Comparativism and the Measurement of Belief

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Draft of March 13, 2020

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 in interaction with her desires guides her intentional behaviour and the formation of her preferences. This paper concerns what Sally's beliefs might be like at the most fundamental level, and moreover the relationship between the different types of beliefs she might be taken to have.

To get the ball rolling, I will assume that Sally has at least two kinds of belief: *partial* and *comparative*. With respect to the former, Sally is for instance quite certain that there's an external world, somewhere between 95% and 98% confident that the Earth is an oblate spheroid, uncertain about the consequences of global warming, but doubtful they'll be good. These are attitudes directed towards single propositions, each of which comes with some (possibly imprecise) *strength*, which can at least sometimes be represented numerically. And with respect to her comparative beliefs, Sally is, for example, at least as confident that $2 + 2 = 4$ as she is anything else, she's more confident that she'll find good coffee in Melbourne than she will in Sydney, and indeed she's less confident that she'll find good coffee in Sydney than that she'll win the lottery next week.

Taking that for granted, it's natural to wonder about the relationship between Sally's partial and comparative beliefs. It's clear enough that they're closely connected: if Sally has high confidence that P and low confidence that

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Q, then she's more confident that P than she is that Q; likewise, if Sally has the same confidence in P as she does in Q, then she's 50% confident that P just in case she's 50% confident that Q. Moreover, these conditionals have a certain feel of *apriority* about them. Consequently it's reasonable to think that they're underwritten by some interesting conceptual or metaphysical connection. In particular, you might wonder whether one of the two kinds of belief is *more fundamental* than the other.

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 2016; 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 of comparativist theories 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 serving as a 'theoretical tool' for representing the positions of propositions within that ranking.

In this paper, I will raise a number of challenges for comparativism. Glossing over some of the details to come, in §3 I'll argue that comparativism lacks an adequate account of the measurement of the strengths of our beliefs. I will furthermore argue in that section that there are apparently meaningful and theoretically useful distinctions between belief states that comparativism cannot account for, and that it struggles to handle widely observed instances of irrationality. Objections and responses to these arguments will be considered in §4; and in §5, I will critically consider the 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).

2 Probabilistic Comparativism

Before we discuss problems with comparativism, we get a clearer idea of what comparativism *is*. In fact, there are two essential components to any comparativist's theory. The first component is, to put it roughly, 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)

[1] Let me emphasise: comparativism, as I'm understanding it here, is *not* the idea that partial beliefs depend on *some comparative thing or other*. For the purposes of this paper, I'm taking it to be a specific claim about the metaphysical relationship between partial and comparative beliefs. Comparativism can be—and often is—divorced from the thesis that partial beliefs depend on other comparative mental states such as preferences. The reader should be careful to keep these ideas separate, though see §5 and §6 for more discussion.

The second component is an explanation of *where cardinality comes from*. Why, for instance, does it *seem* to make good 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, and so on? That is, the numbers we use to represent the strengths of belief seem to encode more-than-merely-ordinal information. Let us (stipulatively) refer to this henceforth as *cardinal information*.

In §2.1, I'll say more about the first component; and in §2.2 and §2.3, more about the second. I note, though, that I will not try to describe every possible variety of comparativism, nor even all those currently on the market. Instead, I will focus my exposition on a relatively straightforward and common version of the view, what I will call *probabilistic comparativism*.

2.1 Where the numbers come from

I like to think that Sally accepts *probabilism*, as well she should. Supposing that she does, what is it then that she's accepting exactly? According to the usual gloss, probabilism 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 Sally's beliefs (whether partial or comparative) aren't literally real-valued functions. Of course, there's no real problem here—not yet, anyway. The usual gloss was obviously meant to be elliptical. The intention is that Sally's beliefs ought to be such that they can be *represented* by a probability function.

We can break this claim down into two parts. The first is a constraint on the set of propositions (call it $\boldsymbol{\mathcal{B}}$) towards which Sally has beliefs—specifically, that 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. $\boldsymbol{\mathcal{B}}$ is non-empty, and (i) if P is in $\boldsymbol{\mathcal{B}}$ then the complement of P with respect to Ω is in $\boldsymbol{\mathcal{B}}$, (ii) if P and Q are in $\boldsymbol{\mathcal{B}}$, then $P \cup Q$ is in $\boldsymbol{\mathcal{B}}$

The second (and more interesting) part is a constraint on the beliefs themselves. Specifically, Sally's beliefs ought to be such that there is a probability function pr that represents her beliefs; that is, a function from a Boolean set of propositions $\boldsymbol{\mathcal{B}}$ to the reals which satisfies at least the following conditions:

Normalisation. $pr(\Omega) = 1$
Non-Negativity. $pr(\varnothing) = 0$
Additivity. If $P \cap Q = \varnothing$, then $pr(P \cup Q) = pr(P) + pr(Q)$

That tells us a little more about what probabilism requires, but we still don't know yet what it is for pr to *represent* Sally's beliefs. What would Sally's beliefs have to be like, exactly, to be probabilistically representable?

[2] By 'possible worlds', I mean nothing more nor less than that the worlds are complete and closed under classical propositional logic. What I have to say won't depend on anything stronger than this. I think it's plausible to treat Ω as a set of 'scenarios' in the sense of (Chalmers 2011); i.e. ω is possible iff it cannot be ruled out a priori. Some comparativists might want to model propositional contents using either sets of possible *and* impossible worlds, or sentences in some formal language, or something else. Some of my critical arguments will depend on the matter of how $\boldsymbol{\mathcal{B}}$ is characterised, so I'll have more to say about this in §4.1.

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—fix the facts about the latter and you'll have fixed everything there is to the former. Consequently, the probabilistic comparativist says, a probability function *represents* Sally's beliefs just when the order of the values assigned to the propositions she believes corresponds exactly to the ordering induced over those propositions by her comparative beliefs (i.e. her *belief ranking*).

We can put that more formally as follows. Assume henceforth that $\boldsymbol{\mathcal{B}}$ is Boolean. Next, assume that Sally's belief ranking can be faithfully represented with a single binary relation $\succsim$ defined over the propositions in $\boldsymbol{\mathcal{B}}$, where

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

Where $\succ$ and $\sim$ stand for the *more confident* and *equally confident* comparatives respectively, we will consequently also want to assume

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

Finally, from now on we will say that a real-valued function f (that is, not necessarily a probability function) *agrees with* Sally's belief ranking just in case

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

Thus, if we suppose that Sally's beliefs are ultimately just 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 just 'mathematical constructs' designed to help us reason about what, according to the comparativist, 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 entirely unambiguous formulation of exactly what it is that probabilism requires of us. Since (de Finetti 1931), we've known that for probabilistic agreement it is necessary that Sally's belief ranking satisfies (for all P, Q, R in $\boldsymbol{\mathcal{B}}$):

WEAK ORDER. $\succsim$ is transitive and complete

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

MINIMALITY. $P \succsim \varnothing$

QUALITATIVE ADDITIVITY. If $R \cap (P \cup Q) = \varnothing$, then $P \succsim Q$ iff $(P \cup R) \succsim (Q \cup R)$

We need something a little stronger than QUALITATIVE ADDITIVITY if we want necessary *and* sufficient conditions for probabilistic agreement (see 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 belief ranking (see Fishburn 1986). But the details of those further (and more complicated) conditions need not concern us—what's listed here is more than enough for us to get a handle on the kinds of constraints a belief ranking has to satisfy to be probabilistically representable.

