails that γ_1 and γ_2 laid end-to-end will be $2/5$ as long as α , and $2/3$ as long as γ_3 , γ_4 , and γ_5 laid end-to-end.) With a bit of mathematical trickery we can also extend the idea to give meaningful ratio comparisons for the non-rational reals. But regardless of whether we generalise it to all real ratio comparisons or just stick with rational ratios, we'll need to make two general assumptions.

The first is existential: if we're going to characterise ratios of lengths between arbitrary pairs of objects, then we're going to need that there are enough

disjoint objects of varying lengths lying about. That's the rough version; we don't need to worry too much about what the precise version looks like for this discussion. What matters, simply, is that *the more ratio comparisons you want to characterise, the more objects of varying lengths you're going to need*. And that's reasonable enough, given there's quite a few objects in the universe for us to play with. The second assumption concerns the structure of the ordinal length relations themselves, and how those relations interact with the physical operation of laying objects end-to-end. In short, the *is at least as long as* relation should behave like \geq , and the *laid end-to-end* operation should relate to the *is at least as long as* relation in the same way $+$ relates to \geq .

We can state this requirement more exactly. Let \succ^* designate the *is at least as long as* relation, with \sim^* and \succ^* defined in the usual way. Then, let $\alpha \oplus \beta$ designate some object that's as long as two disjoint γ_1, γ_2 laid end-to-end, where $\gamma_1 \sim^* \alpha$ and $\gamma_2 \sim^* \beta$. Furthermore, say that α has *zero length* just in case for all β , $\alpha \oplus \beta \sim^* \beta$. Then, we need that for all α, β, γ ,⁴

- WEAK ORDER. \succ^* is transitive and complete
- POSITIVITY. $\alpha \oplus \beta \succ^* \alpha$ whenever β has non-zero length
- WEAK COMMUTATIVITY. $(\alpha \oplus \beta) \sim^* (\beta \oplus \alpha)$
- WEAK ASSOCIATIVITY. $\alpha \oplus (\beta \oplus \gamma) \sim^* (\alpha \oplus \beta) \oplus \gamma$
- MONOTONICITY. $\alpha \succ^* \beta$ iff $\alpha \oplus \gamma \succ^* \beta \oplus \gamma$
- ARCHIMEDEAN. Nothing is infinitely longer than anything else

Each of these is entirely plausible, and one of the key results in the theory of measurement is that if they're all satisfied, *and* the appropriate existential conditions are also satisfied, then there's a way f of assigning real numbers to objects such that for all such objects α, β ,

- (i) $f(\alpha) \geq 0$
- (ii) $f(\alpha) \geq f(\beta)$ iff $\alpha \succ^* \beta$
- (iii) $f(\alpha \oplus \beta) = f(\alpha) + f(\beta)$

Call any f satisfying these three properties an *additive measure* of length.

Now it's important to be clear how the existence of an additive measure relates to the transformation argument. It's well known that additive measures are unique up to positive similarity transformations—that is, if f is an additive measure then so too is f^* just in case

$$f^*(\alpha) = r \cdot f(\alpha), \quad r > 0$$

It's also well known that positive similarity transformations preserve ratios. Consequently, if length is measured using any additive f , then $f(\alpha) = n \cdot f(\beta)$ iff α is as long as n of β 's length-duplicates laid end-to-end. But none of *these* facts yet explain why ' α is n times longer than β ' is meaningful, simply because there's nothing about the nature of length *itself* that forces the use of an additive measure (cf. Krantz et al 1971, pp. 11–12, 99ff; Luce and Narens 1984).

For concreteness, where \mathcal{O} is the set of objects, say that f is an *accurate* measure of length just in case it maps \mathcal{O} onto some $\mathbb{R}^* \subseteq \mathbb{R}$ such that \succ^* is

⁴ The ARCHIMEDEAN condition is here stated informally. See (Krantz et al. 1971, pp. 71ff) for the precise version. A statement of the ARCHIMEDEAN condition for the additive measurement of confidence rankings can be found in (Chateauneuf and Jaffray 1984, p. 193).

represented by \geq and \oplus is represented by some operation $\circ : (\mathbb{R}^* \times \mathbb{R}^*) \mapsto \mathbb{R}^*$. The additive measures are a special class of accurate measures—those in which \mathcal{O} is mapped onto a (subset of) the non-negative reals $\mathbb{R}_{\geq 0}$ and \oplus is represented with $+$. But there’s an infinite variety of equally accurate non-additive measures that we could have chosen to use instead. Consider, for example, a *multiplicative measure* f' , whereby:

- (i') $f'(\alpha) \geq 1$
- (ii') $f'(\alpha) \geq f'(\beta)$ iff $\alpha \succ^* \beta$
- (iii') $f'(\alpha \oplus \beta) = f'(\alpha) \times f'(\beta)$

The two numerical systems $\langle \mathbb{R}_{\geq 0}, \geq, + \rangle$ and $\langle \mathbb{R}_{\geq 1}, \geq, \times \rangle$ are isomorphic to one another, so a multiplicative measure will be accurate whenever an additive measure is. But multiplicative measures are *not* unique up to positive similarity transformations, and α is as long as n of β 's length-duplicates laid end-to-end iff $f'(\alpha) = f'(\beta)^n$. More generally, given that \circ can be any operation on some $\mathbb{R}^* \subseteq \mathbb{R}$, every order-preserving transformation f'' of any accurate measure f will itself count as accurate relative to *some* \mathbb{R}^* and \circ . In the vast majority of these cases the operation \circ will be extremely ‘unnatural’ by comparison to $+$ or \times , and only the most masochistic of us would want represent \oplus using it—but that doesn’t change the fact that we could *in principle* do so if we were so inclined.

There’s no fact of the matter as to which measure f, f', f'', \dots , is the *correct* one to use, only a choice borne of convention and convenience. So let’s say that a measure is *adequate* just in case it’s accurate *and* it represents \oplus in a convenient way. Additive measures of length are adequate, but then so too are multiplicative measures (albeit slightly less so). The measure f'' generated by an arbitrary order-preserving transformation of f (or of f') will in most cases be highly inadequate, though it’ll be no less accurate because of this.

We can now finally say exactly where the transformation argument is going wrong. The error lies in thinking that what we’ve been *calling* ‘ratio information’ in our partial beliefs depends in any interesting way on the *numbers* we use to represent the strengths of those beliefs. This is a mistake, as the example with length shows. There’s nothing stopping us from using an additive measure f or a multiplicative measure f' to represent length, but

$$f(\alpha) = n \cdot f(\beta) \text{ iff } f'(\alpha) = f'(\beta)^n$$

That is: there’s no deep connection between what we’ve determined to be the *truth conditions* for claims about ‘ratios’ of lengths and the *specific numerical relationships* that hold between the numbers assigned by different but equally accurate ways of measuring length. We call it ‘ratio information’ because we’ve a historical preference for additive measures, but had physics developed slightly differently we’d be calling it ‘power information’.

