Chapter 4: Degrees are numerical

One of the central tenets of Bayesianism is probabilism. This is the claim that degrees of belief either do or should satisfy the axioms of probability theory. Taking this literally, it means that degrees of belief are non-negative real numbers, that the degree of belief one has in a proposition one is certain of is 1, and that degrees of belief are additive for the disjunction of two incompatible propositions. However, there are many objections to the idea that this claim can literally be true, basically because it requires degrees of belief to be numbers, and yet numbers aren’t really there in the head. In this chapter I will consider several of these objections and use them to explain what I think should be the correct understanding of probabilism. This will involve ideas from the theory of measurement, both in social science and the philosophy of science, and will investigate the idea of just what is needed for a feature of the world to have a numerical representation.

Zynda’s objection

Lyle Zynda (2000) seeks to qualify the notion of “probabilism”. He focuses on the arguments for probabilism based on decision-theoretic representation theorems (as discussed in Chapter 2), but similar concerns arise for other arguments. As he puts it, the argument relies on three premises:

The Rationality Condition: The axioms of expected utility theory are the axioms of rational preference.

The Reality Condition: If a person’s preferences can be represented with a set degrees of belief that obey the probability calculus, then the person really has degrees of belief that obey the laws of the probability calculus.

The Representation Theorem: A person’s preferences satisfy the axioms of expected utility theory if and only if the person’s preferences
can be represented with a set of degrees of belief that obey the probability calculus.

Although the Rationality Condition is questionable, Zynda focuses attention on the Reality Condition.

He considers four different views that one might have of the sense in which a person “really has” degrees of belief. He calls these, “eliminativism”, “antirealism”, “weak realism”, and “strong realism”, borrowing terminology from the philosophy of mind and philosophy of science. Eliminativism (as familiar about mental terminology particularly in the work of Patricia and Paul Churchland) is the view that a particular concept doesn’t correspond to anything in the world, and isn’t theoretically useful for describing the world either. Antirealism (as familiar about unobservable scientific kinds from the work of Bas van Fraassen) is the view that a concept might be theoretically useful for describing the world, but it either definitely doesn’t correspond to anything in the world, or at best we can never overcome agnosticism about whether it corresponds to anything in the world. Weak and strong realism both hold that the concept does correspond to something in the world, but disagree as to how fundamental the concept is. Their difference is about how fundamental the relevant concept is. As Zynda formulates things the question is whether facts about degrees of belief are fundamentally expressible in terms of facts about preferences (just as facts about centers of gravity are fundamentally expressible in terms of facts about the distribution and masses of parts of an object) or whether degrees of belief exist in some sense independently of preferences and serve to guide them. However, Zynda seems to use the distinction in some ways that seem to presuppose a deeper distinction between weak and strong realism. The challenge Zynda raises for probabilism is the following. First, an eliminativist or even antirealist view about degrees of belief seems insufficient to sustain any interesting form of probabilism. But, he claims, a strong realist can’t justify the Reality Condition in the argument for probabilism, and he suggests that a weak realist must be careful about how to understand the relevant sort of realism in order to justify it. To demonstrate this, he considers two hypothetical agents that he calls Leonard and Maurice. Leonard and Maurice have all the same preferences over options. But where Leonard says he has degrees of belief that satisfy the axioms of probability theory, on a scale from 0 to 1, Maurice does not, and instead says he has degrees of belief on a scale from 1 to 10 that are exactly proportional to Leonard’s. If we take these self-descriptions at face value, then Maurice appears to be a counterexample to the Reality Condition.

Zynda says that there are two options for the realist to respond to this case.