Before we move on, let me briefly flag one important way in which comparativists might want to diverge from the specifics of the view that I've been describing. What we might call *quarternary probabilistic comparativism* replaces the *binary* comparative confidence relations I've been describing for *quarternary* relations; e.g. $P, Q \succsim R, S$ iff Sally has at least as much confidence in P *given* R as 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

$$P, Q \succsim R, S \text{ iff } pr(P \mid Q) \geq pr(R \mid S)$$

(See Koopman 1940; Fine 1973, pp. 28–32; DiBella 2018; see also Hawthorne 2016 for related ideas.) Each of the main points that I raise below with regards to binary comparativism can also be raised for quarternary comparativism, *mutatis mutandis*; for the sake of brevity, though, I won't spell out the details.

2.2 Where cardinality comes from: the transformation argument

You might worry that something is still missing from the picture. In particular, the probabilistic comparativist still owes us an explanation of why it apparently makes sense to say, e.g. that Sally might believe P *much more* than Q, or that she might believe P *several times as much* as Q, and so on.

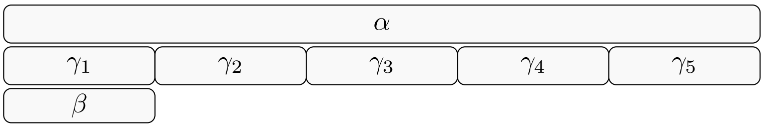
This is sometimes raised as a challenge for comparativism; we can call it the *cardinality challenge* (e.g. Meacham and Weisberg 2011, p. 659). To get an initial sense of why there might be a problem here, imagine a situation in which Sally is about to roll an ordinary six-sided die. Now consider:

Ord. Sally is more confident of rolling ≥2 than of rolling a 1
Int. Sally is much more confident of rolling ≥2 than of rolling a 1
Rat. Sally is five times as confident of rolling ≥2 than of rolling a 1

The meaningfulness of claims like *Int* and/or *Rat* presents a *prima facie* problem here for the comparativist. Suppose we have a probability function, pr, which agrees with $\succsim$. Any order-preserving transformation of pr will therefore also agree with $\succsim$. Only information which is common to these functions—i.e. the ordering—can be taken to represent something doxastically 'real'. However, we know that neither *ratios* nor *ratios of differences* need be preserved across arbitrary order-preserving transformations. Therefore, it seems, only purely ordinal claims like *Ord* can be taken to have genuine doxastic meaning, whereas claims like *Int* and *Rat* only appear to be meaningful due to an arbitrary choice of scale. Call this the *transformation argument*.

The transformation argument is flawed. Fans of probabilistic comparativism have a long-standing and at this point very standard story about how cardinal information might be explained in a manner entirely consistent with their view. It has been discussed in numerous locations, though in particular depth in (Fine 1973, pp. 68ff). Since this will be the focus of my arguments below, so I'll spend a fair bit of time on it here. The basic idea is to treat the measurement of belief like the measurement of length, or mass, or any of a wide array of other extensive quantities. As such, I'll begin by showing how one can construct a ratio scale for length out of a system of ordinal length comparisons; following that, I'll discuss what's going wrong with the transformation argument.

Consider the claim 'α is much longer than β'. Our task is to provide truth conditions for this claim without presupposing any cardinal information. Towards that end, suppose first that 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 *length-duplicates* of β. 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, with no spaces between, 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 without presupposing any cardinal information whatsoever: 'α is five times longer than β' just means that α is as long as 5 disjoint length-duplicates of β laid end-to-end.

It's a touch harder to specify truth conditions for claims like 'α is much longer than β', simply because of the general context-sensitivity of 'much'. But once we've got absolute ratio comparisons like the above in our picture, *ratios of differences* in length follow naturally, so you can see such a definition might go: 'α is *much longer* than β' holds true just in case the difference in length between α and β is greater than some contextually-determined threshold difference in length (which might be, e.g., twice the length of β itself).

So that's the basics, and simple variations on the same strategy can be used to provide truth conditions for any other *rational* ratio or interval comparison as well. For example, what we've said already entails that any object δ that's as long as γ_1 and γ_2 laid end-to-end will be $2/5$ as long as α. With a bit of mathematical trickery we can also extend the idea to give meaningful ratio and interval comparisons for the non-rational reals. But whether we generalise it to all of the reals or just stick to rationals, the strategy will require making two kinds of background assumption.

The first is existential: if we're going to define arbitrary ratios and intervals of lengths in terms of objects joined together end-to-end, then we're going to need that there are enough disjoint objects of varying lengths with which to construct these definitions. That's the rough version; we don't need to worry too much about what the precise version looks like for the purposes of this discussion. What matters, simply, is that the more cardinal comparisons you want to make, the more objects of varying lengths you'll need to have lying around. This is a reasonable enough presupposition, since there are quite a few objects in the universe for us to play with. But, as we'll see in §3.1, the analogous requirement in the case of comparativism can cause some problems.

The second assumption concerns the structural properties of the ordinal length relations themselves, and how they interact with the physical operation of laying objects together end-to-end. The very core of the definitional strategy being described here is the recognition that (i) the *is at least as long as* relation is analogous to $\geq$, and (ii) the way the *laid end-to-end* operation interacts with the *is at least as long as* relation is analogous to how $+$ interacts with $\geq$.

We can state this structural requirement more exactly. Let $\alpha \oplus \beta$ represent α laid end-to-end with β; then, we need that for all α, β, γ,[3]

Weak Order. The *is at least as long as* relation is transitive and complete

Positivity. $\alpha \oplus \beta$ is at least as long as α

Weak Commutativity. $(\alpha \oplus \beta)$ is as long as $(\beta \oplus \alpha)$

Weak Associativity. $\alpha \oplus (\beta \oplus \gamma)$ is as long as $(\alpha \oplus \beta) \oplus \gamma$

Qualitative Additivity. If γ is disjoint from α and β, then α is at least as long as β iff $\alpha \oplus \gamma$ is at least as long as $\beta \oplus \gamma$

Archimedean. Nothing is infinitely longer than anything else

One of the key results in the theory of measurement is that if the stated existential and structural requirements are satisfied, then there's a way to assign positive numerical values to objects such that (i) the ordinal length relations are reflected in the ordering of the values so assigned, and (ii) the length of any finite collection of disjoint objects laid end-to-end will be equal to the sum of their individual lengths. In other words, there is some way to assign numbers to objects—*numerical lengths*—such that those numbers encode cardinal information in a natural and easy-to-use way.

