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From Values to Probabilities

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Abstract: According to the Fitting-Attitude Analysis of value (FA-analysis), to be valuable is to be a fitting object of a pro-attitude. In earlier publications, setting off from this format of analysis, I proposed a modelling of value relations which makes room for incommensurability in value (Rabinowicz 2008, 2009a, 3009b, 2013). In this paper, I first recapitulate the value modelling and then move on to suggest adopting a structurally similar analysis of probability. Indeed, many probability theorists from Poisson onwards did adopt an analysis of this kind. This move allows to formally model probability and probability relations in essentially the same way as value and value relations. One of the advantages of the model is that we get a new account of Keynesian incommensurable probabilities, which goes beyond Keynes in distinguishing between different types of incommensurability. It also becomes possible to draw a clear distinction between incommensurability and vagueness (indeterminacy) in probability comparisons.

Keywords: Value; probability, incommensurability; parity; incomparability; vagueness, indeterminacy, imprecision, uncertainty, doxastic state, credence; preference; Normativity; permissibility; Keynes

According to the Fitting-Attitude Analysis of value (FA-analysis, for short), to be valuable is to be a fitting object of a proattitude. In earlier publications, setting off from this format of analysis, I suggested a modelling of value relations which makes room for incommensurability in value (Rabinowicz 2008, 2012; see also 2009a, 2009b). In this paper, I first recapitulate this proposal (Section 1) and then move on (in Section 2) to suggest that one might adopt a structurally analogous analysis of probability. Indeed, many probability theorists from Poisson onwards did adopt an analysis of this kind.

Interpreting probability on these lines allows to formally model probability relations in essentially the same way as relations of value. One of the advantages of the model is that we get a new account of Keynesian incommensurable probabilities. Indeed, it becomes possible to move beyond the Keynesian approach: We can introduce distinctions between different types of incommensurability that Keynes did not make. We can also use the model to draw a clear distinction between probabilistic incommensurability and vagueness (indeterminacy) in probability comparisons (Section 3).

In a sense, this paper follows a well-trodden path: It shows how formal machinery can be transferred from one area to another. That such transfers are possible is, of course, one of the advantages of formalization. What is new, however, is the direction of transfer. Ethics often takes over formal models from logic, decision theory, economics or political science. But the influence rarely goes in the opposite direction: from ethics to other disciplines. In this respect, then, my paper is different. I hope to show that formal epistemology can profit from formal ethics, and – more specifically – from value theory.

1. *Values*

According to the FA-analysis of value, an item is valuable if and only if it is fitting to favour it. Here, "favour" is a place-holder for a proattitude, while "fitting" stands for the normative component in the analysis. Other terms that have been used for the normative component are "warranted", "appropriate", "required", "ought", etc.

FA-analysis should be seen as an analytic schema than as a fully specified analysis, not least because its attitudinal component can vary: favouring might consist in desiring, preferring, admiring, etc. Depending on what proattitude is deemed to be fitting, we get different kinds of value: desirability, admirability, etc. One of the advantages of FA-analysis is that it demystifies value, at least to some extent: It rejects the Moorean view that values are indefinable properties (see Moore 1903) and it makes clear how and why value judgments can be normatively compelling: An object is valuable precisely insofar as we are ought to respond to it in a positive way.˙ Also, as it stands, the analysis is compatible with competing metaethical views concerning the nature of normativity and the semantics of normative utterances. It is neutral between ethical naturalism (of a subjectivist or an objectivist variety), ethical non-naturalism and non-cognitivism. These views involve competing semantic accounts of the normative component in the FA-analysis, but they are all compatible with the analysis in question. Another advantage of the analysis is that it is so friendly to value pluralism, as we already have seen (different kinds of value correspond to different kinds of fitting proattitudes), and, relatedly, to a pluralism concerning the nature of value bearers: Value can be assigned to states, events, actions, persons, concrete things, ecological systems, etc., since all these ontologically different items can be objects of proattitudes.[1]

[1] These two forms of pluralism should not be confused, however, with a more familiar 'substantive' value pluralism. On that view, even if we keep the kind of proattitude and the ontological category of the value bearer fixed, there may be different and irreducible types of objects within this category that call for the attitude in question. In this sense one might be a pluralist about, say, political values, such as freedom, equality and justice.

FA-analysis is readily applicable to standard value relations: betterness and equal goodness. On the FA-account, such value comparisons may be seen as normative assessments of preference. Thus, one item is *better* than another if and only if it is required (fitting, appropriate) to prefer the former to the latter.[2] Two items are *equally good* if and only if it is required to equi-prefer them, i.e., to be indifferent. This approach to betterness and equal goodness goes back to Brentano (1889), who may be considered to be the founding father of the FA-account of value.[3]

But what if none of these standard relations obtains? To fix terminology, let us say that two items are *commensurable* in value if one is better, worse or equally as good as the other (with 'worse' being the converse of 'better'). Otherwise, they are *incommensurable*. Ruth Chang has argued that two items which are incommensurable in the sense just defined might still be comparable in value. They might be, as she puts it, *on a par*.(cf. Chang 1997, 2002a, 2002b, 2005).[4] Thus, consider two great artists, say, Mozart (*x*) and Michelangelo (*y*). They are comparable in their artistic excellence and it is plausible to deny that one of them is a better artist than the other. However, they are not equally good artists either, as shown by what Chang calls "the Small-Improvement Argument". Thus, let's imagine a third artist, x^+, slightly better than *x* but otherwise very similar to *x*. x^+ is a fictional figure – a slightly improved version of Mozart – perhaps Mozart who composed yet another *Requiem* and a couple of additional operas. Now, by hypothesis, x^+ is a better artist than *x*. If *x* and *y* were equally good, then anything that's better than one of them would have to be better than the

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All these things fall within the same ontological category and all of them might be deemed to be independently desirable. Needless to say, this kind of substantive value pluralism is fully compatible with the FA-account. I am indebted to Richard Dietz and to an anonymous referee for asking me to clarify this issue.

[2] Preference can be understood as a dyadic attitude or as a degree comparison between two monadic attitudes of favouring/disfavouring. On the latter approach, preference for *x* over *y* consists in favouring *x* to a higher degree than *y* (or disfavouring *x* to a lower degree than *y*). See Rabinowicz (2012) for an elaboration of this distinction.

[3] However, instead of requiredness, Brentano appealed to the notion of *correctness*. That Brentano's correctness is a normative cencept has been questioned by Danielson & Olson (2007). On their suggestion, correctness should instead be understood as analogous to truth. Does this mean that a proattitude is correct insofar as it is true to the value of its object? If so, then such a 'representational' interpretation of fittingness will make the FA-account circular. An alternative might be to treat correctness (representational fittingness) as a basically primitive notion. In this paper, I focus on the normative interpretation instead.

[4] For some notions close to Chang's parity, cf. Griffin's (1986, pp. 81, 96ff) "rough equality", and Parfit's (1984, p.431) "rough comparability".

Concerning my usage of "incommensurability": In the ordinary parlance, "incommensurability" means that a common scale of measurement is missing for items under consideration. Which does not exclude that one of them might be better than the other. Thus, my usage differs the ordinary one. I would have used the term "incomparability" instead, were it not for the fact that, if Chang is right, two items might still in some sense be comparable in value, even if none of which is better than, worse than or equally as good as the other. I am indebted to an anonymous referee for pressing me on this point.

other. But, intuitively, x^+ is not better than y: the slightly improved Mozart is not a better artist than Michelangelo. Indeed, it seems that x^+ and y are on a par, just as x and y.