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1Zynda suggests that eliminativism couldn’t be reasonable because the eliminativist is denying the existence of “those features that contribute essentially toward making him or her a person and agent.” However, it seems to me that an eliminativist about degree of belief is likely to insist that it is some other more sophisticated concept that is relevant to personhood and agency, or else to be eliminativist about personhood and agency as well. Consider how eliminativists about phlogiston insist that oxygen is actually the substance that is relevant for life.
in order to save the Reality Condition. First, the realist might say that there is some reason to reject Maurice’s self-description, and say that he actually has the degrees of belief that Leonard reports. Second, the realist might say that in the relevant sense of realism, the degrees of belief that Leonard and Maurice have are the same and that the two different descriptions of them are equally good. Zynda dismisses the first strategy, because he claims that there are no good reasons for preferring Leonard’s self-description over Maurice’s. But on the second strategy, we have to say more about what degrees of belief actually are, and what it means for them to “obey the probability calculus”, if they can equally well be represented on a scale from 1 to 10. As Zynda notes, “this would commit the weak realist to the view that additivity (as defined earlier) cannot be taken literally as a property common to all rational degrees of belief”.

Measurement theory

This seems like a point that one should already be willing to accept for reasons that apply quite generally to numerical representations of any sort. As Krantz et al. (1971) point out, the concepts of length, mass, temperature, and so on aren’t literally numerical either. Rather, in each case, we have numerical representations of some physical structure, and accept that some features of the numerical representation are literally true of the physical structure, while others are just a result of notational conventions. The same is true for degree of belief, and the challenge should be to figure out which aspects of the representation are to be taken literally and which are to be taken as conventions.

For length and mass, what we generally say is that the fundamental physical facts involve certain relations among the bearers of length and mass. There is a fact about when one distance is longer than another and when one object is more massive than another. There is also a fact about when one distance is equal to the “concatenation” of two other distances (that is, the result of laying them end-to-end in a straight line), and when one object is as massive as the “concatenation” of two other objects (that is, the object that is the fusion of these two objects). We then have a convention in each case that greater distances or masses should be represented by larger numbers, and that a distance or mass that is equal to the concatenation of two others should be represented by the sum of the two numbers representing those others. Given some basic facts about how length and mass comparisons and concatenations behave, one can then prove two things. First, there does exist a numerical representation of length and mass satisfying these properties. Second, any two distinct numerical representations of length, or of mass, are constant multiples of each other. This is familiar from the fact that lengths can equally well be

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Zynda seems to suggest that a strong realist would have to recognize some difference between Leonard and Maurice that isn’t compatible with the Reality Condition, but he doesn’t fully argue for this claim. Instead, his primary argument against strong realism comes from the idea that the connection between degree of belief and preference must be a necessary connection and not merely a normative one.
represented in kilometers or miles or inches or centimeters, and that masses can equally well be represented in kilograms or pounds (assuming we are careful about the distinction between weight on Earth and mass).

For temperature, the situation is rather more complicated, because there is no simple notion of “concatenation”. Instead, thermodynamics gives some ways of understanding when the “difference” between one pair of temperatures is greater or less than that between another pair of temperatures. We standardly ask that any scale for temperature represent pairs of temperatures with the same difference with numbers that have the same numerical difference. Most people are familiar with the fact that temperature can equally well be represented on the centigrade and the Fahrenheit scale. The number “0” represents a different temperature on each scale (the freezing point of water on the centigrade scale, and a somewhat lower temperature on the Fahrenheit scale), and two temperatures with a difference of 5 degrees centigrade have a difference of 9 degrees Fahrenheit. For many scientific purposes, it is more useful to represent temperature on the Kelvin scale which has the same differences as the centigrade scale, but puts “0” at the point of “absolute zero” rather than the freezing point of water. But for other purposes, it is sometimes even more useful to represent temperature with the reciprocal of its value in Kelvin, sometimes called the “coldness” or the “perk” of the system. This reciprocal scale doesn’t represent constant differences with numbers that have the same difference, but does represent other features of the energy distribution in a substance more directly.