We can now clearly see where the transformation is going wrong. The error lies in thinking that what I've been *calling* 'ratio' and 'interval' information depends in any interesting way on the numbers we use to represent strengths of beliefs and the specific numerical relationships that hold between those numbers. This is a mistake. According to the comparativist, the belief ranking itself contains the relevant information if anything does. Hence, the question of what specific numerical relationships are preserved across order-preserving transformations of a given numerical representation of that belief ranking is irrelevant.

By way of comparison, consider: we typically measure length using an *additive scale*; that is, an assignment such that $\alpha \oplus \beta$ always gets a value ≥ 0 that's equal to the *sum* of the values assigned to α and β. We call this is an *adequate* representation because it renders the relevant structural properties of the system of comparative lengths salient and easy to use. But we could instead have used a *multiplicative scale*; that is, an assignment such that $\alpha \oplus \beta$ gets a value ≥ 1 equal to the *product* of the values assigned to α and β (cf. Krantz et al. 1971, pp. 99–102). The choice between additive and multiplicative scales is a matter of convenience, and if we choose the former then α is as long as n length-duplicates of β just in case the values x and y assigned to α and β respectively are such that $x = n{\cdot}y$, whereas if we choose the latter then those values will be assigned such that $x = y^n$.

There is no deep connection, in other words, 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 and equally adequate measurements of length. Moreover, not all *accurate* representations need to be *adequate*: any order-preserving transformation of any adequate representation will ipso facto represent the very same real-world facts, though it might not do so as nicely.

[3] The Archimedean condition is here stated informally. See (Krantz et al. 1971, pp. 72ff) for the precise version. A statement of the Archimedean condition for the additive measurement of belief rankings can be found in (Chateauneuf and Jaffray 1984, p. 193).

2.3 Where cardinality comes from: the SCEC

While the transformation argument rests on a mistake, it *does* serve to highlight a related and very important further question that comparativists need to answer. That is, if the comparativist wants to show that the same style of explanation for cardinal information as was used in the case of length can *in principle* be made to work for comparative beliefs, then it needs to be shown at least that there is some operation on propositions (*qua* the relata of a belief ranking) that can serve as the doxastic analogue of addition.

Such an operation is easy to find: *if* Sally's belief ranking agrees with a probability function, 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 the Additivity axiom. 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).

And indeed, from the axioms of probability plus a few general properties of $\cup$, if $\succsim$ agrees with a probability function, then for all P, Q, R in $\boldsymbol{\mathcal{B}}$,

Weak Order. $\succsim$ is transitive and complete
Positivity. $(P \cup Q) \succsim Q$
Weak Commutativity. $(P \cup Q) \sim (Q \cup P)$
Weak Associativity. $(P \cup (Q \cup R)) \sim ((P \cup Q) \cup R)$
Qualitative Additivity. If $R \cap (P \cup Q) = \varnothing$, then $P \succsim Q$ iff $(P \cup R) \succsim (Q \cup R)$
Archimedean. Sally is not infinitely more confident that P than that Q

In other words, *if* Sally's belief ranking agrees with a probability function pr, then that ranking will have an 'additive' structure with respect to the union of disjoint propositions which will be *adequately* represented by any pr that agrees with that ranking. So, for example, if $\succsim$ agrees with a probability function pr, then Sally has at least n times as much confidence towards P as Q whenever $P \succsim (R_1 \cup \cdots \cup R_n)$, where $R_1, \ldots, R_n$ are pairwise disjoint and $R_1 \sim \cdots \sim R_n \sim Q$. In this case, $R_1, \ldots, R_n$ are Q's *confidence-duplicates*, and consequently the confidence that Sally has in their union will be 'adequately' represented by any probability function pr that agrees with $\succsim$ as the sum of the strengths with which Sally believes the $R_1, \ldots, R_n$.

We thus have what I'll call the *standard comparativist explanation of cardinality*, or SCEC. There is a clear analogy between the SCEC and what I've been saying about the measurement of length, and to the extent this analogy can be maintained it undoubtedly lends some degree of plausibility to the comparativist's proposal as a whole (cf. Fine 1976; 1973, pp. 15–16; Stefánsson 2016; 2018). After all, there's something obviously compelling about explaining ratios and intervals of strengths of belief using a method that's straightforwardly consonant with how things are usually described in the case of other extensive physical quantities like length, volume and duration.

But we should be careful not to overstate what's been established by all this. What we've not been provided with at any point so far is a sustained argument to the effect that *if* $\succsim$ agrees with a probability function, then Sally has 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 detailed 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 a comparativist *might* explain cardinal information given some quite strong assumptions about the shape of an agent's belief ranking. It's enough to establish that comparativism can *in principle* explain cardinality, but it's not yet enough to establish that the SCEC is *correct*.

3 The Case Against Comparativism

In this section and the next, I will present a range of intuitive problem cases for comparativism. First, though, I'll provide a general overview of the intended dialectic, and describe how the following discussion is supposed to be read in relation to several of the main arguments that have been put forward in favour of comparativism (or other views in the vicinity). In particular, I want to focus on the following three arguments:[4]

- The *measurement argument*: the SCEC provides an illuminating explanation of cardinal information.
- The *conceivability argument*: there are conceivable systems of comparative belief that correspond to no system of partial belief, but not vice versa.
- The *sufficiency argument*: comparative beliefs suffice in principle for our theoretical needs.

We've just seen the measurement argument in action, and the primary goal for the following discussion will be to show that the SCEC isn't quite as illuminating as we might have hoped. There are a number of different cases where it's plausible that cardinal information is present (both for intuitive and theoretical reasons), and yet the SCEC is *in principle* incapable of accounting for the presence of that information. Consequently, the SCEC is inadequate. Indeed, I think that the SCEC is deeply misleading: whatever cardinality our beliefs possess probably has nothing directly to do with comparative confidence rankings and disjoint unions or any other doxastic analogue of addition, but is instead more plausibly explicable in relation to the role our beliefs play with respect to our utilities and preferences. I will have more to say about that as we go along, and I'll flag some alternative possible explanations of cardinality in §6.

But wait—there's more! Aside from highlighting the SCEC's inadequacies, my secondary goal is to provide a response to the conceivability and sufficiency arguments. So allow me to say a little more about these, since together they constitute important source of support for comparativism that's independent of matters concerning the adequacy of the SCEC.