Just as equal goodness, parity is a symmetric relation, but – unlike equal goodness – this relation is not transitive. In our example, x^+ is on a par with y, y is on a par with x, but x^+ is not on a par with x: it is better than x. Again, unlike equal goodness, parity is irreflexive: No item is on a par with itself.

The Mozart-Michelangelo-Mozart$^+$ appraisal is a variant of Chang's own example, but there are many other cases with the same structure. My own favorite is a comparison between two holiday trips, one to Australia (x) and the other to South America (y). We might well want to deny that one of these trips is better than the other. And we can use the Small-Improvement Argument to establish that the two trips are not equally good. As for x^+, we can think of it as the trip to Australia with a small discount of, say, \$100. x^+ is better than x,[5] but it is not better than y.[6]

As it stands, the Small-Improvement Argument is incomplete. To begin with, it doesn't take into account possible *knowledge gaps* regarding values. Perhaps we simply don't know (or maybe even cannot know) whether Mozart is better as an artist than Michelangelo, worse, or equally as good. But then, for all we know, one of these possibilities might still obtain, i.e., Mozart and Michelangelo might still be commensurable in their value as artists. Nor does the argument take into account potential *vagueness* in value comparisons. Here I am assuming that vagueness is not the same as knowledge gap, *pace* Williamson (1994). We have to do with vagueness or, to use another label, *indeterminacy*, when there is no fact of the matter that makes a sentence true or false: Vague sentences lack truth-value. We are unwilling to say that Mozart is better as an artist than Michelangelo, worse, or that they are equally as good. But perhaps this is so only because it is *indeterminate* which of these three value relations obtains between them? Then it might still be determinate that one of these relations does obtain, i.e., that they are commensurable. A disjunction might be determinately true even if every disjunct is indeterminate in its truth value.

Chang is of course aware of these potential objections to her argument. She has tried to show that at least some purported cases of parity cannot be explained away as instances of

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[5] If it is objected that a discount does not make a trip better but only cheaper, then one might let x^+ instead be the trip to Australia with an additional exhilarating bush-walk.

[6] Prior to Chang, the Small-Improvement Argument was proposed by several other philosophers: de Sousa (1974), Broome (1978), Sinnott-Armstrong (1985) and Raz (1986). Raz (1986, p. 326) calls the possibility of such a 'one-sided' improvement "the mark of incommensurability".

vagueness or as mere gaps in knowledge (see Chang 2002b). I won't take a stand on this matter. While I am inclined to believe that cases of parity exist, I don't know how to prove it. Instead, my objective is to provide a *logical* space for parity and for other kinds of incommensurability, by developing a model that has room for all these possibilities.

If parity is, as Chang suggests, a fourth form of comparability in value, along with betterness, worseness and equal goodness, we are left with two notions that need elucidation: parity and comparability.

Gert (2004) proposed to define parity using a variant of the FA-account. On that account, as we have seen, value comparisons are normative assessments of preference. The novelty of Gert's approach rests on the observation that there are two levels of normativity—the stronger level of requirement ('ought') and the weaker level of permission ('may'). Permission is the dual of requirement: something is permitted if and only if its absence is not required. It is the availability of permission that provides conceptual room for parity: *x* and *y* might be said to be *on a par* if and only if it is permissible to prefer *x* to *y and* likewise permissible to have the opposite preference. (For this definition, see Rabinowicz 2008. While Gert's original definition of parity also made use of the distinction between two levels of normativity, it was unacceptably demanding; it implied, for example, that parity was a transitive relation.)

Intuitively, we might expect to encounter cases of parity when comparisons between items involve taking into account different dimensions or respects. An item *x* might be superior to an item *y* in one respect, but inferior in another respect. The all-things-considered preference we arrive at will then depend on how we weigh the relevant respects against each other. Different weight-assignments might be admissible and sometimes might lead to opposing permissible all-things-considered preferences, i.e. to instances of parity in value.

To forestall possible misunderstandings, it should be noted that preferring one item to another is not the same as judging it to be better.[7] Preference is a conative attitude; it is not a judgment. Thus, it is not inconsistent to prefer *x* to *y* even though one at the same time judges *x* and *y* to be on a par, i.e. even if one considers the opposing preference regarding these two items to be just as admissible as one's own. We are capable to recognize our assignments of weights to different respects of comparison as being optional to some extent. If preference is understood as a dyadic attitude with an experiential content, then it might be argued that

[7] Cf. Rabinowicz (2008). I am indebted to an anonymous referee for pressing me on this issue.

whenever we prefer *x* to *y*, *x appears* to us as better than *y*. But we are capable to recognize this as a mere appearance, in the same way as we are recognize that it is a mere appearance that a stick immersed in water looks to be bent. And if preference is not understood in this experiential way, then even this connection between preference and appearance of betterness cannot be established.

According to Chang, parity is a form of comparability in value. To elucidate the notion of comparability, we might ask what *in*comparability would amount to. Since parity is supposed to be the standard form of incommensurability, incomparability would have to be an incommensurability of a more radical kind. In Rabinowicz (2008), I made the following suggestion. Preference and indifference are different kinds of preferential attitudes. But with regard to some pairs of items we might *lack* a fixed preferential stance. If we nevertheless need to choose between them, we typically experience the choice situation as involving an unresolved conflict. We can see reasons on either side, but we cannot (or do not) balance them off. Or, we waver between possible resolutions without taking a definite stand. We make a choice without the conflict of reasons being resolved. This possibility of a 'preferential gap' – of incompleteness in preferences – allows us to accommodate incomparabilities within the FA-framework: *x* and *y* are *incomparable* if and only if a preferential gap with respect to *x* and *y* is required, i.e., if and only if it is impermissible to prefer one of these items to the other or to be indifferent.

Shouldn't it be enough for incomparability that the preferential gap be *permissible* rather than required? I do not think so. Consider an analogy: We don't say that something is undesirable if it is merely permissible not to desire it. It is undesirable only if desiring it is *im*permissible or inappropriate in some sense. Similarly, incomparability obtains if taking a definite preferential stance is impermissible. However, if we wish, we can introduce the weaker, permissive relation by a stipulative definition: *x* and *y* are *weakly incomparable* if and only if it is permissible neither to prefer one to the other nor to be indifferent.

While incomparability is a 'requiring' concept, *comparability* in value might be understood either in permissive terms, as the contradictory of incomparability, i.e. as the relation that obtains between *x* and *y* whenever it is permissible to prefer one of these items to the other or permissible to be indifferent, or in requiring terms, as the contradictory of weak incomparability, i.e. as the relation that obtains between *x* and *y* whenever a preferential gap with respect to *x* and *y* is disallowed. The permissive reading of comparability seems more natural. On that reading, parity implies comparability.

If two items belong to different ontological categories, say, one is a state of affairs and the other is a person, then such items are incomparable in value: It doesn't make sense to prefer one to the other or to equi-prefer them. Similarly, if the items do not belong to the field to which a given value is applicable, they are incomparable with respect to that value. To illustrate, Mozart and Michelangelo are incomparable in their excellence as scientists: they fall outside the field of that value. But setting such trivial cases aside, it is difficult to provide realistic examples of incomparability. The only kind of examples I have been able to come up with are cases such as Sophie's Choice. Sophie needs to choose which of her two children she is going to save (she cannot save both), but arguably she ought to make this choice without preferring to save one child rather than the other and without being indifferent. Her choice must not be anchored in a preference or in indifference. Such cases are of course quite rare, which would confirm Chang's suggestion that in most cases of incommensurability the items under consideration are comparable in value.