Ultimately, it’s the *structure* of the underlying system of length relations that explains why ‘ $f(\alpha) = n \cdot f(\beta)$ ’ has real-world meaning. And it’s that underlying structure which is established by the *existence* of an additive measure—the matter of what numerical relationships are preserved across additive measures is besides the point. By the same token, if comparativism is right, then it’s the confidence ranking *itself* that contains cardinal information if anything does. How we choose to represent that information numerically is irrelevant.

2.3 Where cardinality comes from: the SCEC

Comparativism isn't out of the woods yet! To show the same style of explanation of cardinal information can *in principle* be made to work for comparative beliefs, it first needs to be shown that there's some operation on propositions (*qua* relata of \succsim) that can serve as the doxastic analogue of addition in the same way that \oplus serves as the analogue of addition for length.

Such an operation is easy to find, *if* Sally's confidence ranking agrees with a probability function—for then the restriction of the union operation to disjoint propositions will behave like addition with respect to that ranking. This is an immediate consequence of ADDITIVITY. Consequently, comparativists have typically pointed towards the *union of disjoint propositions* as their proposed qualitative analogue of addition (e.g. Fine 1973, p. 68; Krantz et al. 1971, p. 200; Stefánsson 2018; DiBella 2018; though cf. Elliott forthcoming, §3.2, for a more general operation). And indeed, if \succsim agrees with a probability function, then for all disjoint $P, Q, R \in \mathcal{B}$,

WEAK ORDER. \succsim is transitive and complete

POSITIVITY. $(P \cup Q) \succ Q$ whenever $P \succ \emptyset$

WEAK COMMUTATIVITY. $(P \cup Q) \sim (Q \cup P)$

WEAK ASSOCIATIVITY. $(P \cup (Q \cup R)) \sim ((P \cup Q) \cup R)$

MONOTONICITY. $P \succsim Q$ iff $(P \cup R) \succsim (Q \cup R)$

ARCHIMEDEAN. Sally is not infinitely more confident that P than that Q

So if Sally's confidence ranking agrees with some probability function pr , then 'Sally believes P n times as much as Q ' whenever $P \succsim (R_1 \cup \dots \cup R_n)$, where the R_1, \dots, R_n are disjoint and $R_1 \sim \dots \sim R_n \sim Q$. Here the R_1, \dots, R_n are Q 's *confidence-duplicates*, and it follows that $pr(P) = n \cdot pr(Q)$. Hence, \succsim has an 'additive' structure with respect to the union of disjoint propositions, which can be *adequately* represented using the additive measure pr .

We have thus arrived at what I'll call the *standard comparativist explanation of cardinality*, or SCEC. If the SCEC is on the right track, then there's a close analogy between the measurement of partial belief and the measurement of length, and this undoubtedly lends *some* plausibility to comparativism as a whole (cf. Fine 1976; 1973, pp. 15–16; Stefánsson 2017; 2018). After all, there's something obviously compelling about explaining ratios of strengths of belief using a method that's consonant with how such things are usually described for other basic physical quantities like length.

But we should be careful not to overstate what the SCEC establishes. Nothing so far suffices to show that if \succsim agrees with a probability function, then Sally has, say, at least twice as much confidence in P as in Q if *or* only if $P \succsim (R_1 \cup R_2)$, where R_1 and R_2 are disjoint confidence-duplicates of Q . Still less have we been given any account of what to say when \succsim *doesn't* agree with a probability function. Rather, the SCEC is a 'how possibly' story—an account of how comparativism *might* explain cardinality given some very strong assumptions about the shape of \succsim and its relationship with \cup . It's enough to establish that comparativism might *in principle* be able to account for cardinality, but by no means does it establish that the account is *correct*.

It is also important to note that there's more than one way to explain how partial beliefs might come to contain meaningful cardinal information. Denying the SCEC does not commit one to the manifestly absurd idea that there

is no qualitative explanation of where the numbers come from and how they manage to encode cardinal information. Comparativists don't have a monopoly on scientifically respectable explanations of cardinal information! Other well-established methods for understanding quantitative ratio information do not follow the SCEC's pattern—i.e., they do not require us to locate some operation (like \cup) which shares structural features with $+$ in relation to \succsim .

For example, in additive conjoint measurement (Luce and Tukey 1964; Krantz et al. 1971), cardinal information for a given quantity q can be determined by reference to how that quantity interacts with another quantity q' to produce an ordering \succsim^* with respect to some further quantity q^* distinct from either q or q' . No real-world analogue of addition is needed: structural properties of the \succsim^* -ordering are used to establish that intervals and/or ratios have meaning for q (and for q'), given background theoretical assumptions about how q and q' interact with q^* . Kahneman and Tversky (1979) have suggested that conjoint measurement theory can be used to simultaneously explain the measurement of both partial beliefs and utilities in terms of how they interact to produce preferences over choices, and a representation theorem which simultaneously builds probabilities and utilities out of preferences (e.g., Savage 1954) can help to provide the foundations for such a view.

A distinct but closely related strategy explains the measurement of partial belief on the pattern of dimensionless quantities. Unlike conjoint measurement, where the cardinal properties of two quantities q and q' are defined by relation to a third quantity q^* , most dimensionless quantities of interest are defined in terms of ratios of differences in a single quantity q . Consider Mach numbers. A Mach number is not a unit of speed; rather, it's a ratio that represents the speed of an object travelling through a medium *relative to* the speed of sound in that medium. If we let s be any measure of speed on at least an interval scale, then relative to a given medium we can define an object's Mach number:

$$\text{Mach}(\text{object}) = \frac{s(\text{object}) - s(\text{stationary})}{s(\text{sound}) - s(\text{stationary})}$$

Along these lines, I prefer an approach that originates with Ramsey (1931) and also centrally involves preferences: beliefs are causally tied to preferences in such a way that cardinal information can be extracted from their relationship. On a very simplified version of this approach, the degree of belief Sally has towards P is a ratio that represents the impact the belief has on the utility of a gamble conditional on P *relative to* that gamble's best and worst outcomes—where u measures the Sally's preferences measured on an interval scale, Sally is indifferent between Q and $\langle R \text{ if } P, S \text{ otherwise} \rangle$, and she prefers R to S , then

$$\text{pr}(P) = \frac{u(Q) - u(S)}{u(R) - u(S)}$$

Note that saying this has no implications regarding the relative fundamentality of beliefs, utilities, and/or preferences. The claim is *not* that facts about partial beliefs are nothing over and above the facts about preferences; nor is the claim that beliefs have no other functional roles other than those in relation to preferences. Instead, the claim need only be that the meaningfulness of ratios and intervals of strengths of belief is *explicable* by reference to that part of the typical causal role that beliefs play in relation to preferences.