The conceivability argument, in Fine's words, 'refers to the curious phenomenon that there exist relatively simple examples of what we consider to be valid [comparative probability] statements that are incompatible with any representation in the usual quantitative theory' (1973, p. 16). In more detail, we start with a claim about the relationship between partial and comparative

[4] For the measurement argument, see especially (Fine 1976; 1973, pp. 15–16, 68–74) and (Stefánsson 2016; 2018); for different versions of the conceivability argument, see (Fine 1973, p. 15–16), (Koopman 1940, p. 270), and (Dibella 2018); for the sufficiency argument, see especially (Stefánsson 2016, p. 574; 2018, p. 381), and to a lesser degree, (Fine 1973, pp. 15–16, 37–42). Hawthorne (2016) makes something analogous to the sufficiency argument to support the claim that the theory of evidential support requires only comparative support relations.

beliefs. Assuming that facts about the relative *strengths* of partial beliefs entail the corresponding facts about comparative beliefs, then you can't have a system of partial beliefs without also having the corresponding system of comparative beliefs. On the other hand, there *appear* to be conceptually possible systems of comparative belief that correspond to no sensible system of partial beliefs whatsoever. Example: supposing that a minimal condition on something coming in different *strengths* is that those strengths at least form a preorder (i.e. a transitive and reflexive ranking), then we just need to imagine that Sally's belief ranking includes something like $P \succsim Q \succsim R \succ P$. So, it seems that having some system of partial beliefs seems to conceptually entail having the naturally corresponding system of comparative beliefs, but it's conceptually possible to have comparative beliefs without having partial beliefs.

This doesn't yet *entail* comparativism, but it's certainly suggestive—and at the very least it rules out one of comparativism's most obvious competitors: the view that facts about comparative beliefs hold in virtue of facts about partial beliefs. Moreover, the conceivability argument can be very naturally combined with a claim about the theoretical sufficiency of comparative beliefs, providing a one-two punch in favour of comparativism. In Stefánsson's words, 'comparative belief can play the [theoretical] role that we want numerical degree of belief to play' (2018, p. 381; cf. 2016, p. 574). Thus, while it's true that numerical representations of systems of partial belief can be more *fine-grained* than their corresponding systems of comparative belief, according to this sufficiency argument any information contained in those representations that isn't entailed by the corresponding system of comparative beliefs is theoretically unnecessary. So not only do partial beliefs entail comparative beliefs but not vice versa, but what's more we don't even *need* to talk about partial beliefs in the cases where they do make sense to talk about!

Well, I'm not convinced. Regarding the conceivability argument, I am happy to accept that there *might* be some conceivable comparative belief states that are incompatible with any conceivable partial belief state. Assuming a close link at least in this case between conceivability and possibility in this case, that would indeed be a compelling reason to think that facts about comparative beliefs cannot supervene on facts about partial beliefs. But that conclusion by itself is obviously compatible with the falsity of comparativism, since it's compatible with a view on which neither partial nor comparative beliefs are more fundamental than the other—either because they're metaphysically independent of one another, or (more plausibly) because the facts about both types of belief state are grounded in something more fundamental still.

For instance, a view that I'm sympathetic to is that partial beliefs *and* comparative beliefs are both ways we have for describing salient aspects of a singular, more fundamental holistic or 'map-like' belief state (cf. Lewis 1994, p. 311). In some cases, perhaps, that holistic state *might* be such that only comparative belief attributions make sense of it, and I'm sure that for some theoretical purposes a belief ranking might contain all the information that's needed. In other cases, though, I think it's likely (for the reasons I'll discuss) that a person's doxastic state might as a whole contain more information—and more theoretically relevant information—than can be represented by any belief ranking alone. The two key questions to ask in relation to the conceivability and sufficiency arguments, therefore, are (i) whether a doxastic state might *conceivably* contain more information than can be represented by a belief ranking, and (ii) whether that additional information is, or at least can be, *theoretically relevant*.

As a response to the conceivability argument, most of the examples in this section can be read as different ways of saying "well, two can play at that game". I'll argue that there *appear* to be conceptually possible states of partial belief (with corresponding, apparently meaningful, differences in cardinal information) that correspond to the same comparative belief ranking. Assuming the very same close link between conceivability and possibility that the conceivability argument relies on, that's a compelling reason to think that facts about partial beliefs cannot be determined merely by the facts about comparative beliefs. Moreover, many of the same examples highlight problems for the sufficiency argument: it seems that the additional cardinal information that is (or can be) stored in numerical representations of partial belief isn't superfluous after all.

3.1 Comparativism and 'almost omniscience'

So let's get started. Suppose to begin with 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 philosophical 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 long as they're not radically imprecise.) So I take it that the situation described is plausibly conceivable; it *seems* to make sense.

However, the SCEC cannot explain how it might be possible that Sally is *much* more confident that the actual world is ω_1 than that it is ω_2. Let $\boldsymbol{\mathcal{B}}$ be any set of propositions with a Boolean structure defined over any space of worlds Ω. Then, a probability function pr will agree with Sally's belief ranking if and only if, for all P in $\boldsymbol{\mathcal{B}}$,

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

Now, the problem here is *not* that there are some probability functions satisfying the stated condition where the difference between x and y is large (arbitrarily close to 1), and some where the difference is very small (arbitrarily close to 0), and either type of function agrees with Sally's belief ranking so either representation is equally accurate. We're not making the mistake of the transformation argument here: the variability in the numbers is only relevant inasmuch as it points to the deeper problem.

The reason that the SCEC cannot give us any explanation of cardinality in this case is that one of the key assumptions needed for the SCEC to get up and running isn't satisfied. To recall, explaining cardinal length comparisons requires an important existential assumption—roughly, that there are plenty of length-duplicates of objects of varying lengths for us to 'add' together. The analogous requirement is not satisfied here: for any proposition P such that ω_1 or ω_2 (or both) belongs to P, there are precisely zero disjoint confidence-duplicates of P, and so there are not enough of the right kinds of propositions around to 'add'. Indeed, since the only disjoint confidence-duplicates for any propositions we'll find will be confidence-duplicates of $\varnothing$ (and hence, Sally will always have as much confidence in their 'sum' as she does in $\varnothing$), the SCEC won't supply us with any sense of cardinal information *at all* in a case like Example 1.

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 is conceivable, and there's some epistemic connection between conceivability claims and possibility claims, then we've found an initial problem for the SCEC. Furthermore, it's not hard to turn this isn't a conceivability argument against comparativism: just imagine that Sally has a friend with the same belief ranking, but who is only a *little* more confident that the actual world is ω_1 than that it's ω_2. Since it's possible for Sally and her friend to have the same belief ranking but distinct doxastic states overall, it follows that in principle there might be more to a person's doxastic state than their comparative beliefs.

Of course, the comparativist could deny that there's any strong link between conceivability and possibility. Personally I'm a big fan of arguments that use this link, so long as they're done right (see Chalmers 2002)—and there's no obvious impropriety in its use here. Moreover, denying the philosophical methodology is going to be a dialectically dangerous game to play inasmuch as the very same methodology is being called upon by in the conceivability argument for comparativism. Likewise, Stefánsson (2016, p. 575), following Eriksson and Hájek (2007), uses the conceivability-possibility link in arguing against alternatives to his comparativist theory.