In general, the work of Krantz et al. (1971) suggests that for any numerical representation of a quantity, we should first identify the underlying features of that quantity that should be preserved in the numerical representation, second make some conventional choice of how those features should correspond to numerical relations, and third demonstrate that the quantity obeys some axioms that suffice to prove the existence of such a representation, and show how to calculate the values under one representation from the values under another representation. For length and mass, we have chosen as a convention that concatenations are represented additively and that increasing length or mass corresponds to increasing number. Although numerical representations subject to this convention are not unique, they are unique up to constant multiple. Importantly, we could represent concatenations multiplicatively instead of additively, and there would also exist representations (which would be the exponentials of the representations under the standard convention) and they would be unique up to raising all values to the same exponent. For temperature, we have chosen as a convention that increasing temperature corresponds to increasing number, and a second convention that fixes differences of temperature as being represented uniformly. Under these conventions, there exists a numerical representation of temperature, and any two such representations

\[ \text{This reciprocal represents the rate at which occupancy of states in our thermodynamic system declines as we consider states of higher energy, and thinking in terms of this reciprocal helps explain why it is impossible to reach \textit{absolute zero}, because it would require this quantity to actually be \textit{infinite}.} \]
differ as Fahrenheit and centigrade do, with a change of zero point and a size of unit. But with different conventions (increasing temperature corresponding to *decreasing* number, and some other features involving entropy and energy distribution), we find that there also exists a numerical representation (such as the reciprocal of the temperature in Kelvin) and that any two such representations differ only by a constant multiple.\textsuperscript{4}

### Measuring degree of belief

With this discussion, we can return to consideration of Zynda’s example of Leonard and Maurice. As Zynda himself notes, to convert between Leonard’s scale and Maurice’s, we can just multiply by 9 and add 1, so this conversion is just like that between centigrade and Fahrenheit (multiply by 9/5 and add 32). From the perspective of measurement theory, this suggests that we should first identify the underlying features of degree of belief that should be preserved in a numerical representation, second make some conventional choice of how those features should correspond to numerical relations, and third demonstrate that the quantity obeys some axioms that suffice to prove the existence of such a representation, and show how to calculate the values under one representation from the values under another.

And this is exactly what the traditional decision-theoretic representation theorems do. The underlying features of degree of belief that should be preserved are the way that degree of belief and utility combine to determine preference. The conventional choice of how these features correspond to numerical relations is that this determination should proceed by the standard calculation of expected value. The only slight difference from representation theorems for mass, length, and temperature is that the axioms that preferences are said to obey are merely *normative* for preference rather than *necessary*, but the axioms suffice to prove the existence of a numerical representation of degree of belief, and with the given convention, the representation is in fact *unique*.

Zynda’s observation about Leonard and Maurice then shows that if we relax the conventions of representation so that we only require that degree of belief and utility combine in a linear way (rather than specifically by means of expected value) to determine preference, then there will exist multiple numerical representations that are related to each other by linear transformations like the one relating Leonard and Maurice’s degrees of belief. If we relax the conventions further, so that the way degree of belief and utility combine to determine preference can be more general, then there will be even more numerical representations.

One particularly perspicuous representation that is sometimes used is the *odds* representation — if the probability of a proposition is \( p \), then the odds of that proposition are \( p/(1−p) \).\textsuperscript{5} These odds are sometimes stated in terms of

\textsuperscript{4}This connection between temperature and “coldness” scales is an instance of the connection Krantz et al. (1971) note between “ratio scales” and “log-interval” scales.

\textsuperscript{5}In continental Europe, it is traditional to use the value \( 1/(1−p) \) instead of \( p/(1−p) \), but
integers in the numerator and denominator in some representation of this value as a fraction, with the numerator first being said to be the odds “in favor” while the denominator first being said to be the odds “against”. Thus, at a racetrack, a horse that is estimated to have a 1/4 probability of winning a race will be said to be at odds of 3 to 1 against, and one that is estimated to have a 2/11 probability of winning is said to be at odds of 9 to 2 against. (It is traditional to list the odds against rather than the odds in favor, because it is rare for any one horse to have probability more than 1/2 of winning, and thus the larger number is usually first with the odds against.) Determining preferences from odds and utilities is substantially more tedious than determining preferences from probabilities and utilities, but since the odds and probabilities are related in a one-to-one way, it can be done.