Comparativists will no doubt have objections to these alternatives. I’ve defended both against many of these objections in other works, and I’m not going to do it again here (see Elliott 2017; forthcoming). This paper is about problems for comparativism, so let’s get to them.

3 The Case Against the SCEC

The overarching goal for the following discussion will be to show that the SCEC isn’t quite as illuminating as we might have hoped. Broadly speaking, I’ll argue for this in two ways. First, there appear to be conceptually possible and theoretically-relevant distinct states of partial belief, with corresponding and apparently meaningful differences in cardinal information, which correspond to the very same probabilistically representable confidence ranking (§3.1–§3.4). Hence, the SCEC cannot explain the differences in cardinal information—and more generally comparative beliefs do not suffice to determine the facts about partial beliefs, which alone rules out any strict version of comparativism. Second, there appear to be conceptually possible and theoretically-relevant distinct states of partial belief that correspond to no probabilistically representable confidence ranking whatsoever (§3.5). So, again, the SCEC cannot explain the cardinal information present in those systems of belief.

3.1 The SCEC and almost omniscience

Suppose that Sally is almost omniscient:

Example 1. Sally is ideally rational, and her comparative beliefs satisfy all the requirements for agreement with a probability function. Furthermore, she is almost omniscient, in the sense that she’s narrowed down which possible world she inhabits to exactly two possibilities: ω_1 and ω_2 . While Sally’s got some confidence in each, she’s *much* more confident that the actual world is ω_1 than that it’s ω_2 .

The notion of *almost omniscience* should make sense; in fact, a minor variation on it already exists in the literature in the case of David Lewis’ two gods (1979, pp. 520–1). And we could easily imagine each one of Lewis’ gods being more or less confident regarding which of the two (centred) worlds they inhabit by some substantial amount, even if the exact amount is itself imprecise to some degree. (The point here won’t hinge on whether the strengths of belief are precise.) So I take it that the situation described is conceivable.

However, the SCEC cannot explain how an almost omniscient Sally might be *much* more confident that the actual world is ω_1 than that it’s ω_2 . A probability function pr will agree with \succsim if and only if, for all P in \mathcal{B} ,

$$pr(P) = \begin{cases} 0, & \text{if neither } \omega_1 \text{ nor } \omega_2 \text{ are in } P, \\ x, & \text{if } \omega_1 \text{ is in } p, \text{ but } \omega_2 \text{ is not in } P, \\ y, & \text{if } \omega_2 \text{ is in } p, \text{ but } \omega_1 \text{ is not in } P, \\ 1, & \text{if both } \omega_1 \text{ and } \omega_2 \text{ are in } P, \end{cases} \quad \text{where } 1 > x > 0.5 > y > 0$$

The problem here is *not* that there are some probability functions satisfying this condition where the difference between x and y is large (arbitrarily close to 1), and some where the difference is small (arbitrarily close to 0). Either type

of probability function would *agree* with Sally’s confidence ranking, so they’re just as accurate as one another. But the numbers are irrelevant.

The reason the SCEC cannot give us any explanation of cardinality in this case is that one of the key background assumptions isn’t satisfied. Recall that explaining cardinal information in the case of length requires an important existential assumption—roughly, that there are enough length-duplicates of objects of varying lengths for us to ‘add’ together. The analogous requirement is not satisfied in [Example 1](#): for any P such that $\omega_1 \in P$ or $\omega_2 \in P$, there are no appropriate ‘confidence-duplicates’ of P —and so there’s not enough propositions around to ‘add’. And yet it certainly *seems* conceivable that Sally could be almost omniscient and believe one proposition *much* more than another. It’s not like, by virtue of knowing almost everything there is to know, Sally suddenly loses the ability to believe one thing much more than another.

So if the situation described in [Example 1](#) is conceivable, then we’ve found an intuitive problem for comparativism. Just imagine that Sally has a friend with the same confidence ranking, but who’s only a *little* more confident that the actual world is ω_1 than that it’s ω_2 . Since it’s conceptually possible for Sally and her friend to have the very same confidence ranking but distinct doxastic states, it follows that there might be more to a person’s beliefs than their comparative beliefs. Moreover, the missing information is cardinal information, which due to the lack of ‘confidence-duplicates’ cannot be explained by the SCEC.

There’s a few ways one might respond. One might agree that the described situation is possible, and that the facts about comparative beliefs *alone* aren’t always enough to ground the facts about partial beliefs. Thus one might shift to a weaker view according to which the facts about Sally’s partial beliefs hold in virtue of the facts about her comparative beliefs *plus* some further qualitative phenomenon. I’ll come back to this idea in [§5](#), since it’s an important response to much of the following discussion as well. Until then, we focus on the stricter version of comparativism. Another obvious response would be to deny that there’s any strong link between conceivability and possibility. Comparativism is a supervenience thesis, and if conceivability doesn’t entail possibility (at least in this case) then there’s simply no problem to worry about. Well, if that’s the route you want to take, then so be it—here’s not the place to decide fundamental questions of philosophical methodology.

There is a third response, however, which I think is more interesting: perhaps the intuitive sense that there is cardinal information in this case lacks an appropriate *theoretical* foundation. As Fine rightly notes,

... from the viewpoint of the theory of measurement it is only reasonable to insist upon an additive scale (probability) for uncertainty if this numerical relationship [i.e. $pr(P \cup Q) = pr(P) + pr(Q)$ for disjoint P and Q] reflects an underlying empirical relationship between uncertainties. (1973, p. 24)

That is: claims relating to the presence of cardinal information in our degrees of belief need to be founded appropriately in theory, and according to comparativism that foundation just *is* the structure of an agent’s confidence ranking. Without such a foundation, who’s to say that any intuitions we have regarding this and similar examples aren’t just an *illusion* brought about by a lack of awareness regarding how probability functions actually serve to measure

strengths of belief—similar to how some people might mistakenly think it makes sense to say that if it’s 30° Celsius during the day and 15° Celsius in the evening, then it’s *twice* as hot during the day as it is in the evening.

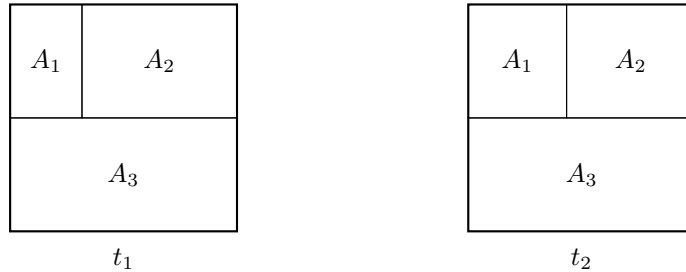
This is an important response, and I suspect it’s one that many comparativists will reach for. To provide evidence that any anti-comparativist intuitions regarding [Example 1](#) aren’t a mere illusion, we need a theoretical justification for positing more structure than can be captured by a confident ranking.