But there are some other styles of response that might be more interesting. For one thing, one might agree that the described situation is possible, and 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* something further qualitative phenomenon. With more information in the supervenience base, more distinctions will be possible, and perhaps the view we end up with won't be *too* dissimilar from comparativism. 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 stronger version of comparativism.

Another potential response would be to argue that the mere 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 dis-

joint P and Q] reflects an underlying empirical relationship between uncertainties. (1973, p. 24)

That is: claims relating to the presence of cardinal information need to be founded appropriately in theory, and according to SCEC that foundation just *is* the structure of an agent's belief ranking in relation to disjoint unions. Moreover, without such a foundation, who's to say that any intuitions we have regarding this example and other 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.

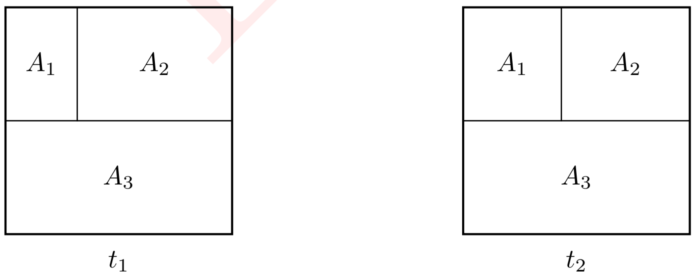
This is a powerful response, and I suspect it's one that many comparativists will reach for. So let's now see whether it's successful.

3.2 Comparativism and decision theory

Say that a proposition P is an *atom* for Sally just in case she has no beliefs regarding any non-empty propositions more fine-grained than P. Furthermore, say that $\boldsymbol{\mathcal{B}}$ is atomic just in case every proposition in $\boldsymbol{\mathcal{B}}$ 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 Boolean algebra $\boldsymbol{\mathcal{B}}$, with atoms A_1, A_2, A_3. Her belief 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 at t_2. A probability function pr agrees with Sally's belief ranking at either time just in case

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

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

Suppose that at t_1 Sally strictly prefers α to β; and at t_2 she has the opposite preferences. Assuming that pr is a probability function satisfying the aforementioned conditions, then 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 in preferences 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.

Comparativists are committed to saying that the change in Sally's preferences *cannot* be due to any changes in her beliefs—after all, there are no such changes. Indeed, since her preferences differ over time for choice situations with the same relevant structure but her beliefs do not, comparativists seem committed to saying that Sally has made an irrational choice at t_1, t_2, or both. But this isn't plausible, as there's nothing apparently *irrational* about either pattern of preferences—neither leads to Dutch bookability, and both are straightforwardly consistent with any plausible set of coherence conditions on preferences that decision theorists have laid down over the past century or so.

Moreover, saying that Sally made an irrational choice at some point would leave us without a satisfying explanation of her preferences in the event that those preferences *consistently* point towards Sally believing A_2 more than twice as much as A_1 at t_1, and less than twice as much at t_2. For example, we could imagine that in the following choice situation, 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 consistently choose '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, and that explanation is strictly off-limits to probabilistic comparativism.

More generally, not only is there a conceivable *cardinal* difference between Sally's beliefs at t_1 and t_2, that difference is doing 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. In this case,then, there's a theoretical foundation for the intuition that it's possible for Sally's beliefs to change between t_1 and t_2 despite there being no change in her comparative beliefs.

Or to put that another way: 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. And those differences *in principle* cannot be explained by reference to Sally's comparative beliefs with respect to unions of disjoint equally-ranked propositions. Indeed, just as with Example 1, the SCEC cannot help us explain *any* cardinal relationships between the strengths of her beliefs for A_1 and A_2 (at either time), since the requisite existential conditions are not satisfied. 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.

3.3 Comparativism and radical Pyrrhonianism

Is the comparativist's explanation of cardinal information at least still successful in those special cases where relevant existential conditions are satisfied and the belief ranking (therefore) agrees with exactly one probability function? Well, no—I don't think so. Consider:

> **Example 3.** Although her belief ranking agrees with exactly one probability function, Sally is *not* an ideal Bayesian agent. After reading perhaps a little too much radical Pyrrhonian literature, she insists that 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 end up true. Moreover, her preferences consistently reflect her new-found Pyrrhonian commitments—for instance, she'd prefer being given \$90 outright to the gamble $\langle\$100$ *if* $P \vee \neg P$, $-\$1000$ *otherwise*$\rangle$, and she prefers $\langle\$100$ *if* $P \vee \neg P$, $\$100,000$ *otherwise*$\rangle$ to being given \$1000 outright.

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

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

Think of pyr as pr squished down by 1% on either side, keeping the order of the propositions fixed. How would this help to explain Sally's preferences? Because those preferences *make sense* given pyr.[5] We may want to say that Sally is *epistemically* irrational, since she fails to attach complete certainty to self-evident tautologies. But, she's 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 just as much as pyr does. The comparativist might say that only one of those functions constitutes an *adequate* representation of her beliefs, but both

[5] Orthodox expected utility theory presupposes probabilistically coherent partial beliefs. Consequently, in order to explain the sense in which Sally's preferences 'make sense' given pyr, we will need to go beyond the orthodoxy. In (Elliott 2017), I describe what Sally's preferences would need to be like in order 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 P and $\neg P$ always sum to 1.

must be *accurate*. And, presumably, any agent with partial 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 being given \$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. It's obvious that have to posit some degree of irrationality somewhere, in order to make sense of Sally's preferences. If we accept that there's an epistemically relevant difference between *pr* and *pyr*, then the former explanation still leaves us with an agent that makes *sense*. It's easy to imagine an agent so committed to radical Pyrrhonianism that they doubt even the most obvious logical truths, being absolutely certain of nothing, and likewise give some positive probability to even the most absurd of contradictions. The latter explanation, on the other hand, leaves us with an agent whose preferences are bafflingly nonsensical given what she supposedly believes.

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 comparativism—the error of re-scaling our model of Sally's beliefs without making appropriate 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 a standard model of preference formation. I agree that this would be an error—indeed, it would be an instance of the very same error that underlies the transformation argument. We can all agree that the scale we use to measure belief is a matter of stipulation. Except for making some of the mathematics easier, 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 the change.

But, importantly, we get to say that *pyr* is a mere *re-scaling* of *pr* only under the assumption that whatever Sally believes least of all, she believes to the least extent possible. This assumption is implicit in the SCEC's construction of a ratio scale: since $P \cup \varnothing = P$ for all P, $\varnothing$ needs to be treated as the identity element on any additive representation of belief (i.e. 0). Yet it's certainly not obvious that it's a mere matter of stipulation that *having at most as much confidence in P as in anything else* is just the same thing as *being 0% confident that P*, since it's not obvious that P's sitting at the bottom of Sally's belief ranking plays the same functional or theoretical role's that 0% confidence is supposed to play. These are substantive claims, and they need to be established by argument.[6]

3.4 Comparativism and non-additive capacities

Suppose you think that Example 3 is too far-fetched, and it's not *really* possible to have less than 100% confidence, and more than 0% confidence, for *every* proposition. 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 the comparativist to pass *all* of these off as mere 're-scalings' of that function.