On this decision theoretic account, we can thus say that the fundamental facts are ones about preference, and that there are numerical representations of degrees of belief and utilities that can be determined (subject to some conventions) from those preferences. This basically lines up with the type of weak realism that Zynda endorses. However, it does not seem to be compatible with strong realism, on which degrees of belief are more fundamental than preference. Furthermore, if the axioms that go into the representation theorems are not actually required for a set of preferences to be rational, then this argument won’t work at all.

Probabilities from accuracy

Some of these worries were the motivation for the discussion in the previous chapter of accuracy as a primary condition on confidence. However, Joyce (1998)’s argument (as well as that of Leitgeb and Pettigrew (2010), and the further work Pettigrew has done) presupposes a numerical representation of degree of belief for the accuracy function to work with. Importantly, the conditions that Joyce, Leitgeb, and Pettigrew assume for the accuracy function are such that only probability functions could fail to be dominated in terms of accuracy by other degrees of belief. Thus, if these conditions are correct, then there must be something irrational in having Maurice’s degrees of belief, or in having degrees of belief that correspond to odds rather than probabilities. But given the easy ways of converting between these different numerical representations, it seems that this must be the result of some sort of mistake.

What is real about degrees of belief must be what is shared between these different representations, and not the numerical facts themselves. And this is the main conclusion that Zynda motivated as well for any defender of probabilism. Although Zynda himself endorses a decision-theoretic justification of the resulting features of degree of belief, and thus weak realism about degrees of conveniently, $1/(1 - p) = 1 + (p/(1 - p))$, so these values are easy to convert back and forth. From the European odds, it is easy to calculate the probability, since $p = 1 - (1/o)$, where $o$ is the European odds. Thus, all three of these representations can be calculated from each other relatively straightforwardly.
belief, it seems that a strong realist who wants degree of belief to play a role in guiding preferences (rather than being determined by them) should also find some way to justify the same weaker conditions on degree of belief.

On p. 65, Zynda states several qualitative conditions degree of belief must satisfy if probability is to be one representation of it. These are phrased in terms of a relation “⪰”, where “A ⪰ B” is interpreted as meaning “the agent is at least as confident of A than of B”. These conditions include A ⪰ F, where F is any proposition of which the agent is certain that it is false; T ⪰ A, where T is any proposition of which the agent is certain that it is true; and a condition Zynda calls “qualitative additivity”: if A ⪰ B and A and B are both disjoint from C, then (A ∪ C) ⪰ (B ∪ C). However, as Joyce notes in the last two pages of his (1998), even all of the qualitative principles Zynda mentions are not sufficient to guarantee the existence of a numerical representation of degree of belief by a probability function, without a further condition called “Scott’s Axiom”, which is fairly complicated, and not obvious how to directly justify.6

Fortunately, there is another historically important theorem of probability theory that can guarantee the existence of a numerical representation for degrees of belief that satisfy the probability axioms. This is Cox’s Theorem, which was first developed by Cox (1946), but has been phrased more precisely by other authors since then.7 The first assumption of Cox’s theorem is that there is a function that relates the degree of belief one has in a proposition to one’s degree of belief in its negation. That is, for any two propositions, if one has the same degree of belief in those propositions, then one has the same degree of belief in their negations. Furthermore, Cox assumes that this function is decreasing.8

The second assumption of Cox’s theorem is that there is a function relating the degree of belief one has in a proposition A, and the degree of belief one has in another proposition B conditional on A, to the degree of belief one has in their conjunction. That is, the degree of belief in A and the degree of belief in B conditional on A suffice to determine the degree of belief in the conjunction A ∩ B. Furthermore, this function is assumed to be increasing9 and continuous.10 If these assumptions are supplemented with a further technical

6Scott’s Axiom can be formulated as follows. Let X₁, . . . , Xₙ and Y₁, . . . , Yₙ be 2ⁿ propositions, such that in every epistemic possibility, exactly the same number of X’s are true as Y’s. Then there is at least one i such that Xᵢ ⪰ Yᵢ, and also at least one i such that Yᵢ ⪰ Xᵢ.