Now, what would be an appropriate formal modelling for this approach to different kinds of value relations? Gert (2004) suggests an interval model, in which every item is assigned an interval of preference strengths that are permissible with regard to the item I question. To satisfy the definition of betterness as required preference, x is better than y in this model if and only if the weakest permissible preference for x, x_{min}, is stronger than the strongest permissible preference for y, y^{max}. Correspondingly, x and y are on a par in the model if and only if their respective intervals overlap, i.e., if they share a sub-interval of a non-zero length. Incomparability cannot be defined in the interval model, and – what is more worrying – the model implies that betterness is a so-called interval ordering, i.e., that for any items x, y, x^+ and y^+ such that x^+ is better than x and y^+ is better than y, it must be that x^+ is better than y or y^+ is better than x.[8] This is counter-intuitive, as shown by the example with Mozart, Michelangelo and Mozart$^+$. If we to this example add Michelangelo$^+$, a small improvement of Michelangelo, it might well be the case that the latter is not better than Mozart, just as Mozart$^+$ is not better than Michelangelo. The reason why the model leads to counter-intuitive results is that it lacks resources to determine constraints on *combinations* of preference strength. Mozart$^+$ is a slightly better artist than Mozart. But this doesn't mean that the weakest permissible preference for the former is stronger than the stronger permissible preference for the latter. Instead, it is a certain connection between our proattitudes towards

[8] Proof: In the interval model, if x^+ is better than x and y^+ is better than y, (i) $x^+_{min} > x^{max}$ and (ii) $y^+_{min} > y^{max}$. There are two possible cases: (1) $x^{max} \geq y^{max}$, or (2) $y^{max} \geq x^{max}$. In case 1, (i) implies that (iii) $x^+_{min} > y^{max}$, i.e., that x^+ is better than y. In case 2, (ii) implies that (iv) $y^+_{min} > x^{max}$, i.e., that y^+ is better than y.

these two items that is required: however strong our preference for Mozart might be, our preference for Mozart^{+} should be stronger. But there are no means to express such requirements on connections in the interval model.

To determine constraints on combinations of preferential attitudes with respect to different items we need a framework that doesn't deal with each item separately, as in assignments of preference intervals to items, but instead handles them jointly. This is the main idea of the *intersection model* in Rabinowicz (2008). Its two components are a non-empty domain **I** of items under consideration and a non-empty class **K** of orderings of the items in **I**. Intuitively, **K** consists of all permissible preference orderings of **I**. Orderings in **K** need not be representable by measures of preference strength. Indeed, they might be incomplete: preference gaps might be permissible. However, it is assumed that in each **K**-ordering, weak preference (i.e. preference-or-indifference) is a quasi-order: a transitive and reflexive relation. This entails, in particular, that both permissible preference and permissible indifference are transitive.

In terms of **K**, the relation of betterness between items can be defined as the intersection of permissible preferences. This is another way of saying that *x* is better than *y* if and only if preferring *x* to *y* is required.

(**B**) *x* is *better* than *y* if and only if *x* is preferred to *y* in every ordering in **K**.

Other value relations are defined correspondingly:

(**E**) *x* and *y* are e*qually good* if and only if they are equi-preferred in every **K**-ordering;

(**I**) *x* and *y* are *incomparable* if and only if every **K**-ordering contains a gap with regard to *x* and *y*;

(**P**) *x* and *y* are *on a par* if and only if *x* is preferred to *y* in some **K**-orderings and dispreferred to *y* in some **K**-orderings.

And so on.

The intersection model exhibits some similarities with Sen's 'intersection approach" (see Sen 1973, ch. 3; cf. also Atkinson 1970). But Sen wasn't after constructing a value relation such as betterness in terms of permissible preference orderings. Instead, his intersection approach served to construct the relation of *definite* betterness from a class of value orderings, with each such ordering representing a different aspect of value comparison. His orderings thus importantly differ from preference orderings in our model. If we analyze betterness all-things-considered, then each ordering in **K** represents a permissible preference all-things-considered, and not just a preference reflecting one particular aspect of comparison. This makes our approach closer to the model developed by Levi (1990), whose "permissible rankings" represent comparisons of options not just in one respect but overall – in all respects. Thus, they are like permissible preferences in our model. However, since for Levi his rankings express different permissible *valuations* of options (valuations all-things-considered), he does not aggregate them to a (yet another) value ordering, as we do when we move from permissible preferences to value relations. Instead, he uses them to determine admissible and optimal *choices* among the options. Thus, despite formal similarities, Sen's and Levi's constructions importantly differ from our intersection model.[9]

The intersection model has no difficulties with determining constraints on combinations of preferential attitudes. In the Mozart-Michelangelo example, each **K**-ordering would place Mozart$^+$ above Mozart and Michelangelo$^+$ above Michelangelo, but some **K**-orderings would place Mozart above Michelangelo$^+$, while other **K**-orderings would place Michelangelo above Mozart$^+$. This would give us all the intended value relationships: Mozart$^+$ is better than Mozart, Michelangelo$^+$ is better than Michelangelo, while both Mozart and Mozart$^+$ are on a part with Michelangelo and Michelangelo$^+$.

The intersection model allows us to derive formal properties of value relations from the constraints on permissible preference orderings. Given that every permissible preference ordering is a quasi-order, one can prove that betterness is a transitive and asymmetric relation, that equal goodness is an equivalence relation, and that whatever is better than, worse than, on a par with or incomparable with one of two equally good items must have exactly the same value relation to the other item. Thus, the modeling does some work.[10]

We now also have all we need for a general taxonomy of binary value relations. In the table below, each column corresponds to one type of a value relation, with plus signs marking the preferential stances that in this type are permissible with regard to a pair of items. There are four such stances, which correspond to the rows in the table: preference ($\succ$), indifference ($\approx$), dispreference ($\prec$), and a gap (/). Since with regard to any two items at least one preferential stance must be permissible (which follows if **K** is non-empty), the number of columns equals the number of ways one can pick a non-empty subset out of the set of four stances. For

[9] I am indebted to an anonymous referee for asking me to clarify this issue. A useful and lucid comparison between Sen and Levi can be found in Sen (2004).

[10] Indeed, the model also makes clear what needs to be given up if one for example, like Temkin (2012), wants to reject the view that betterness must be transitive. This position is now translatable into the view that requirements on permissible preferences need to be correspondingly relaxed.

example, in type 9, all preferential stances are permissible, while in types 1 and 15, which correspond to betterness (**B**) and incomparability (**I**), the only permissible stances are preference and gap, respectively.

The table's columns stand for *atomic* types of value relations. Collections of atomic types, such as parity (**P**, types 6 – 9, i.e. all types in which there are plus signs in the first and the third row), comparability (types 1 - 14), or weak incomparability (types 8 - 15), form types in a broader sense of the word. While Chang was right to suggest that parity is a form of comparability, it is not an atomic type. In this respect, it differs from the three traditional value relations: better (**B**), worse (**W**), and equally good (**E**).