[6] Compare (Stefánsson 2016 pp. 81–2), where the the assumed equivalence between *being 0% (or 100%) confident that P* and P being bottom-ranked (top-ranked) plays a key role in the argument that comparativism can explain interpersonal comparisons of belief.

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

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

cap is a *capacity*. Capacities are a generalisation of classical probability functions that are sometimes used in descriptively-oriented models of belief and decision making—that is, models aimed at representing the beliefs and/or preferences of ordinary, non-idealised agents (e.g. Schmeidler 1989; Sarin and Wakker 1992). Specifically, a capacity *cap* satisfies the usual Normalisation and Non-negativity conditions that any probability function must satisfy (§2.1), but replaces the Additivity condition with the much weaker condition:

Montonicity. If $P \subseteq Q$, then $cap(P) \leq cap(Q)$

With capacities, the value assigned to the union of disjoint propositions P and Q need not equal the sum of the values assigned to P and Q; that is, capacities might be sub-additive, super-additive, or a mixture of both. Any time a probability function agrees with a belief ranking over any non-trivial algebra, there will be numerous capacities that agree with the same ranking.

There's a number of natural generalisations of ordinary expected utility theory which we can use explain and predict the preferences of agents with beliefs represented by a capacity—e.g. Choquet expected utility theory; and see (Elliott 2017) for an alternative generalisation that further extends to systems of belief represented by capacities as well as non-capacities like pyr. And, importantly, these theories will predict distinct patterns of preference for ordinally-equivalent capacities and probability functions.

Thus, suppose that Sally's belief ranking agrees (uniquely) with the 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:

$$pr(P_1) = {}^{1}\!/\!_{3}, \quad pr(P_2 \cup P_3) = {}^{2}\!/\!_{3}$$

But we could easily imagine preferences over appropriately structured choice situations which suggest that Sally has only a third as much confidence in P_1 as she does in $P_2 \cup P_3$, as represented by *cap*:

$$cap(P_1) = {}^{1}\!/\!_{4}, \quad cap(P_2 \cup P_3) = {}^{3}\!/\!_{4}$$

pr 'adequately' represents the additive structure of Sally's belief ranking, and according to the SCEC, it therefore adequately represents Sally's beliefs. But it's the non-additive *cap* that does a better job of capturing the cardinal information implicit in how her system of beliefs relates to her preferences.

3.5 Comparativism and irrational belief rankings

The function *cap* is a special case of a capacity, in that it's ordinally equivalent to a probability function. Consequently, *cap* agrees with a belief ranking that satisfies each of the conditions (Weak Order, Positivity, Weak Commutativity, Weak Associativity, Qualitative Additivity, and Archimedean) used to support the purported analogy with the measurement of length.

But there are also capacities that seem to do a good job of representing possible belief states, and yet do not have the appropriate structure to support the analogy. In particular, there are many capacities that agree with a belief ranking that falsifies the QUALITATIVE ADDITIVITY condition. For instance:

> **Example 4.** Relative to Sally's comparative beliefs, $P_1, \ldots, P_{100}$ and $Q_1, \ldots, Q_{101}$ are two sequences of pairwise disjoint propositions, where $P_1 \sim \ldots \sim P_{100} \sim Q_1 \sim \ldots \sim Q_{101}$. However, due to an accounting error, Sally has as much confidence in $P_1 \cup \cdots \cup P_{100}$ as in R, and as much confidence in R as in $Q_1 \cup \cdots \cup Q_{101}$. 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 belief 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 belief 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 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 like that which is, apparently, well-represented in *irr*? Why should we say that, when we can plug the capacity *irr* into a decision model that makes room for non-additive capacities and have that information do theoretical work?

The more general challenge here, of course, is for comparativism to plausibly explain cardinality 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 QUALITATIVE ADDITIVITY 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). Some people even seem to commit the double conjunction fallacy, judging $P \cap Q$ to be more probable than both P and Q.

[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 of course aware that the philosophical and psychological literature on the extent of human probabilistic 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 are *not* probabilistically coherent, both with respect to our partial beliefs and 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 is disagreement relating to the conjunction fallacy and other purported examples of probabilistic irrationality, it usually concerns the *extent* of our failings.

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,

$$P = (P \cap Q) \cup (P \cap \neg Q), \qquad (P \cap Q) \cap (P \cap \neg Q) = \varnothing$$

Hence, for the analogy with the measurement of length to hold, we would need:

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

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.

So we have good evidence that ordinary agents' belief rankings sometimes falsify Qualitative Additivity, and consequently don't have the appropriate structure to support the measurement analogy. Yet this surely doesn't prevent them 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.

Thus, a major and still very much unanswered aspect of the more general cardinality challenge is to plausibly explain cardinality in the face of ordinary human irrationality—and it doesn't seem that the SCEC or anything much like it is going to be of much help here. But more on that in a moment.

4 Objections and Replies

In the previous section, I've discussed the limits of the SCEC when the requisite structural conditions are satisfied by the existential conditions 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 cases, the SCEC fails to supply any meaningful cardinal information at all, and in others, it supplies the *wrong* information. So, it seems that comparativism still lacks an adequate response to the *cardinality challenge* after all.

Furthermore, the many of the cases discussed highlight difficulties for both the conceivability and sufficiency arguments. Or to put that another way, comparativism also suffers from a *granularity challenge*—that is, the challenge to draw sufficiently many distinctions (and theoretically relevant distinctions) between conceptually distinct belief states. Partial beliefs, it seems, are more than just a theoretical tool for representing comparative beliefs.

In this section, I'll consider a number of potential objections and responses to the arguments of the previous section.[8] 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, and then I'll briefly flag some potential non-comparativist alternatives to the SCEC.

[8] Thanks are due to John Cusbert, Nicholas DiBella, Jim Joyce, audiences at the University of Leeds and the Australian National University, and to several anonymous referees, for the raising the following points or for closely related discussions.

4.1 Impossible worlds

My argument that the conjunction fallacy is inconsistent with a belief ranking that supports the analogy with the measurement of length relies on the assumption that Ω is a set of *logically possible* worlds, closed (at least) under the classical introduction and elimination rules for negation and conjunction or disjunction. (This assumption is used to guarantee that $(P \cap Q) \cap (P \cap \neg Q) = \varnothing$ 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) belief ranking with respect to propositions drawn from one set of worlds Ω can always be re-expressed as a probabilistically representable belief 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 (2019b) 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 2019b), 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 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 belief 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 belief ranking doesn't support the measurement analogy *exactly* in Example 4, it at least *approximates* a ranking 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 2016, p. 576, fn. 6.)