7In fact, Halpern (1999a) shows that Cox’s original proof was in fact incorrect, and the conditions Cox claimed to be sufficient were in fact insufficient to prove the result. In fact, Paris (1994) was the first to provide a list of assumptions that are in fact sufficient to prove the result. Halpern (1999b) gives a fuller analysis of sets of assumptions that are sufficient, though I won’t go into all the technical details, since they aren’t necessary for my purposes. Interested readers who are willing to work through some very technical mathematics can consult those works.

8If one is more confident in A than in B, then one is less confident in the negation of A than in the negation of B.

9If one is more confident in A’ than in A and equally confident in B conditional on either A or A’, then one is more confident in A’ ∩ B than in A ∩ B; if one is more confident in B’ conditional on A than in B conditional on A, then one is more confident in A ∩ B’ than in A ∩ B.

10For any degree of belief d between the degrees of belief one has in A ∩ B and A ∩ B’, there is some B” such that one’s degree of belief in A ∩ B” is d, and similarly for a degree of belief
assumption, (Halpern, 1999b) then one can prove that degrees of belief can be represented by a unique probability function. The degrees of belief themselves might be Maurice’s, or they might be odds, or they might not be numerical at all, but they can be represented by a probability function, and thus in any of the other numerical ways that correspond to a probability function as well.

Many authors (particularly physicists like Jaynes (2003)) have taken Cox’s theorem by itself to be their central argument for probabilism. However, this argument would be akin to taking the representation theorem for decision theory by itself as an argument for probabilism. One needs to justify the claim that degree of belief ought to satisfy the assumptions made in Cox’s theorem, and these assumptions are technical enough that it’s not obvious how one might go about doing this.

Fortunately, the central result of Lindley (1982) happens to do exactly what is needed, when translated into the language of Joyce (1998). Assume that degrees of belief are linearly ordered (so that for any two distinct degrees of belief, one degree is a “greater” degree than the other, and this notion of “greater” is transitive). Assume that there is a function that assigns a numerical degree of accuracy to each degree of belief depending on whether it is held in a proposition that is true or one that is false. Assume that the function for the accuracy of holding a degree of belief in a truth is increasing and the one for a falsehood is decreasing. From these assumptions, we can see that for any numerical value of accuracy, there is at most one degree of belief that has that value of accuracy when it is held in a true proposition, and this degree of belief has a particular value of accuracy when it is held in a false proposition. This gives us a function $F$ from numbers to numbers that relates the value of accuracy in truth to value of accuracy in falsehoods. That is, if there is some degree of belief whose accuracy is $x$ if held for a true proposition, then its accuracy is $F(x)$ if held for a false proposition.

Assume further that for any value of accuracy, there is some degree of belief that has that accuracy when held in a true proposition. (This is a strong assumption — it implies that there are infinitely many different degrees of belief, and that the function $F(x)$ mentioned above is continuous.) Finally, assume that $F$ is differentiable, that its derivative is continuous, and that the limit of this derivative as $x$ approaches perfect accuracy is 0. Lindley shows that under these conditions, degree of belief satisfies the assumptions of Cox’s theorem. Thus, degree of belief can be numerically represented by a probability function, or an odds function, or any of the other standard representations, or even non-standard ones like the one Zynda attributes to Maurice.

Lindley’s theorem has weaker assumptions than Joyce’s. However, even though we can dispense with the initial assumption that degree of belief is represented by real numbers between 0 and 1, we still need assumptions about the accuracy of those degrees of belief being represented by real numbers, and some of those assumptions are quite technical. For this procedure to serve as a fully adequate argument for probabilism (in the broader sense of establishing the

between those of $A \cap B$ and $A' \cap B$.}
qualitative results that ensure a representation by a probability function, rather than in the narrower sense of establishing that degrees of belief are probabilities) we would need to justify this set of assumptions as well. This is similar to the situation for decision-theoretic arguments for probabilism, where we need a proper argument that all of the supposed rationality conditions for preference really are rationally required.