3.2 The SCEC and decision theory

Towards that end, consider the following example. Say that a non-empty proposition P is an *atom* for Sally just in case she has no beliefs regarding any non-empty propositions stronger than P . Furthermore, say that \mathcal{B} is atomic just in case every non-empty proposition in \mathcal{B} is identical to the union of some collection of atoms. Now consider:

Example 2. At t_1 , Sally has probabilistically representable beliefs with respect to an atomic algebra \mathcal{B} , with atoms A_1, A_2, A_3 . Her confidence ranking includes $(A_1 \cup A_2) \sim A_3 \succ A_2 \succ A_1$. Furthermore, Sally has more than twice as much confidence in A_2 as in A_1 . At t_2 , she changes her beliefs slightly: while her comparative beliefs remain the same as they were at t_1 , she’s now just a little more confident regarding A_2 than she is regarding A_1 .

Sally’s situation should again be conceivable. In pictorial form, where the size of the boxes represent the relevant strengths of belief,



The probabilistic comparativists cannot agree that Sally’s beliefs have changed; instead, they have to say that what seems here like a conceivable difference between Sally at t_1 and at t_2 is in fact impossible. But we can go beyond simple conceivability intuitions in this case, since we can have theoretically well-motivated reasons for saying that Sally attaches more confidence to A_1 at t_1 than she does at t_2 .

A probability pr agrees with Sally’s confidence ranking at either time iff

$$pr(A_1) + pr(A_2) = pr(A_3) = 0.5 > pr(A_2) > 0.25 > pr(A_1) > 0.$$

There’s no shortage of probability functions satisfying this condition. And *each* such function predicts a different set of preferences when it’s (i) taken to model a possible belief state, and (ii) combined with any standard model of rational preference formation, e.g. orthodox expected utility theory.

Suppose for instance that at both t_1 and t_2 , Sally faces choices which have the same decision-theoretic structure:

	A_1	A_2	A_3
Option α	$-2x$	x	x
Option β	0	0	x

Now imagine that at t_1 Sally strictly chooses α ; and at t_2 she makes the opposite choice. The expected utility of α is greater than the expected utility of β just in case $pr(A_2) > 1/3$, so a natural explanation for the change is that at t_1 Sally has more than twice the confidence in A_2 as she does in A_1 , while at t_2 she doesn't. More generally, we could easily imagine that Sally's dispositions to choose regarding an infinite range of possible choice situations *consistently* point towards her believing A_2 more than twice as much as A_1 at t_1 , and less than twice as much at t_2 . Sally prefers δ to γ at t_1 , and γ to δ at t_2 :

	A_1	A_2	A_3
Option γ	$2x$	$-x$	x
Option δ	0	0	x

There's a natural explanation for why Sally would have dispositions 'as if' she believed A_2 more than twice as much as A_1 at t_1 and less than twice as much at t_2 —because she does indeed believe A_2 more than twice as much as A_1 at t_1 and less than twice as much at t_2 . That explanation is off-limits to comparativism.

The point of all this is that not only is there a *conceivable* cardinal difference between Sally's beliefs at t_1 and t_2 , but that such differences can do useful theoretical work. If Sally has more than twice the confidence in A_2 as she does in A_1 , then the utilities of the outcomes at A_2 get more than twice the weighting as the utilities of the outcomes at A_1 —and since the difference in utility between x and 0 is half the difference between 0 and $-2x$, the cardinal differences in the weightings for A_1 and A_2 between t_1 and t_2 is what makes all the difference to her preferences. There's a theoretical foundation for the intuition that Sally's beliefs changed from t_1 to t_2 despite no change in her comparative beliefs.

Cardinal differences between ordinally-equivalent systems of partial belief can show up in how those beliefs interact with utilities in the generation of preferences according to the standard theory of decision making. Those differences cannot be explained by reference to Sally's comparative beliefs with respect to unions of disjoint equally-ranked propositions. Indeed, the SCEC in principle cannot help us explain *any* cardinal relationships between the strengths of her beliefs for A_1 and A_2 (at either time), since again the required 'confidence-duplicates' are missing. So, in at least some cases, the SCEC fails to adequately recapture one of the key theoretical reasons for supposing that our beliefs carry cardinal information.⁵

⁵ Let me reiterate that while [Example 1](#) and [Example 2](#) are intended as problems specifically for *binary* probabilistic comparativism, analogous cases can be raised for *quarternary* probabilistic comparativism as well. The examples are only meant to be illustrative. The core of the problem is that there are always some binary/quarternary \succsim that agrees with multiple unconditional/conditional *pr*. This occurs whenever there are too few 'confidence-duplicates' to determine ratios for all pairs of propositions. And as a general rule of thumb, the numerical differences between two ordinally-equivalent probability functions will be relevant in some choice situations.

3.3 The SCEC and radical Pyrrhonism

Is the SCEC at least still successful in those special cases where there’s no shortage of ‘confidence-duplicates’, and subsequently the confidence ranking agrees with exactly one probability function? Well, no—I don’t think so. Consider:

Example 3. Although her confidence ranking agrees with exactly one probability function, Sally is *not* an ideal Bayesian agent. After reading a little too much radical Pyrrhonian literature, she insists it’s never rational to be *fully* certain of anything: one should always reserve *some* slight doubt (say, 1%) that even the most firm of logical truths might be false, and that any logical falsehood might be true. Moreover, her preferences consistently reflect her new-found commitments—for instance, she’d prefer being given \$90 outright to the gamble ($\$100$ if $P \vee \neg P$, $-\$1000$ otherwise), and she prefers ($\$100$ if $P \vee \neg P$, $\$100,000$ otherwise) to being given \$1000 outright.

Here’s one explanation of Sally’s preferences: where pr is the unique probability function that agrees with Sally’s confidence ranking, her partial beliefs are in fact modelled by the non-additive pyr , where

$$pyr(P) = 0.98 \times pr(P) + 0.01$$

Think of pyr as pr squished down by 1% on either side. How would this help to explain Sally’s preferences? Because those preferences *make sense* given pyr .⁶ We may want to say that Sally is *epistemically* irrational, since she fails to attach complete certainty to self-evident tautologies. But she is at least *pragmatically* rational enough to choose appropriately given her slightly misled beliefs.

Such an explanation is off-limits for comparativism: pr and pyr are ordinally equivalent, so comparativists are committed to saying that pr represents Sally’s beliefs iff pyr does. They might say that at most one of pr and pyr constitutes an *adequate* representation of her beliefs, but both must be *accurate*. And any agent with beliefs accurately represented by the probability function pr ought to prefer ($\$100$ if $P \vee \neg P$, $-\$1000$ otherwise) to being given \$90 outright, and prefer \$1000 outright to ($\100 if $P \vee \neg P$, $\$100,000$ otherwise). Consequently, Sally must—for *some* reason—have chosen irrationally given her beliefs. But that’s hardly satisfying. We have to posit some irrationality somewhere in order to explain Sally’s preferences. If there’s a doxastically relevant difference between pr and pyr , then the former explanation leaves us with an agent that *makes sense*: it’s easy to imagine an agent so committed to radical Pyrrhonism that they doubt even the most obvious logical truths. The latter explanation, on the other hand, leaves us with an agent whose preferences are bafflingly nonsensical given what she supposedly believes.