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
$\succ$	+	+				+	+	+	+	+	+				
$\approx$		+	+	+			+		+		+	+	+		
$\prec$				+	+	+	+	+	+				+	+	
/								+	+	+	+	+	+	+	+
	B		**E**		**W**	**P**	**P**	**P**	**P**						**I**

The fifteen atomic types listed above are all *logically* possible. But some of them might not represent 'real' possibilities – possibilities that are instantiated in the domain under consideration. Thus, for example, one might reject incomparability as a real possibility. Likewise, one might reject parity. Indeed, one might reject incommensurability altogether, which would leave only three types (1, 3, and 5). And even if one allows incommensurability and in particular allows parity, it is arguable that both indifference and preferential gaps with regard to items that are on a par should always be permissible. If it is permissible to prefer *x* to *y* and permissible to prefer *y* to *x*, then – one might argue – it should also be permissible to be indifferent between *x* and *y* and permissible to lack a fixed preferential attitude with respect to such items. This would exclude types 6, *7* and 8. In result, only type 9 would be left for parity, which would mean that parity after all does boil down to one atomic type. Note that such extra requirements might be constraints on either each ordering in **K** (example: the prohibition of preferential gaps) or on class **K** taken as a whole: they can stipulate state that **K** must, or must not, contain orderings of certain kinds *if* it contains orderings of certain other kinds (example: the prohibition of parity).[11]

[11] Instead of restricting the space of possibilities, one might instead suggest that this space should be *expanded*. Perhaps we should increase the number of possible preferential stances: For example, isn't there such a relation as *preferential parity*, along with preference, dispreference, indifference, and a gap? Indeed, Chang herself

2. *Probabilities*

If we let, say, desire to stand for a paradigmatic proattitude, then we can express the main idea of the FA- analysis as a slogan: *To be valuable is to be desirable*. The suggestion I want to explore is whether we might approach the notion of probability in an essentially similar way. The proposal is to adopt the view that *to be probable is to be credible*. A proposition is probable if and only if it ought to be given credence. As applied to propositions to which we assign quantitative degrees of probability, this idea can be made a bit more precise:

A proposition *A* is *probable to degree k* if and only if *A* ought to be given credence of degree *k*.[12]

In what follows, I will assume that the 'ought' under consideration is implicitly relativized to a given body of evidence.[13]

Like the FA-account of value, this analysis of probability contains an *attitudinal* component (credence) and a *normative* component (ought). It thus makes probability an explicitly normative notion. In this respect it differs from subjective probability accounts which simply

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seems to have thought so. In (2002b, p. 666), she wrote: "Perhaps most striking, the possibility of parity shows the basic assumption of standard decision and rational choice theory to be mistaken: preferring X to Y, preferring Y to X, and being indifferent between them do not span the conceptual space of choice attitudes one can have towards alternatives." And, if one allows for preferential parity, why not split this stance into different subtypes, just as we have divided value parity into several subtypes, and why not in addition allow for several other possible preferential stances, which are analogues of the value relations we have identified? If we do this, however, then the number of possible *value* relations will dramatically increase. If the number of preferential stances is increased to 15 (which is the number of types of value relations in our taxonomy), the number of atomic types of value relations will go up to 2^{15}-1. And for each new value relation we might then ask whether this relation does not have an analogue on the preferential level. If it does, however, then this leads to yet more value relations that need to be introduced; we are thus heading towards a veritable *explosion* of logical possibilities. (I am indebted to anonymous referees for raising these issues.)

The answer to this worry is to be sought in the analysis of preferential attitudes. We should be prepared to allow for preferential stances that we can distinguish, conceptually and *phenomenologically*, as different possible attitudes. We understand what value parity is, but is there such a thing as preferential parity? What would such an attitude consist in? And if preferential parity has subtypes, what do those subtypes consist in? The answers to these questions can be provided, I think, and some fine distinctions can be made, if one lets permissible preferential states of an agent, i.e. the different members of **K**, be non-empty sets of orderings rather than single orderings. Thus, for example, preferential parity between *x* and *y* would obtain in a preferential state containing both an ordering that ranked *x* above *y* and an ordering that ranked *y* above *x*. But this process of increasing the complexity of the preferential base for taxonomy of value relations cannot be continued *ad infinitum*: phenomenology of possible preferential attitudes sets limits to purely technical constructions.

[12] A biconditional like this might be accepted even by someone who isn't prepared to view it as an *analysis* of probability, but rather takes it as an adequacy criterion for a satisfactory analysis of this notion. The modelling I develop in what follows is compatible with such a cautious position.

[13] This account of probability lends itself, however, also to more 'objective' interpretations, on which the relativization to a body of evidence is replaced by relativization to time: Ought is then relativized either to the total evidence available at a time, or – in an even more objective vein – to everything that an ideal subject could know at the time in question (which would include full knowledge of the preceding history of the world and, possibly, also of the laws in accordance with which the world develops). On the latter version, probability would arguably coincide with objective chance. I am indebted to Christian List for pressing me on this point.

identify probability with credence. To the extent the subjective accounts are normative, their normativity is implicit: It is embodied in various formal constraints that credences are supposed to satisfy (Kolmogorov axioms, the Reflection Principle, the Principal Principle, conditionalization, etc.). By contrast, the account I consider wears normativity on its sleeve. And this normativity is not restricted to purely formal constraints. The evidence available to a subject might not only be reflected in her credences; it might also constrain her credences for other propositions in various substantive ways.

In what follows, I do not intend to directly argue for this FA-account of probability. Rather, I want to explore how it could be made more precise, particularly in its application to probability relations. My overall strategy will be to examine to what extent we can transfer to probability comparisons the lessons gained from the analysis of value relations. My hope is that the picture we'll arrive at in this way will be plausible and attractive. However, before I embark on this project, I first want to highlight, very briefly, some of the advantages of the FA-account of probability and to anchor this account in the historical tradition.

The advantages of this account over a purely subjective one should be clear. It does square much better with our ordinary understanding of probability. On that understanding, probability judgments should guide our credences rather than express or report the latter: We ought to give high credence to what is highly probable. This normative character of probability judgments of course immediately follows given the FA-account. A closely related consideration is that, on the ordinary understanding, an agent's credences might in some cases come into conflict with her probability judgments, just as her preferences might come into conflict with her judgments of value. And the agent might be aware of the conflict. This awareness of a conflict between one's credences and one's probability judgments – "credo quia improbabile" – is difficult to account for on a subjective interpretation of probability. Unless, of course, one insists that an agent who experiences this conflict must interpret probability judgments incorrectly.

The normativity of the FA-account also makes it attractive when we compare it with 'objective' theories of probability, such as frequentism or propensity theory. On objectivist theories, the role of probabilities as a guide for credences is not obvious; it is something that needs to be argued for. The Principal Principle, according to which credences ought to reflect expected objective probabilities, is a substantive claim. But no argument is needed to establish that credences ought to track probabilities, if the latter are understood in accordance with the FA-account. It should be noted, however, that accepting the FA-account of probability does not in any way require rejecting theories of objective chance. The latter might of course be needed anyway in our descriptions of the world.[14]

The FA-acount of probability is sometimes referred to as "the epistemic interpretation of probability".[15] It has a history which – just as the FA-account of value – goes back to the nineteenth century, but starts about half a century earlier. Thus, already Poisson (1837), contrasted probability with objective chance, in the following way:

> The *probability* of an event is our reason to believe that it will occur or occurred […] Probability depends on our knowledge about an event; for the same event it can differ for different persons. Thus, if a person only knows that an urn contains white and black balls, whereas another person also knows that there are more white balls than black ones, the latter has more grounds to believe in the extraction of a white ball. In other words, for him, that event has a higher probability than for the former. […] we attach here the word *chance* to events taken independently from our knowledge, and retain its previous definition for the word *probability*.[16] Thus, by its nature an event has a greater or lesser chance, known or unknown, whereas its probability is relative to our knowledge about it. (Poisson 2013 [1837], p. 31)[17]