**Interpersonal comparisons**

**Odds from confirmation**

I’ll write these sections in later drafts of this chapter.

**Holton’s objections**

Richard Holton (2015) accepts that there is a notion of “partial belief” that comes in degrees (so that we can be more or less confident of some propositions) but denies that these partial beliefs are anything like precise enough to be faithfully represented by probabilities. He suggests that for agents with extremely sophisticated and precise mental mechanisms, credences (along with decision-theoretic preferences) could be used to make flexible plans and might enable more careful reasoning and action. However, for more limited agents like us, we need the ordinary notions of belief and intention to govern our reasoning and action. (This is the view that Dallmann (2015) works out in greater detail for agents that do have credences, but lack the computational capacities to update them fully rationally in light of all relevant evidence.)

The issue I will focus on here is the set of objections Holton raises to argue that his “partial beliefs” are not probabilistic. While to an extent this is an empirical argument that would require very detailed psychological evidence to fully settle, I will argue that Holton’s objections can all be answered, once we think of degrees of belief in the right way. To say that they are probabilistic is merely to say they have some sort of structure of one of the sorts mentioned above, which ensures that it can be represented numerically. It does not require the level of introspective ability or perfection in planning that Holton seems to suggest that it does.

First, Holton argues against Zynda’s weak realism (citing Eriksson and Hájek (2007)), suggesting that degrees of belief should not be mere behavioral constructs out of preference or action, but instead “should be understood as the states that cause, or in some other way give rise to, such behaviour.” However, Holton says, “the mental states involved here are nothing like credences. They do not typically register anything like numerical degree of probability; we cannot manipulate them; they do not obey anything like the Principal Principle.”

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11He also makes similar suggestions in his earlier (2008), but the quotes in the main text are all from his (2015).
He says, “It is possible that people had been entertaining and manipulating credences for millennia . . . without realizing what they were doing, but it strikes me as implausible.” I don’t find this any more implausible than that people had been experiencing cognitive dissonance and confirmation bias and risk aversion and REM sleep before those concepts were analyzed by psychologists.

He goes on to argue, “If we are to be good subjects for the ascription of probabilistic attitudes, then we should be able to make the kinds of transitions — whether in beliefs or in behaviour — that would be expected. Yet a host of now very familiar research shows that, in many cases, we are very bad at this.” He cites research by psychologists showing that people do quite badly at reasoning with probabilities, including people with extensive scientific and mathematical training, like doctors. However, all this shows is that people are bad at manipulating the explicit mathematics of probability theory. I don’t think it shows anything about whether our degrees of belief are enough like probabilities to be accurately described this way. The visual system has a set of representations corresponding to three-dimensional space, with some implicit understanding of gravitational dynamics — enough to allow people to catch a ball. But humans are notoriously bad at actually solving differential equations, even when they are the ones that appear to be at work in the visual system. The problem is that explicit representations of differential equations are in no way easy to translate into the psychological representations that the visual system uses. Similarly, I claim that our degrees of belief could in fact be just like probabilities, and yet as long as it is difficult to correctly translate claims about probabilities into our native representation of the strength of belief, we can’t take advantage of this fact to solve the problems Holton is worried about.

The notion of “partial belief” that Holton is willing to endorse is somewhat weaker — it consists primarily in the idea that some possibilities are “live”, and that in certain cases we must plan for more than one contingency. “Normally things are clear enough for us to make do with a simple plan. But sometimes we need contingency plans, that will depend on how things turn out. And again this is an old idea, long predating probability theory. Keeping such complex plans in mind is costly, and the complexity quickly ramifies; so we only do so when the need is really pressing.” I find this highly implausible - surely we always have this sort of contingency plan going on, even when these alternatives aren’t live. We put on a seat belt. We are polite to strangers we expect to never see again. At some point we draw up a will. We enroll in insurance plans. And given that we have some sensitivity to these possibilities on some level, and often behave in approximately appropriate ways to the different levels of possibility of unlikely situations, it seems to me that there must be some sort of gradation that is more specific than the mere partial belief that Holton works with.