I should note that my argument here does not fall foul of the error discussed by Joyce (2015, pp. 418–9) in his defence against a closely related objection to

⁶ Orthodox expected utility theory presupposes probabilistic coherence. Consequently, in order to explain the sense in which Sally’s preferences ‘make sense’ given pyr , we need to go beyond the orthodoxy. In (Elliott 2017), I describe what Sally’s preferences would need to be like to ‘make sense’ given an incoherent function like pyr under a generalisation of expected utility theory that requires only that the strengths of belief assigned to complementary pairs of propositions sum to 1. See also (Pruss forthcoming).

comparativism—the error of re-scaling our model of Sally’s beliefs without making adjustments to how expected utilities are calculated, thus giving the misleading impression that pyr generates different predictions about Sally’s preferences when they’re plugged into the standard decision-theoretic model. I agree this would be an error—it would be an instance of the very same error underlying the transformation argument. We can all agree that the scale we use to measure belief is a matter of stipulation. It doesn’t really matter if we represent Sally’s partial beliefs on a 0-to-1 scale, a 0.01-to-0.99 scale, or a $\sqrt{2}$ -to- π scale. Likewise it doesn’t really matter if we represent Sally’s partial beliefs using an additive scale or a multiplicative scale (and so on), so long as we make the appropriate adjustments elsewhere in our theories to accommodate those changes.

However, we get to say that pyr is a mere *re-scaling* of pr only under the substantive assumption that whatever Sally believes least (most) of all, she believes to the least (greatest) extent possible. This kind of assumption is required for the SCEC’s construction of ratio information: since $P \cup \emptyset = P$ for all P , \emptyset needs to be treated as the identity element on any additive representation of belief (i.e. $pr(\emptyset) = 0$). Yet it’s not a mere matter of stipulation that *having at most as much confidence in P as in anything else* is the same thing as *being 0% confident that P* , since—as [Example 3](#) suggests—it’s not obvious that P ’s sitting at the bottom of Sally’s confidence ranking plays the same functional or theoretical role’s that 0% confidence is supposed to play.

3.4 The SCEC and non-additive beliefs

Perhaps [Example 3](#) is too far-fetched. Not to worry: there’s any number of order-preserving transformations of a given probability function pr with values in the 0-to-1 range inclusive, and it would be implausible for comparativists to pass *all* of these off as mere ‘re-scalings’ of that function.

Consider the following non-additive order-preserving transformation of the probability function pr :

$$cap(P) = \begin{cases} 3/4 \times pr(P), & \text{whenever } 0 \leq pr(P) < 0.5 \\ 1 - (3/4 \times pr(\neg P)), & \text{whenever } 1 \geq pr(P) > 0.5 \\ pr(P) & \text{otherwise} \end{cases}$$

cap is a *capacity*. Specifically, a capacity like cap satisfies the usual NORMALISATION and NON-NEGATIVITY conditions that any probability function must satisfy, but replaces the ADDITIVITY condition with the weaker:

$$\text{If } P \subseteq Q, \text{ then } cap(P) \leq cap(Q)$$

Every probability function agrees with a confidence ranking, and every such confidence ranking agrees with multiple non-probabilistic capacities. And, importantly, these ordinally-equivalent representations of belief will each predict different patterns of preference on any of a range of natural generalisations of ordinary expected utility theory which make room for partial beliefs represented by capacities—e.g. (Schmeidler 1989), (Sarin and Wakker 1992), (Elliott 2017), or (Pruss [forthcoming](#)).

Thus, suppose that Sally’s confidence ranking agrees with exactly one probability function pr , and that P_1 , P_2 , and P_3 are equally ranked and pairwise

disjoint, with $(P_1 \cup P_2 \cup P_3) \sim \Omega$. According to the SCEC, Sally will have half as much confidence in P_1 as she does in $P_2 \cup P_3$, as represented by pr :

$$pr(P_1) = 1/3, \quad pr(P_2 \cup P_3) = 2/3$$

In this sense, pr ‘adequately’ represents the ‘additive’ structure of \succsim in relation to the union of disjoint propositions. But we could easily imagine preferences over appropriately structured choice situations which suggest that Sally has only $1/3$ as much confidence in P_1 as she does in $P_2 \cup P_3$, as represented by cap :

$$cap(P_1) = 1/4, \quad cap(P_2 \cup P_3) = 3/4$$

In this case, it’s the non-additive cap that does a better job of capturing the cardinal information implicit in how her beliefs relate to her preferences.

3.5 The SCEC and irrational rankings

Most capacities are not ordinally-equivalent to any probability function, but nevertheless seem to do a perfectly good job of representing possible belief states. In particular, there are many capacities that agree with a confidence ranking that does not satisfy the MONOTONICITY condition that the SCEC relies upon. For instance:

Example 4. Relative to Sally’s comparative beliefs, P_1, \dots, P_{100} and Q_1, \dots, Q_{101} are two sequences of pairwise disjoint propositions, where $P_1 \sim \dots \sim P_{100} \sim Q_1 \sim \dots \sim Q_{101}$. However, due to an accounting error, Sally has as much confidence in $P_1 \cup \dots \cup P_{100}$ as in R , and as much confidence in R as in $Q_1 \cup \dots \cup Q_{101}$. Moreover, her preferences fit with what we’d expect if Sally has beliefs represented by the capacity irr , which is additive with respect to the P_i but sub-additive by $100/101\%$ for significantly large unions of the Q_i .

A confidence ranking like this is no doubt conceptually possible. However, it’s easy to see that the *union of disjoint propositions* cannot behave like doxastic analogue of addition for any confidence ranking that agrees with irr . For suppose that it did. Then Sally would believe R 100 times as much as P_1 , and she would believe R 101 times as much as Q_1 . But she also believes P_1 as much as she believes Q_1 , and she obviously doesn’t believe R 1% more than itself. Hence, \succsim doesn’t have the right structure to support the measurement analogy.

Should this mean that Sally doesn’t have partial beliefs with respect to the P_i and Q_i , or that those partial beliefs don’t carry any determinate and meaningful cardinal information? Why should we say that, when we can plug the capacity irr into any decision model that makes room for non-additive capacities and have the determinate cardinal information represented by that function do useful theoretical work? And if that information *is* meaningful, then its being so cannot be explained by the SCEC.