Cournot (1843, pp. V, 438) makes a similar distinction between objective probabilities (chances) and the subjective ones, where the latter vary with the subject's knowledge. De Morgan (1847) starts out by identifying probability with the degree of belief:

> By degree of probability we really mean, or ought to mean, degree of belief. (De Morgan 1837. p. 172)

But then he explains that he has in mind the degree of belief that we *ought* to have:

> 'It is more probable than improbable' means in this chapter 'I believe that it will happen more than I believe that it will not happen.' Or rather 'I *ought* to believe, &c.:' for it may happen that the state of mind which *is*, is not the state of mind which should be. D'Alembert believed that it was two to one that the first head which the throw of a halfpenny was to give would occur before the third throw [...] But I shall say, for all that, that the probability is three to one: meaning, that in the universal opinion of those who examine the subject, the state of mind to which a person ought to be able to bring himself is to look three times as confidently upon the arrival as upon the non-arrival. (ibid., p.173)

And he emphasizes the role of knowledge (evidence) as the normative basis for belief:

> what we know is to regulate what we believe; nor can we make any effective use of what we know, except in obtaining and describing what we believe, or ought to believe. (p.174)

Donkin (1851) writes in the same vein:

> the 'probability' which is estimated numerically means merely 'quantity of belief,' and is nothing inherent in the hypothesis to which it refers. It is therefore always relative to a particular state of

[14] It is possible, in fact, to analyze objective chances in terms of FA-probabilities, on the lines of David Lewis's well-known reduction of objective chances to rational credences given appropriately circumscribed states of knowledge or evidence. Cf. the preceding footnote.

[15] But the terminology varies. In Galavotti (2011), "the epistemic interpretation" is subdivided into two categories: the subjective and the logical interpretation. The adherents of the FA-approach are included among the representatives of the logical interpretation and the latter culminates with Carnap. For another useful recent historical account, see Zabell (2011).

[16] This suggests that there was an earlier definition of "chance" which was like the definition that Poisson now wants to give for "probability". But whose definition was it? Poisson's or somebody else's? We aren't told.

[17] It may be somewhat misleading to count Poisson as a precursor of the account of probability I am interested in. It seems that on his proposal an event is more probable on one kind of evidence rather than another if the former evidence gives us "more grounds to believe" it. Which is not quite the same as to say that it gives us grounds to believe it more, as the analysis I am interested in would have it. To put it differently: Stronger reasons to believe are not necessarily co-extensional with reasons to believe more strongly. For an analysis of "more probable than" in terms of stronger reasons to believe, cf. Skorupski (2010), ch. 9.

> knowledge or ignorance; but it must be observed that it is absolute in the sense of not being relative to any individual mind; since, the same information being presupposed, all minds ought to distribute their belief in the same way. (Donkin 1851, p. 355)
>
> by the 'probability' of a hypothesis is meant the quantity of belief which ought to be given to it by a person in a determinate state of information respecting it. The word hypothesis is used to denote anything, or rather any proposition, which can be believed or disbelieved … (ibid., p. 356)

Boole (1854, p. 187) refers to Poisson's definition of probability and continues:

> With the degree of information which we possess concerning the circumstances of an event, the reason we have to think that it will occur, or, to use a single term, our expectation of it, will vary. Probability is expectation founded upon partial knowledge. (ibid., p.187f)[18]

Jevons (1873) starts out by misrepresenting de Morgan and Donkin as adherents of the purely subjective interpretation and then declares that the subjective view is wrong because probability is normative (just as De Morgan and Donkin were claiming all along!):

> De Morgan says, 'By degree of probability we really mean or ought to mean degree of belief.' The late Professor Donkin expressed the meaning of probability as 'quantity of belief'; but I have never felt satisfied with such definitions of probability. The nature of belief is not more clear to my mind than the notion which it is used to define. But an all-sufficient objection is, that the theory [of probability] does not measure what the belief is, but what it ought to be. (Jevons 1873. p. 198)[19]

This development culminates with J. M. Keynes's *A Treatise on Probability* (1921). Here are some representative quotes from the first pages of this book:

> The terms *certain* and *probable* describe the various degrees of rational belief about a proposition which different amounts of knowledge authorise us to entertain. (Keynes 1921, p. 2)
>
> The Theory of Probability [...] is concerned with the degree of belief which it is *rational* to entertain in given conditions, and not merely with the actual beliefs of particular individuals, which may or may not be rational. (ibid., p. 3)
>
> A *definition* of probability is not possible, unless it contents us to define degrees of the probability relation by reference to degrees of rational belief. We cannot analyse the probability-relation in terms of simpler ideas. (ibid., p. 7)

"Rational" stands for the normative component in Keynes's account of probability. I will have reason to return to Keynes in connection with my discussion of incommensurable probabilities, but before we leave him for now there is one thing I need to mention. Keynes

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[18] He then, however, adds:

> it would be unphilosophical to affirm that the strength of that expectation, viewed as an emotion of the mind, is capable of being referred to any numerical standard. [...] The rules which we employ in life-assurance, and in the other statistical applications of the theory of probabilities, are altogether independent of the mental phaenomena of expectation. They are founded upon the assumption that the future will bear a resemblance to the past; that under the same circumstances the same event will tend to recur with a definite numerical frequency; not upon any attempt to submit to calculation the strength of human hopes and fears. (p.188)

It is unclear whether Boole here merely rejects the measurement of probability by measuring the strength of the *actual* psychological state of expectation, or rejects the previous suggestion that probability is rational expectation based on partial knowledge, "the reason we have to think that [an event] will occur".

[19] It is possible, though, that Jevons should not be counted among the adherents of the FA-approach. While he thinks the probability is normative with respect to belief, he is unwilling to *define* it in terms of what we ought to believe:

> "I prefer to dispense altogether with this obscure word belief, and to say that the theory of probability deals with *quantity of knowledge* ... An event is only probable when our knowledge of it is diluted with ignorance, and exact calculation is needed to discriminate how much we do and do not know." (ibid., p. 199)

combines his FA-account of probability with strict objectivism concerning what is rational to believe given evidence:

> When once the facts are given which determine our knowledge, what is probable or improbable in these circumstances has been fixed objectively, and is independent of our opinion (ibid., p. 3)

But the FA-approach does not by itself presuppose such objectivism. As I have mentioned above, in connection with the analysis of value, the FA-account is compatible with different interpretations of the normative component in the analysis, including subjectivist and even non-cognitivist interpretations. Whether such interpretations are plausible or not is a separate issue.

The history of the FA-account of probability does not end with Keynes, of course. Among its later proponents we find such philosophers as as Jeffreys (1931, 1939) and Carnap (1962, 1971). In his (1971), the latter distinguishes between ”statistical probability” and ”personal probability”, with the latter being ”the degree of belief of [a person] *X* in [an event] *H*”. He stresses that we need to distinguish between ”two versions of personal probability, one representing the *actual* degree of belief and the other the *rational* degree of belief” (ibid, p. 8). Rational degrees of belief satisfy standard axioms of probability and they also are appropriately related to the person’s evidence (at a given time). His conception of logical (or, as he sometimes calls it, ”inductive” probability) is a further (and much more radical) elaboration of this idea of a degree of belief that is rational given evidence.

The FA- account of probability lends itself to the treatment of probability relations that is exactly analogous to the treatment of value relations. Taking propositions to be the objects of credence and letting *A* and *B* vary over propositions, we get the following series of definitions:

A is *more probable* than *B* if and only if *A* ought to be given more credence than *B.*

A and *B* are *equiprobable* if and only if *A* and *B* ought to be given equal credence.