I certainly don’t have a convincing argument that we humans actually do have degrees of belief with the structure required for one of the above arguments. But I think Holton is far from establishing that we don’t. And I don’t see that Holton’s use of his notion of “partial belief” in explaining the role of full belief, planning, and the like requires that partial belief not be structured like probability. Due to the work of Dallmann (2015) and others, I suspect that
roles much like the ones Holton is interested in could still be of relevance for creatures with probabilistic degrees of belief, whether or not we actually are such creatures.

**Imprecise probability**

One thought that is quite natural, and that probably motivates Holton’s concerns, is that regardless of whether numerical representation is actually part of the doxastic states (and presumably it is not), it seems implausible that our minds could actually have enough different states to correspond to all of the numbers between 0 and 1, as any of the above strategies seem to suggest. Many theorists have been motivated by the idea that although degrees of belief might be like probabilities in some way, they can’t be fine-grained enough to actually be probabilities, even in the attenuated sense suggested here.

However, much of the recent philosophical literature on this topic deals primarily with the idea of probabilities not as single numbers but instead as sets of numbers. (See, for instance Sturgeon (2008) and the reply by White (2009); Hájek and Smithson (2011); Elga (2010) and the reply by Moss (2015); and Schoenfield (2016).) Some of the earlier literature (including some work by Isaac Levi and Wolfgang Spohn) as well as much of the literature among statisticians (starting at least with Walley (1991)) is somewhat more sensitive in considering other alternate mathematical representations of degree of belief. The representation by sets of numbers, while it can be motivated by some formal considerations, doesn’t actually do anything to help address Holton’s concerns.

On a weak realist view of degree of belief, there is a natural way for these sets of real numbers to arise. The standard representation theorems get unique numerical degrees of belief out of an agent’s preferences by assuming that the agent has preferences over an extremely rich collection of potential actions, most of which she will never encounter as options. Some authors (for instance Hawthorne (2015)) don’t assume that the agent actually has all these preferences, but instead just assume that the agent’s preferences can consistently be extended to all of these actions. There may be more than one such consistent extension, and each will come with its own numerical representations. There is some sense in which none of these numerical representations is an adequate representation of the degrees of belief constructed out of the agent’s preferences, because any one particular representation only corresponds to some of the preferences that could consistently extend her actual ones. The only way to faithfully represent the difference between the preferences she actually has and the ones that she could consistently be extended to have is to use the full set of numerical representations. However, on this picture, it’s misleading to say that the agent actually has degrees of belief that are adequately represented by these sets. The degrees of belief are just a mathematical construct, and the preferences are the real thing.

For the strong realist about degrees of belief however, this will be more difficult. Recall that for the strong realist, there actually are such things as the
degrees of belief the agent has in various propositions, and these degrees pro-
vide some guidance for the agent’s preferences (among other roles they might
play). It may well be that these actual degrees of belief fail to satisfy all the
assumptions needed for one of the above theories of measurement to represent
them as real numbers that satisfy the probability axioms. But it’s far from ob-
vious that sets of real numbers will provide better representations. Elga (2010)
argues that it is at least hard to get this sort of representation from decision-
theoretic resources (Moss (2015) shows that there are consistent decision rules
that one could use to guide one’s actions if one could independently establish
that degrees of belief correspond to sets of probabilities, but doesn’t say how
to do this). Both Lindley (1982) and Schoenfield (2016) argue that accuracy
arguments can’t establish a theory of measurement with sets of probabilities as
the representation. And there is no obvious way to modify the confirmation-
theoretic argument to arrive at this sort of representation either.

Thus, while it is certainly right that philosophers ought to consider representa-
tions of degrees of belief other than ones satisfying the axioms of probability
theory, it’s far from obvious that sets of probabilities are the right way to go.

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