The more general challenge here, of course, is for comparativism to plausibly explain cardinal information in the face of ordinary human irrationality. There is a substantial amount of work which suggests that ordinary agents’ comparative beliefs are not probabilistically representable, and that they’ll often fail to satisfy

the MONOTONICITY condition. There are many examples to draw from, but one of the most striking—and robust—is the conjunction fallacy:⁷

P . Linda is a bank teller

Q . Linda is active in the feminist movement

$P \cap Q$. Linda is a bank teller and active in the feminist movement

A large number of people—the exact percentage doesn’t matter—when they are asked to judge the relative probabilities of these propositions, seem to commit the single conjunction fallacy: they will say that they judge $P \cap Q$ to be more probable than one of the other propositions (usually P).

The propositions P and Q in this example are not disjoint, but it doesn’t take much work to show that instances of the conjunction fallacy run up against the general proposal that the confidence Sally has in the union of disjoint propositions will be the sum of the confidences she has for those propositions individually. For in this case, $P = (P \cap Q) \cup (P \cap \neg Q)$, and $(P \cap Q) \cap (P \cap \neg Q) = \emptyset$. Hence, for the analogy with the measurement of length to hold, we would need:

$$pr(P) = P((P \cap Q) \cup (P \cap \neg Q)) = pr(P \cap Q) + pr(P \cap \neg Q)$$

However, $(P \cap Q) \succ P$, so if pr agrees with \succsim , then $pr(P \cap Q) > pr(P)$ —which would require that $pr(P \cap \neg Q) < 0$, a nonsensical assignment on anyone’s view.

So we have good evidence that ordinary agents’ confidence rankings sometimes falsify MONOTONICITY, and consequently don’t have the appropriate structure to support the measurement analogy. Yet this surely doesn’t prevent such agents from having beliefs with meaningful cardinal information. A person who falls foul of the conjunction fallacy might still, for example, be *a little more* confident that $P \cap Q$ than that P , perhaps by roughly 2% (say); while another might be *much more* confident that $P \cap Q$ than that P . I take it that this is intuitively obvious—or, at least, that if we’re going to say otherwise, then compelling reasons would be required. The people who commit the conjunction fallacy don’t suddenly lose their capacity to believe the relevant propositions with meaningful cardinal differences in strength.

4 Objections and Replies

I’ve discussed the limits of the SCEC when the requisite structural assumptions are satisfied but the existential assumptions are not (§§3.1–3.2); when the existential and structural conditions are both satisfied (§§3.3–3.4); and when the structural conditions are not satisfied (§3.5). In some of these cases the SCEC fails to supply any meaningful cardinal information at all, while in others it supplies the *wrong* cardinal information. So it seems that comparativism still

⁷ See (Lu 2016) for a recent review of the empirical literature, and (Moro 2009) for discussion on the interpretation of the results. I am aware that the philosophical and psychological literature on the extent of human doxastic irrationality is both vast and for the most part controversial, and the cited works cover only a tiny fraction of it. I cannot plausibly cover the relevant interpretive and empirical issues here. Nevertheless, there’s widespread agreement that we’re *not* probabilistically coherent, both with respect to our partial beliefs and (*mutatis mutandis*) our comparative beliefs. Even most ‘descriptive’ Bayesians will usually agree with this much (e.g. Chater et al. 2011; Griffiths et al. 2012)—to the extent that there’s disagreement on this front, it usually concerns the *extent* of our irrationality, not *whether* we are irrational to some degree.

lacks an adequate response to the *cardinality challenge* after all. The reason, I think, is that cardinal information isn't grounded in the structure of \succsim in relation to the union of disjoint propositions, as proposed by the SCEC, but is instead grounded in how beliefs relate to preferences. Partial beliefs are more than just a theoretical tool for representing comparative beliefs.

In this section, I'll consider some potential objections and responses to the arguments of the previous section. Following that, I'll discuss the view according to which the facts about partial belief supervene on the facts about comparative beliefs *plus* some other qualitative phenomenon.

4.1 Impossible worlds

My argument in §3.5, that the conjunction fallacy is inconsistent with any confidence ranking that supports the analogy with the measurement of length, relies on the assumption that Ω is a set of *logically possible* worlds. This assumption is used to guarantee that $(P \cap Q) \cap (P \cap \neg Q) = \emptyset$ and $P = (P \cap Q) \cup (P \cap \neg Q)$.

However, if Ω were to include enough impossible worlds of the right kind, then the argument would be invalid. More generally, we know that any apparently non-probabilistic (complete) confidence ranking with respect to propositions drawn from one set of worlds Ω can always be re-expressed as a probabilistically representable confidence ranking where the probability function in question is defined for propositions drawn from a larger space of worlds, Ω^+ . See, for example, (Cozic 2006) and (Halpern and Pucella 2011). Elliott (2019) shows that for the fully general result to hold, Ω^+ needs to include not only logically impossible worlds, but also 'incomplete' worlds—i.e., worlds that leave some matters unspecified. So perhaps comparativists might maintain the measurement analogy if they let propositions be characterised as sets of possible *and* impossible/incomplete worlds.

I have raised this objection only to acknowledge it, and then set it aside. In other works, I have argued that letting Ω include logically impossible worlds creates special problems in the probabilistic context (Elliott 2019), and will in fact severely *undermine* the comparativist's analogy with the measurement of length rather than support it (Elliott forthcoming). I won't repeat those arguments here, but let me add two quick points. First, impossible worlds aren't going to help with any of the problems discussed in §§3.1–3.4, where \succsim is probabilistically representable. Second, and more importantly, if saving the measurement analogy from the threat of irrational confidence rankings *requires* the use of logically impossible and incomplete worlds, then that is a significant theoretical *cost* for the view. There are general reasons to worry about the use of sets of possible *and* impossible/incomplete worlds as models of belief content (e.g. Bjerring 2014; Bjerring and Schwarz 2017), so comparativists might not want to put all their eggs into this one basket.

4.2 Approximating probabilistic agreement

While Sally's confidence ranking doesn't support the measurement analogy *exactly* in Example 4, it at least *approximates* one that does. So maybe we could use the cardinal information extracted from the probabilistically representable ranking or rankings that \succsim most closely approximates to explain how Sally's beliefs still manage to support some indeterminate form of cardinal information? (cf. Stefánsson 2017, p. 576, fn. 6.)

Now, if the goal were merely to explain how Sally’s beliefs contain *some* cardinal information, whether determinate or indeterminate, then something like this kind of response might suffice. But the point of [Example 4](#) was not that comparativism has no way of making sense of cardinal information in some form whenever \succsim doesn’t satisfy MONOTONICITY. Of course there are many ways one might preserve *some* semblance of cardinality in these cases. That’s obvious—what’s not obvious is whether this will be *enough*. My argument was that the SCEC cannot get us the *right* cardinal information in cases like this—specifically, cases where (i) Sally’s confidence ranking only agrees with non-additive functions like *irr*, yet (ii) at least one of those functions seems to do a good job of representing her partial beliefs as reflected by fit with her preferences.