A and *B* are *incommensurable* (probabilitywise) if and only if neither is more probable than the other nor are they equiprobable.

A and *B* are *on a par* (probabilitywise) if and only if

(i) it is permissible to give *A* more credence than *B*

and

(ii) it is permissible to give *B* more credence than *A*.

A and *B* are *incomparable* (probabilitywise) if and only if it is impermissible to give more credence to either of these propositions or to give equal credence to both.

What is required in case of incomparability is a *credence gap* – a state in which neither proposition is given more credence than the other, nor are they given equal credence. How to interpret such credence gaps will be considered later. As in the case of value relations, probabilistic incomparability is a radical form of incommensurability, while probabilistic parity is incommensurability of a more liberal kind, which does not require credence gaps (just as parity in value does not require preference gaps).

Comparability is the contradictory of incomparability: Two propositions are *comparable* probabilitywise if and only if they are not incomparable (i.e., the credence gap is not required). Along with incomparability – the required credence gap – we may distinguish *weak incomparability*, which is present when the credence gap is permissible.

Can these different kinds of probabilistic incommensurability be instantiated in real life? As for probabilistic parity, we might expect to encounter it in some of the cases in which different parts of our evidence come from different sources or are of different kinds. In order to assign credences to propositions under consideration, we will need to weigh these parts of evidence against each other. Different assignments of weights might be equally admissible and it might turn out that one such way of weighing favors *A* over *B* while another favors *B* over *A*. In a case like this, it will be permissible to give higher credence to *A* than to *B*, but it will also be permissible to give higher credence to *B* instead. Thus, *A* and *B* will be on a par probabilitywise.

…The possibility of probabilistic parity depends on there being a difference between credence and a probability judgment (just as the possibility of parity in value depends on there being a difference between preference and a value judgment). Giving credence to *A* is not the same as making a judgment about *A* and its probability. Indeed, probability judgments require conceptual resources that aren't needed in order to have credences. Likewise, giving more credence to *A* than to *B* is not the same as judging *A* to be more probable than *B*. This makes it possible to give more credence to *A* than to *B* even though one judges *A* and *B* to be probabilistically on a par, i.e., even if one considers the opposing credential stance with respect to these two propositions to be as permissible as one's own. We should be capable to recognize our own assignments of weights to different respects of comparison as being at least partly optional, i.e. to accept that other ways of weighing might also be admissible.

But, and this should also be stressed, in some cases involving heterogeneous evidence, it might turn out that there is *no* admissible way of weighing the different parts of evidence against each other, or – even if there are such ways – that it is at least permissible to abstain from weighing. Thus, Keynes describes the following case:

> Is our expectation of rain, when we start out for a walk, always more likely than not, or less likely than not, or as likely as not? I am prepared to argue that on some occasions none of these alternatives hold, and that it will be an arbitrary matter to decide for or against the umbrella. If the barometer is high, but the clouds are black, it is not always rational that one should prevail over the other in our minds, *or even that we should balance them*,—though it will be rational to allow caprice to determine us and to waste no time on the debate. (Keynes 1921, pp. 31f, my italics)

Keynes is saying that in a case like this, with different parts of evidence pointing in the opposite directions ("the barometer is high, but the clouds are black"), it might be rational of us to arbitrarily choose an action (whether to take an umbrella or not), without balancing against each other different considerations that support, respectively, the relevant hypothesis (Rain) and its negation (No Rain). I read it as saying that it might be rational of us to remain in the state of a credence gap and to make our choice at random. It is unclear, though, whether Keynes thinks that we *should not* balance the opposing considerations in this case, which would mean that it is a case of incomparability, or merely *denies that we should*, which would make it a case of weak incomparability.[20]

Moving now to formal representation, how can the different probability relations be modelled? One possibility is to use an *interval* model and assign to each proposition A a range of credences that are permissible with respect to the proposition in question. To keep thhings simple, such a range might be a closed interval $[A_{min}, A^{max}]$. (Instead of intervals, one might also work with ranges interpreted as arbitrary sets of reals between 0 and 1.) This would mean that it is permissible to give higher credence to A than to B if and only if there is a degree of credence within $[A_{min}, A^{max}]$ that is higher than some degree of credence within $[B_{min}, B^{max}]$. As a result, two propositions would be on a par, probabilitywise, if there is an overlap comprising more than one degree of credence between their respectve ranges. We would then define 'more probable than' by comparing minima with maxima: A is more probable than B if and only if $A_{min} > B^{max}$. That is, if and only if the lowest permissible credence with respect to A is higher than the highest permissible credence with respect to B.

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[20] In describing another example, Keynes is more categorical in his rejection of balancing. He suggests that in that example there is no admissible way of weighing different considerations against each other:

> Consider three sets of experiments, each directed towards establishing a generalisation. The first set is more numerous; in the second set the irrelevant conditions have been more carefully varied; in the third case the generalisation in view is wider in scope than in the others. Which of these generalisations is on such evidence the most probable? There is, surely, no answer; there is neither equality nor inequality between them. We cannot always weigh the analogy against the induction, or the scope of the generalisation against the bulk of the evidence in support of it. (ibid., p. 31)

(With ranges interpreted as arbitrary sets of numerical values between 0 and 1, this definition would need to be re-formulated: It would require that every degree of credence in the range for *A* is higher than every degree of credence in the range for *B*.)

As is easily seen, this model suffers from the same maladies as Gert's interval model for value relations: It cannot represent incomparability: if $A_{min} > B^{max}$, then giving more credence to *A* than to *B* is required, and if $B^{max} \geq A_{min}$, then giving at least equal credence to *B* as to *A* is permissible. Thus, the credence gap can never be required. Indeed, it is not clear how credence gaps can even be permissible in this model. But, more importantly, the interval model lacks resources to specify *constraints on combinations* of credences. In some cases when we would want to say that proposition A^+ is more probable than proposition *A*, their corresponding ranges overlap, but there is an intuitive constraint we would like to impose on the combinations of credences with respect to A^+ and *A*: Whatever credence one assigns to *A*, one ought to assign a higher credence to A^+. The absence of such constraints in the interval model is its major fault.

This fault explains why certain structures of "more probable than"-relationships cannot be represented in this model. The interval model implies that "more probable than" is an interval ordering. It validates the Interval Axiom, according to which, for all propositions *A*, *B*, A^+ and B^+, if A^+ is more probable than *A* and B^+ is more probable than B, then A^+ is more probable than *B* or B^+ is more probable than *A*. This axiom is counter-intuitive, as shown by the following illustration: Suppose we consider two probabilistically incommensurable propositions *A* and *B*, for example "Rain" and "No Rain" in Keynes's little story. Now, let *C* be some proposition that has nothing to do with either *A* or *B*. And suppose that *C* has a very low but positive probability. Given these assumptions, the disjunction $A \vee C$ is slightly more probable than *A* and, similarly, the disjunction $B \vee C$ is slightly more probable than *B*. However, these probability increments resulting from disjoining *A* and *B* with *C* are small given that C is so improbable. Therefore, $A \vee C$ still is not more probable than *B*, nor is $B \vee C$ more probable than *A*. We thus have a counter-example to the Interval Axiom, if we let $A^+ = A \vee C$ and $B^+ = B \vee C$. What drives the counter-example is the fact that while $A \vee C$ is more probable than *A*, the lowest permissible credence for $A \vee C$ is not greater than the highest permissible credence for *A*. Instead, whatever credence we give to *A* we ought to give a slightly higher credence to $A \vee C$. And similarly for the comparison between *B* and $B \vee C$. (Cf. Rabinowicz 2008.)