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 my argument in relation to Example 4 was not that comparativism has no way of making sense of cardinal information in some form or other whenever $\succsim$ fails to satisfy Qualitative Additivity. Of course there are many things the comparativist could say to preserve *some* semblance of cardinality in these cases. That's obvious—but what's not obvious is whether this will be *enough*.

My argument was that comparativism doesn't seem to have any hope of getting us the *right* cardinal information in cases like this; specifically, cases where (i) Sally's belief ranking agrees with only 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 belief ranking in Example 4 will indeed approximate a ranking that agrees with some probability function *pr*; for instance,

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

pr will in turn approximate *irr*. But 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 precisely in its consequences for Sally's preferences. According to *irr*, Sally has the same confidence regarding $Q_1 \cup \cdots \cup Q_{101}$ as she does regarding $P_1 \cup \cdots \cup P_{100}$; according to *pr*, Sally has 1% more confidence regarding the former than regarding the latter. So *pr misrepresents* her beliefs. More generally, any probability function that approximates *irr* will ipso facto *approximately* represent Sally's beliefs, but none of those probability functions will *accurately* represent her beliefs. So how, exactly, are approximations going to help us get at the *right* cardinal information?

If the *only* way to plausibly explain cardinal information were by appealing to belief rankings over disjoint unions, then it would be reasonable to look to approximations whenever $\succsim$ doesn't have quite the right structure to support the measurement analogy—and in that case we'd be forced to accept that the best we can manage is some kind of vague or indeterminate cardinal information. But the central question here is whether the SCEC is on the right track. I'm arguing that it's not, so the response to cases like Example 4 shouldn't presuppose that it *must* be. More generally, the fact that comparativism can apparently only give us indeterminate cardinality in these kinds of cases is not a reason for *non-comparativists* to also agree that they must involve indeterminacy—not, at least, until it has been shown that there's no better alternatives. And as we'll see in §6, there's still plenty of alternatives left to explore.

4.3 Disjunctivism

Next up is what we might call *disjunctivism.* The idea is this: if $\succsim$ doesn't satisfy the requisite structural and existential assumptions needed for the measurement analogy given the assumption that it's the *union of disjoint propositions* that's serving as the doxastic analogue of addition, then perhaps there will be a different operation we could appeal to in those cases instead. We thus have a disjunctive explanation of cardinality: we use belief rankings over unions of disjoint propositions whenever the ranking satisfies the right structural and existential conditions, and look elsewhere when it doesn't.

Note, first of all, that disjunctivism won't help at all with the examples discussed in §3.3 and §3.4, where the belief rankings *do* satisfy all relevant structural and existential requirements. More importantly, if different operations are supposed to explain cardinality for different agents, each one contingent on whatever operation is appropriate for that agent's idiosyncratic belief 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 belief ranking, work out what the relevant operation is, and only then give some doxastic meaning to the statement. Without knowledge of the overall structure of her belief ranking, then, such a claim would only tell us that:

(i) Sally believes P more than Q, and
(ii) There's *some* binary operation $\circ$ that shares structural characteristics with $+$ relative $\succsim$ such that for Q', Q'' where $Q \sim Q' \sim Q''$, $Q' \circ Q'' = P$

The latter is uninformative; and the former we don't need cardinal information to express. And 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.

Moreover, SCEC was supposed to provide us with *essentially* the same kind of explanation as we find in the case of length, mass, and many other extensive physical quantities—but there are no quantities in the sciences where *sometimes* we appeal to one operation as the analogue of addition, and *sometimes* we appeal to another, depending on what works in the moment. One of the main things that the SCEC had going for it was that it was supposed to fit nicely with standard methodologies for explaining cardinal information, but there's nothing like disjunctivism for any quantities elsewhere in the sciences.

Disjunctivism is a non-starter. It won't help to preserve meaningful cardinal information from the problems raised in §3. What disjunctivism leaves us with is not an illuminating explanation of cardinality, but an obviously ad hoc attempt to force a general style of explanation where it doesn't fit.

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 such 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 deidealise at some point later on once we've got a handle on the easier cases. In support of either version of the response though, it's noted that the SCEC *does* seem to work well for ideally rational agents—at least when we set aside cases like those in §§3.1–3.2 where the existential requirements are not satisfied.

Now, there's a reason why the SCEC works particularly well for ideally rational agents, and it has nothing at all to do with comparativism: it's a consequence of a good epistemological theory. According to probabilism, for example, an ideally rational agent will have at least twice as much confidence in P as in Q whenever she has at least as much confidence in P as she does in $Q \cup Q'$, where $Q \sim Q'$ and $Q \cap Q' \sim \varnothing$. This is common ground for comparativists and non-comparativists alike. The question is whether this fact reflects some interesting explanatory relationship between 'twice as much confidence' and comparative beliefs over disjoint unions, or whether it's nothing more than a consequence of a standard epistemological theory.

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 account, 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 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 QUALITATIVE ADDITIVITY. In (Elliott forthcoming), I have shown that it is possible to generalise the SCEC to a limited degree, such that $\succsim$ need only satisfy a weaker condition—what I call R-COHERENCE. However, I furthermore show in that work that R-COHERENCE is a *minimal* condition on any (non-disjunctive) generalisation 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 belief rankings that fail to satisfy the R-COHERENCE condition under very natural assumptions. So it seems that the general strategy of the SCEC might *essentially* require that our comparative beliefs will satisfy quite strong and empirically dubious conditions. Given the problems already noted for generalising the standard explanation by appealing to impossible worlds, approximations, and disjunctivism, it seems that the prospects for generalising the SCEC to non-ideal agents are not particularly strong.

Of course, there might be generalisations we've yet to consider, or strategies for dealing with non-ideal agents that we haven't thought of yet. But that's not very interesting—"maybe we'll come up with something someday" isn't exactly a compelling response to the challenge posed by non-ideal agents. Until we've been given an argument that the SCEC *is* plausibly generalisable, explaining cardinal information in the face of general human irrationality remains a major (and still very much unanswered) problem for comparativism—regardless of their explanatory priorities.

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). Will some variety of supplemented comparativism fare better than non-supplemented comparativism?

There's obviously going to be vastly many possibilities here. As a sample:

- *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 'dependence judgements'.)[9]
- *Evidence-supplemented comparativism*: the facts about Sally's beliefs are determined by her comparative beliefs and her history of evidence.

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

- *Preference-supplemented comparativism*: the facts about Sally's beliefs are determined by her comparative beliefs plus facts relating to her preferences.

So, for example, in relation to in Example 1 the evidence-supplemented comparativist might argue that Sally might have much more evidence that the actual world is ω_1 than that it is ω_2, and hence we could say that she has *much* more confidence regarding the former. Likewise, to generate a distinction between the Sally's beliefs at t_1 and at t_2 in Example 2, then the facts about Sally's preferences could somehow be built into the preference-supplemented comparativist's theory so as to allow for exactly that distinction. A representation theorem similar to that of (Joyce 1999) might help in providing the mathematical foundations for preference-supplemented comparativism. Indeed, with appropriate modifications, those foundations could perhaps even be extended to accommodate agents with preferences incompatible with ordinary expected utility theory.