Sally’s confidence ranking in [Example 4](#) will indeed approximate a ranking that agrees with some probability function; for instance,

$$pr(P_1 \cup \dots \cup P_{100}) = pr(Q_1 \cup \dots \cup Q_{101}) - pr(Q_1)$$

pr will in turn approximate *irr*. But—and this is the important point—it won’t *be irr*. It’s not implausible to think that *irr* represents a possible system of beliefs with *determinate* cardinal information, especially inasmuch as that information might be reflected in the consequences it has for Sally’s preferences. According to *irr*, Sally has the same confidence regarding $Q_1 \cup \dots \cup Q_{101}$ as she does regarding $P_1 \cup \dots \cup P_{100}$; according to *pr*, she doesn’t. So *pr* misrepresents her beliefs, and moreover it generates the wrong predictions about her preferences. So how, exactly, is an approximation like *pr* going to help us get at the *right* cardinal information?

4.3 Disjunctivism

Next up is what we might call *disjunctivism*. The idea is this: if \succsim doesn’t satisfy the requisite structural and/or existential assumptions needed for the measurement analogy *under the assumption that it’s the union of disjoint propositions that’s serving as the doxastic analogue of addition*, then perhaps there will be an alternative operation we could appeal to in those cases instead. We thus have a disjunctive explanation of cardinality: we let the union of disjoint propositions be our doxastic analogue of addition whenever the right conditions obtain, and look elsewhere when they don’t.

Obviously, disjunctivism won’t be enough to solve any of the problems raised in §§3.1–3.4, which involve ordinally-equivalent pairs of representations. More importantly, disjunctivism undercuts one of comparativism’s main selling points. What makes the SCEC compelling—to the extent that it is—is that it’s straightforwardly analogous to the well-established explanations of cardinal information that we find in the case of basic physical quantities like length or mass. But there’s nothing at all like disjunctivism for any quantities in the sciences, where sometimes we appeal to one operation as the real-world analogue of addition, and sometimes we appeal to another, depending on what works in the moment. The reason for this is obvious: it would make the *meaning* of cardinal information for that quantity too unstable.

The same applies to comparative beliefs. If different operations on propositions are supposed to explain cardinality for different agents, with the choice of each operation being contingent on what’s appropriate for that agent’s idiosyncratic confidence ranking, then both interpersonal and intrapersonal comparisons of belief would become quite useless in general. Before we could know

what it means for Sally to believe P twice as much as Q , we would first have to take into account her entire confidence ranking, work out what the relevant operation should be, and only then give some doxastic meaning to the statement. Without knowledge of the overall structure of her confidence ranking, then, such a claim would only tell us:

- (i) $P \succ Q$
- (ii) There's *some* binary operation \circ on \mathcal{B} that shares certain structural characteristics with $+$ relative to \succsim such that for Q', Q'' where $Q \sim Q' \sim Q''$, $Q' \circ Q'' = P$

The latter is utterly uninformative, and the former we don't need cardinal information to express! Worse still, there's no guarantee that the cardinal information we get out will track the most obvious implications of believing P twice as much as Q (e.g. being willing to bet twice as much on the former as on the latter). Disjunctivism is a non-starter.

4.4 'I only want to model ideally rational agents'

A final response to examples that involve non-ideal agents is to limit the intended explanatory scope of comparativism. The basic idea behind this response is that we can (at least for now) safely ignore irrational agents for the purposes of current philosophical theorising. There seem to be two versions of this thought: first, that we can ignore non-ideal agents because what matters for most contemporary philosophical purposes is that we have an explanation of cardinality for *ideally rational agents*; or second, that at this stage it's perfectly reasonable to limit our theories to cases of ideal rationality where we can expect matters to be simpler and more manageable, and de-idealise at some point later on once we've got a handle on the easier cases. In support of both versions, though, it's noted that the SCEC *does* seem to work well for ideally rational agents—at least when there are sufficiently many 'confidence-duplicates' to guarantee unique probabilistic agreement.

Probabilism tells us that an ideally rational agent will have at least n times as much confidence in P as in Q if she has at least as much confidence in P as she does in $R_1 \cup \dots \cup R_n$, where the R_1, \dots, R_n are disjoint and $R_1 \sim \dots \sim R_n \sim Q$. So probabilism predicts that the SCEC will generate the right results for ideally rational agents. But probabilism is common ground for most comparativists and non-comparativists alike. So the question is whether this fact reflects some interesting explanatory relationship between the meaning of ' n times as much confidence' and confidence rankings over disjoint unions, or whether it's just a consequence of the claim that a rational system of partial beliefs ought to be representable by a function that satisfies ADDITIVITY.

If it *does* reflect an interesting explanatory relationship, then presumably that same relationship should also hold for non-ideal agents. We don't want to have a disjunctivist explanation, with one kind of theory for the ideal agent and a wholly separate theory for the non-ideal agent. Moreover, it would be unreasonable to say that Sally doesn't have partial beliefs encoding interesting cardinal information just because she isn't ideally rational. I have partial beliefs with meaningful cardinal information, and I'm far from ideally rational. So, comparativists should be able to show at least that the SCEC or something much like it is *plausibly generalisable*. For if there doesn't seem much hope for

generalising the SCEC to non-ideal agents, then we’ve got good reasons to think that the explanation is false—even in the case of ideally rational agents.

In §3.5, I’ve discussed cases where \succsim fails to satisfy MONOTONICITY. Elliott (forthcoming) shows that it’s possible to generalise the SCEC to some degree, such that \succsim need only satisfy a weaker condition R-COHERENCE. However it’s also shown in that work that R-COHERENCE is a *minimal* condition on any (non-disjunctive) explanation of cardinality that preserves the basic structure of the SCEC in cases where \succsim is uniquely probabilistically representable. Both Example 4 and the case of the conjunction fallacy involve confidence rankings that fail to satisfy R-COHERENCE under very natural assumptions.

The point here is that the strategy of the SCEC seems to *essentially* require that our comparative beliefs will satisfy quite strong and empirically dubious conditions. It seems, then, that the prospects for generalising the SCEC to non-ideal agents are not particularly strong. Or at the very least: constructing a plausible explanation of cardinal information that extends to both ideal and non-ideal agents remains a serious challenge for comparativism.

5 Supplemented comparativism

Let *supplemented comparativism* denote the view that partial beliefs supervene on comparative beliefs *plus something else* (whatever that may be, so long as it doesn’t trivialise the whole affair). Some obvious possibilities might be:

- *Judgement-supplemented comparativism*: the facts about Sally’s beliefs are determined by her comparative beliefs and her qualitative judgements regarding probabilistic dependence and/or evidential relationships; for instance, *that P is evidence for Q*. (We’ll call these ‘evidential-dependence judgements’).⁸
- *Evidence-supplemented comparativism*: the facts about Sally’s beliefs are determined by her comparative beliefs and her history of evidence.
- *Preference-supplemented comparativism*: the facts about Sally’s beliefs are determined by her comparative beliefs plus facts relating to her preferences.⁹

Will some version of supplemented comparativism fare better in response to the above problems than non-supplemented comparativism?