We need to use an *intersection* model instead. In this case, the domain **I** of items consists of propositions – the objects of credence. **I** is non-empty and closed under Boolean operations. For simplicity, I take it that **I** is finite, but extension to the infinite case does not pose any special problems for the model. The class **K** is now the non-empty set of all permissible *doxastic states*. Intuitively, a doxastic state is a possible state of mind of a person, limited to her credences. Formally, we represent a doxastic state *S* as a non-empty set of credence functions, each of which assigns numerical values between 0 and 1 to every proposition in the domain **I** and satisfies Kolmogorov axioms. If *S* is a singleton, i.e., if it consists of just one credence function, then *S* is *fully opinionated*: a person in that state has a definite credence assignment to every proposition. If, on the other hand, we want to represent a state that at least to some extent lacks such doxastic completeness, then we do it by letting *S* consist of more than one credence function. A person's credential state of mind might be interpreted as what's common to the functions that together form a given state *S*. This way of representing a doxastic state – as a set of credence functions – is quite standard in the work modelling uncertainty. In philosophical literature it goes back to Levi (1980).[21]

We can now define what it means to say that a proposition is given more credence than another in a doxastic state or that the two propositions are given equal credence. And we can also define the notion of a credence gap.

A is given *more credence* than *B* in *S* if and only if, for every credence function *C* in *S*, $C(A) > C(B)$.

A and *B* are given *equal credence* in *S* if and only if, for every *C* in *S*, $C(A) = C(B)$.

There is a *credence* gap with respect to *A* and *B* in *S* if and only if none of these propositions is given more credence in *S* than the other nor are they given equal credence in *S*.

Thus, there is a credence gap in *S* with regard to two propositions if and only if the credence functions in *S* do not order these two propositions in a uniform way. For example, S contains a credence gap with respect to *A* and *B* if it contains credence functions *C* and *C'* such that $C(A) > C(B)$ but $C'(A) < C'(B)$. Clearly, this means that credence gaps can only arise in doxastic states that aren't fully opinionated. And, conversely, every doxastic state that isn't fully opinionated must contain some credence gaps.

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[21] For a nice succinct discussion of the advantages of this representation and for some further information on its historical roots in economics and statistics, see Weatherson (2002). Cf. also Weatherson (2007). An alternative, 'linguistic' representation of doxastic states will be briefly considered below.

Needless to say, we can now also define various quantitative claims about doxastic states. Thus, for example, to say that A is given credence lower than k in S means that for every C in S, $C(A) < k$. Analogously, to say that A is given credence k in S means that for every C in S, $C(A) = k$. To say that, in S, A is given twice as high credence as B means that for every C in S, $C(A)/C(B) = 2$. And so on.

There is a certain formal dissimilarity between the model for values and this model for probabilities. In the former, I represented permissible 'preferential states' as partial orderings, instead of defining them as sets of complete orderings, while in the latter I represent doxastic states as sets of complete credence functions. What's the reason for this disanalogy? Why not represent doxastic states in a purely qualitative way, as partial credence orderings of propositions?

The answer is that in the probability case we are interested in representing more than just qualitative probability relations. The model I describe also allows us to represent various quantitative claims about probabilities. Thus, for example, the claim that the probability of A is lower than k, which on the FA-account is interpreted as the requirement to give A credence lower than k, reduces in the model to the constraint on permissible doxastic states: For all S in $\mathbf{K}$, A is given credence lower than k in S (i.e., for all S in $\mathbf{K}$ and all C in S, $C(S) < k$).

But wouldn't it then be enough to represent a doxastic state by a partial assignment of numerical degrees of credence to propositions conjoined with a partial credence ordering of propositions (compatible with the assignment in question)? No, it would not. It would still be too restrictive. We might, for example, want to say that the probability of A is twice as large as the probability of B, even when these two propositions aren't assigned definite degrees of credence in any permissible doxastic state. We can easily make such claims in our model, but not in the impoverished model in which doxastic states are represented as partial credence assignments conjoined with partial credence orderings.

There is, though, an alternative representation of doxastic states that should be mentioned. Suppose we had at our disposal a regimented credence language in which we could specify different features of a doxastic state. A language of this kind would contain sentences such as "A is given more credence than B", "A and B are given equal credence", "There is a credence gap with respect to A and B", "A is given credence k", "A is given credence lower than k", etc., and it would allow quantification over both propositions and over numerical values. We could define a set of sentences Γ in such a language as *coherent* if there is a non-empty set S

of credence functions that makes all the sentences in Γ true, in accordance with the definitions provided above. If some such S that verifies Γ in addition makes no other sentence in the regimented language true than those that belong to Γ, Γ is both coherent and *complete*. Then we could represent doxastic states by coherent and complete sets of sentences in this regimented language. Intuitively, a sentence set of this kind states everything that can be said *in the regimented language* about a person's credential state of mind. Clearly, this linguistic representation would be more coarse-grained than the one I use: a coherent and complete set of sentences can correspond to several different sets of credence functions.[22] But still such a representation would be sufficient for most of our modelling purposes. In what follows, however, I shall abstain from constructing a language that regiments statements about credences: I shall continue to represent doxastic states as non-empty sets of credence functions.[23]

Setting aside, for the time being, quantitative statements about probabilities, we now have what we need to define qualitative probability relations in terms of the intersection model. The definitions follow, of course, the format of the corresponding definitions of value relations. Thus, for example,

A is *more probable* than *B* if and only if *A* is given more credence than *B* in every *S* in **K**.

A and *B* are *equiprobable* if and only if *A* and *B* are given equal credence in every *S* in **K**.

A and *B* are *on a par* (probabilitywise) if and only if

(i) *A* is given more credence than *B* in some *S* in **K**

and

(ii) *B* is given more credence than *A* in some *S* in **K**.

A and *B* are *incomparable* (probabilitywise) if and only if there is a credence gap with respect to *A* and *B* in every *S* in **K**.

And so on.

We can also provide a taxonomy of all binary probability relations, which is exactly analogous to the corresponding taxonomy for value relations:

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[22] Cf. Hawthorne (2009) for a linguistic representation of doxastic states and for a representation theorem connecting such representation with one in terms of sets of credence functions. However, it should be noted that Hawthorne's regimented language is more restricted than the one I envisage; it is purely qualitative and thus does not introduce numerical values.

[23] I am indebted to Christian List for suggesting this linguistic representation of doxastic states.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
$\succ$	+	+				+	+	+	+	+	+				
$\approx$		+	+	+			+		+		+	+	+		
$\prec$				+	+	+	+	+	+				+	+	
/								+	+	+	+	+	+	+	+
	M		**E**		**L**	**P**	**P**	**P**	**P**						**I**

Here, the four possible credential stances with respect to a pair of propositions are: giving more credence to the first proposition than to the second one ($\succ$), giving it less credence ($\prec$), giving both propositions equal credence ($\approx$), and being in a state of credential gap (/). These four stances correspond to the four rows in the the table. Again there are fifteen columns, corresponding to the different atomic types of probability relations, with each type being defined by the credential stances that are permissible in the type in question (these stances are marked by the plus signs in the column). As for the type labels, we still have **E**, **I** and **P**, which now stand for equal probability, probabilistic incomparability and parity, respectively. But **B** (better) and **W** (worse) are replaced by **M** (more probable than) and **L** (less probable than).