I cannot discuss each of these in depth, so allow me instead to make two general points. First, with respect to the challenges relating specifically to the *granularity* of belief rankings—i.e., problems arising from the apparent fact that belief rankings are too coarse-grained to capture all of the relevant facts about our doxastic states—it's obvious that some form of supplemented comparativism *might* be better off. Clearly, if $\succsim$ doesn't contain enough information to pin down all the facts about her beliefs, then $\succsim$ *plus* something else might. Or maybe not. It's not clear to me whether supplementing the supervenience base with dependence judgements or facts about evidential histories will suffice to draw enough distinctions, for example, and whether adding facts about preferences will work depends on exactly what facts are added. Without a detailed theory to play with, it's hard to say anything more than 'maybe'.

More importantly, though, supplemented comparativism owes us a compelling and general explanation of cardinal information just as much as non-supplemented comparativism does. And here we can say something a little more substantive: whatever explanation supplemented comparativism might provide us with, it's either going to be substantially different from the SCEC, or it's going to face the same problems that I've been raising for that explanation.

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 in this example, Sally's dependence judgements are such that the unique best 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 has the same belief ranking, but her dependence judgements are such that the unique 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. But what explains the sense that Sally has *much* more confidence that the actual world is ω_1?

The fact that pr assigns a value to $\{\omega_1\}$ that is much greater than the value it assigns to $\{\omega_2\}$ is not an explanation. The reason should by now be clear: the *numbers* are irrelevant. The fact that judgement-supplemented comparativism can *distinguish* between ordinally-equivalent representations of belief does not thereby entail that it has a satisfactory *explanation* of the cardinal information those representations seem to represent. 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, the kind that occurs when one mistakes an ordinal scale for a ratio scale. So to put the question more generally: what, according to judgement-supplemented comparativism, is the real-world relationship that explains the cardinal information apparently present in our beliefs?

Obviously, the explanation can't be in terms of belief rankings over disjoint propositions, simply 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 comparative beliefs can account for the differences in cardinal information between t_1 and t_2, simply because there *are* no differences between Sally's comparative beliefs at t_1 and at t_2. And 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 each of those functions agrees with the same ranking.

Don't say that judgement-supplemented comparativism can offer us a *disjunctive* explanation—sometimes appealing to disjoint unions when that explanation works, sometimes appealing to something else relating to dependence judgements in other cases. "Maybe something else sometimes" isn't an explanation, and we've seen the problems with the disjunctivist move already. So: either judgement-supplemented comparativism provides us with an alternative to the SCEC, or it has no explanation of cardinality in cases of almost omniscience. I don't know what that alternative could be, and frankly I'm doubtful that the judgement-supplemented comparativist will be able to find an explanation that's just as intuitively compelling and consonant with standard methodologies as the SCEC was supposed to be. But in any case, we do at this point have good reasons to conclude that, if judgement-supplemented comparativism *can* come up with such an answer, then it won't be much like the SCEC.

Moreover, the very same points apply, for the very same reasons, to evidence-supplemented comparativism, preference-supplemented comparativism, or any other X-supplemented comparativism you care to think of. The fundamental problem is that the SCEC appeals to comparative beliefs over disjoint unions, and consequently cannot account for differences in cardinal information between in those cases where the comparative beliefs are identical—regardless whatever else we throw into the supervenience base. Nor can the SCEC account for cardinality in cases where the comparative beliefs lack the requisite structure. No 'further fact' view is going to magically make it that the explanation works even when Sally's belief ranking falsifies QUALITATIVE ADDITIVITY.

If the only takeaway messages of this paper are (a) the facts about belief cannot be reduced to just the facts about comparative beliefs, and (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 Doing Without the SCEC

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. Rather, it's clear enough 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.

Luckily for us, there's more than one approach to explaining in purely qualitative terms how partial beliefs might come to have meaningful cardinal information. Denying the SCEC won't 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. Comparativism doesn't have a monopoly on scientifically respectable explanations of such things—the story in §2.2 about the measurement of length exemplifies only one basic strategy for constructing a ratio scale amongst several.

Other methods for constructing ratio scales do not follow the same pattern as the SCEC; that is, they do not require us to locate some operation $\circ$ which shares structural features with $+$. 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 $\succsim^\star$ with respect to some further quantity distinct from either q or q'. In this kind of case, no analogue of addition is necessary: structural properties of the $\succsim^\star$-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 to produce $\succsim^\star$.

Similarly, various dimensionless quantities—e.g. refractive indices, albedos, and decibels—can be defined in terms of ratios of differences in a further quantity q. Consider Mach numbers. Contrary to common opinion, a Mach number is *not* strictly speaking a unit of speed; instead, it is 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 *speed* be any measure of speed on at least an interval scale, then relative to a medium we can define Mach number of an object as:

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

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

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

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 facts about preferences. Instead, the claim need only be that the *meaningfulness* of ratios and intervals of *strengths of belief* is *explicable* by reference to the *typical causal role* that beliefs with different strengths play in relation to other psychological phenomena like preferences. But I've developed and defended that approach in detail in other works, and I'm not going to do it again here.[10]

In any case, though, there are of course still other ideas to explore. Kahneman and Tversky (1979) have linked the general 'Ramseyan' approach to conjoint measurement theory, letting (potentially non-additive) subjective probabilities and utilities simultaneously be defined in terms of their joint interaction in the production of choices. Suppes and Zanotti (1976) have suggested that *comparative expectations* over random variables can be treated as the basic doxastic state, and from these both partial and comparative beliefs can be derived simultaneously.[11] Alternatively, if partial beliefs are really just outright beliefs about objective probabilities, then perhaps whatever cardinality they possess is derivative upon the cardinal information possessed by those probabilities (for which a separate story would need to be told in turn).

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.[12]

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[10] See especially (Elliott 2017; 2019a). As noted above, in the former work I show how the approach described here can deal with non-additive functions like *pyr*, *cap*, and *irr*.

[11] This approach is sometimes conflated with comparativism, but they should be kept distinct—indeed, Suppes and Zanotti were explicitly motivated to develop the view in part because comparativism's expressive limitations (cf. also Suppes 1986).

[12] Many thanks are due to John Cusbert, Nick DiBella, Daniel Elstein, Alan Hájek, Jessica Isserow, Jim Joyce, Jason Konek, Léa Salje, and Jack Woods for helpful discussions and comments on drafts. Thanks are also due to audiences at the Australian National University, the University of Leeds, the 2018 Formal Epistemology Workshop, the 4^{th} PLM conference (Bochum, 09/17), and the 3^{rd} ICM conference (Braga, 10/17). This project has received funding from the European Union's Horizon 2020 research and innovation programme under the Marie Skłodowska-Curie grant agreement No 703959.

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