I cannot discuss every possible style of supplemented comparativism, so allow me instead to make a few general points on the matter. First, it’s important to be clear what *the problem* is supposed to be. In particular, I want to distinguish between the following two challenges for comparativism:

1. *Granularity*: there seem to be meaningfully distinct systems of partial belief that correspond to the same system of comparative belief.
2. *Cardinality*: there seems to be meaningful cardinal information in some systems of partial belief that the SCEC cannot explain.

It’s obvious that some form of supplemented comparativism will be better placed to deal with the granularity challenge. Clearly, if \succsim doesn’t contain enough

⁸ This kind of view is very briefly suggested in (Joyce 2010, p. 288).

⁹ A representation theorem similar to that of (Joyce 1999) might help in providing the mathematical foundations for preference-supplemented comparativism. Of the three possibilities noted here, I think this is the most plausible. It is also a theory which fits naturally with explaining cardinal information in partial belief by the theory of additive conjoint measurement (§2.3).

information to determine the facts about her beliefs, then \succsim *plus* something else might. That’s not very interesting, and without a detailed theory to play with it’s hard to say much more on the matter. But the target of my arguments throughout has been the SCEC, and *those* arguments apply with equal force to any version of supplemented comparativism that seeks to apply the SCEC.

Consider judgement-supplemented comparativism, and recall [Example 1](#): Sally is almost omniscient, but she has *much* more confidence that the actual world is ω_1 than that it’s ω_2 . Now imagine that Sally’s evidential-dependence judgements are such that the uniquely correct probabilistic representation pr of her beliefs must be such that $pr(\{\omega_1\}) \gg pr(\{\omega_2\})$. Likewise we could imagine that Sally’s friend Bob has the same confidence ranking, but his ‘dependence judgements’ are such that his uniquely best probabilistic representation pr' is such that $pr'(\{\omega_1\}) \approx pr'(\{\omega_2\})$. Great: judgement-supplemented comparativism has managed to make *distinctions* between belief states where ordinary non-supplemented comparativism could not. Of course it can, because it has more resources to play with. But what explains the sense in which Sally has *much* more confidence that the actual world is ω_1 , while Bob doesn’t?

The facts that $pr(\{\omega_1\}) \gg pr(\{\omega_2\})$ and $pr'(\{\omega_1\}) \approx pr'(\{\omega_2\})$ do not constitute an explanation, no matter how unique pr and pr' happen to be relative to Sally’s and Bob’s confidence rankings and evidential-dependence judgements respectively. The reason should by now be clear: the *numbers* are irrelevant. The fact that judgement-supplemented comparativism can *distinguish* between ordinally-equivalent representations of partial belief does not thereby entail that it has a satisfactory *explanation* of the cardinal information those distinctive representations seem to capture. Either ‘much more confidence’ reflects in a natural way some underlying real-world relationship that’s independent of the numbers we use to represent it, or it’s meaningless—at best an illusion of cardinality caused by an arbitrary choice of scale.

So what is the real-world relationship that explains the cardinal information apparently present in Sally’s beliefs? Obviously, the judgement-supplemented comparativist appeal to the SCEC, because in cases of almost omniscience there aren’t enough disjoint propositions at varying ranks around to ‘add’. The same applies for [Example 2](#): no explanation in terms of the structure of \succsim can account for the differences in cardinal information between t_1 and t_2 , simply because there *are* no differences in the structure of Sally’s comparative beliefs at t_1 and at t_2 . For the same reason, no such explanation can account for the differences in cardinal information that seem to be represented in the differences between pr , pyr , and cap (from §§3.3–3.4), since these are ordinally-equivalent.

Judgement-supplemented comparativism owes us a compelling and general explanation of cardinal information just as much as non-supplemented comparativism does. I don’t know what such an explanation would look like, and frankly I’m doubtful that the judgement-supplemented comparativist will be able to come up with one that’s as intuitively compelling and consonant with standard methodologies as the SCEC was supposed to be.¹⁰ But that’s neither

¹⁰ There isn’t anything in the measurement theorist’s toolkit that we could use to explain cardinal information in terms of a confidence ranking \succsim plus a non-transitive evidential-dependence relation R . A new style of measurement system could perhaps be developed, but I do not know how. Moreover, and as I’ve noted, part of the SCEC’s appeal is its continuity with standard methodologies. If judgement-supplemented comparativism requires a whole new theory of measurement just to make sense of belief, then that’s surely a cost for the view.

here nor there. The main target of my arguments has been the SCEC, and the SCEC is just as problematic for judgement-supplemented comparativism.

The very same points apply, for the very same reasons, to any other variety of X -supplemented comparativism you care to think of. The fundamental problem is that the SCEC appeals to comparative beliefs over disjoint unions. Regardless of whatever else we throw into the supervenience base, the SCEC cannot account for (i) differences in cardinal information represented in distinct but ordinally-equivalent representations of partial belief; nor for (ii) cardinality in cases where the comparative beliefs lack the requisite structure. No ‘further fact’ view is going to magically make the SCEC apply even when Sally’s confidence ranking fails to satisfy MONOTONICITY. A different style of explanation is needed.

If the only takeaway messages of this paper are (a) the facts about partial beliefs carry more information than the facts about comparative beliefs, and moreover (b) the SCEC is inadequate, then I’ll be happy. The central challenge that I’ve been pushing is that comparativism currently lacks an adequate explanation of cardinal information. Until such time as supplemented comparativism offers us something new, that challenge extends to it as well.

6 Conclusion

Koopman was right about this at least: the numbers we use to refer to and reason about strengths of beliefs are not essential to them. Nobody seriously thinks that there are numbers literally in the head—that *numerical* strengths of beliefs are somehow metaphysically *sui generis* and we just have to treat their ratios and intervals as intrinsically meaningful. All parties to this debate agree that the numbers are just a way of representing strengths of belief, while their ratios and intervals must in addition represent some closely related and fundamentally *non-numerical* psychological phenomenon by virtue of some abstract structure that phenomenon shares with the relevant numerical operations and relations. The hard part is saying what that structure could be.

Whatever the right account is, though, I think it’s unlikely to be found in the meagre resources allowed by (non-supplemented) comparativism. More generally, I doubt that the standard comparativist explanation of cardinality correctly locates the actual qualitative phenomena that underlies how our partial beliefs come to carry meaningful cardinal information. The purported analogy between comparative beliefs and the measurement of length is misleading. A better explanation is needed.¹¹

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