While the fifteen atomic types listed in the table are all logically possible, some might lack real instantiations. If we are after ‘real possibilities’, we might want to reduce the table by two kinds of restrictions: (i) constraints that need to be obeyed by each permissible doxastic state, i.e., by each element in **K**, or (ii) holistic constraints, imposed on class **K** as a whole.

Here is an example of a constraint of the first type:

Full Opinionatedness: All permissible doxastic states are singletons.

This would exclude credence gaps and thus remove all types from 8 to15.

The following are examples of constraints of the second type:

Convexity: If in some *S* in **K**, *A* is assigned higher credence than *B*, and in some other *S* in **K**, *B* is assigned higher credence than *A*, then there is some *S* in **K** in which *A* and *B* are assigned equal credence.

This would remove types 6 and 8.

Another, similar constraint could be this one:

Credence Gaps: If in some *S* in **K**, *A* is assigned higher credence than *B*, and in some other *S* in **K**, *B* is assigned higher credence than *A*, then there is some *S* in **K** in which there is a credence gap with respect to *A* and *B*.

This would remove types 6 and 7. Together, Convexity and Credence Gaps would imply that parity reduces to type 9.

Credence Gaps would immediately follow if we imposed a more general constraint on **K**:

Unions: If $S, S' \in \mathbf{K}$, then for some $S'' \in \mathbf{K}$, $S \cup S' \subseteq \boldsymbol{S''}$.

A constraint that is worth considering would require that it is permissible to be in a doxastic state in which one's credences mirror major probability relations:

Probability Mirror: There is a state *S* in *K* such that for all *A* and *B*,
(i) *A* is given more credence than *B* in *S* if and only if *A* is more probable than *B*,
(ii) *A* and *B* are given equal credence in *S* if and only if *A* and *B* are equally probable.[24]

The ”if”-parts in clauses (i) and (ii) are trivial given our definitions of ”more probable than” and ”equally as probable as”. It is the ”only if”-parts that form the non-trivial content of this constraint. As is easy to see, Credence Gaps immediately follows from Probability Mirror.

The following condition deserves a special attention; it quite radically reduces the number of types of probability relations:

Uniqueness: There is only one permissible doxastic state.

If Uniqueness is assumed, then there are only four possibilities that remain for any two propositions *A* and *B*: Either *A* is more probable than *B*, or less probable, or equally as probable, or the two propositions are incomparable probabilitywise. The latter is the case if and only if there is a credence gap with regard to *A* and *B* in the single permissible doxastic state. Given Uniqueness, incommensurability reduces to incomparability.

Uniqueness will be an appealing condition for an adherent of the FA-account who is unwilling to allow for probabilistic parity. Such a person is unwilling to allow that, for some *A* and *B*, it may be permissible to give more credence to *A* than to *B* and likewise permissible to give more credence to *B* than to *A*. If the evidence neither requires giving more evidence to

[24] The mirroring of probability relations by credences cannot be complete, however: There are only four credential stances with regard to two propositions, but there are fifteen probability relations. If *S* satisfies conditions (i) and (ii) in Probability Mirror, then the credence gap in *S* mirrors probabilistic incommensurability, but it doesn't distinguish between different atomic types of incommensurability. Even though some of those types can be reflected by different kinds of credence gaps, there are fewer kinds of credence gaps than there are atomic types of incommensurability.

A nor requires giving more evidence to *B*, and if it in addition does not require that *A* and *B* are given equal credence, then – on this view – the only avenue open to the agent is to 'suspend judgement', i.e, to maintain a credence gap with respect to *A* and *B*.

It is easy to see that Uniqueness immediately entails Probability Mirror. It also trivially entails both Unions and Convexity. As for Full Opinionatedness, Uniqueness is compatible with this condition, but does not imply it. Indeed, combining Uniqueness with the rejection of Full Opiniotadness seems like an attractive view. I will have more to say about Uniqueness towards the end of this section.

The above list of constraints provides just some examples of additional restrictions that might be imposed on class **K**. But all these constraints go beyond the basic intersection model that is assumed in this paper.

One of the goals of the paper has been to provide a framework that accounts for incommensurable probabilities, along the lines of Keynes's *Treatise* (1921). The objective is to make room for propositions whose probability cannot be specified numerically, by a single number or even by a number interval or a set or vector of numbers. The model I use does allow us to associate with each proposition *A* a set of numerical values, or more precisely a set of such sets: We can associate with *A* the set of sets of numerical values that are accorded to *A* by different states *S* in K. Each such *S* determines the set $\{k$: for some C in S, $C(A) = k\}$. But such sets of sets of numerical values cannot be said to stand for the probabilities of propositions: At least, we cannot use them to define what it means that one proposition is more probable than another. The reason why we can't do it is essentially the same as the reason why the interval model for probability relations doesn't work: To determine constraints on combinations of credences for different propositions we need a framework that doesn't deal with each proposition separately, but instead handles them jointly.

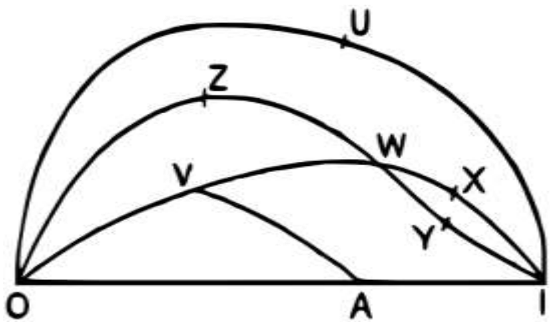
Does this mean then that on our approach some propositions do not have any probability at all? No, it doesn't follow. What does follow is that the probabilities of some propositions might not be quantifiable. Trivially, we can always represent the probability of *A* by a certain class of propositions to which *A* belongs, namely, its equivalence class with respect to the relation "equally as probable as". Note also that non-quantifiable probabilities might sometimes enter into arithmetical relations. For example, as I already mentioned, it is possible in our model to have two propositions *A* and *B* such that *A* is, say, twice as probable as *B* despite the fact that neither *A* nor *B* can be given a numerical probability. This will be the case if in all S in **K** and all C in S, $C(A)/C(B) = 2$. In some such cases it will also be possible to measure the probability difference between two such propositions with non-quantifiable probabilities – if the difference between their credences is constant in all credence functions in all permissible doxastic states.[25]

It should also be pointed out that the intersection model sometimes does allow assigning single numerical probability values to propositions. This will be the case for any A for which there is some k such that for all permissible S and all C in S, $C(S) = k$. But if class **K** is large enough or if some doxastic states in **K** contain many credence functions, this will be an exception rather than a rule.

All this is in line with Keynes's ideas:

> I maintain [...] that there are some pairs of probabilities between the members of which no comparison of magnitude is possible; that we can say, nevertheless, of some pairs of relations of probability that the one is greater and the other less, although it is not possible to measure the difference between them; and that in a very special type of case [...] meaning can be given to a numerical comparison of magnitude. […]
>
> By saying that not all probabilities are measurable, I mean that it is not possible to say of every pair of conclusions [i.e. propositions], about which we have some knowledge, that the degree of our rational belief in one bears any numerical relation to the degree of our rational belief in the other; and by saying that not all probabilities are comparable in respect of more and less, I mean that it is not always possible to say that the degree of our rational belief in one conclusion is either equal to, greater than, or less than the degree of our belief in another. (Keynes 1921, p. 36)

The intersection model bears out Keynes's well-known diagram representing probability space (ibid., p.42):



In this diagram, O stands for the impossible proposition (which is assigned probability 0), while I for a proposition that is certainly true (and thus assigned probability 1); propositions on the same path are linearly ordered from right to left by the relation "more probable than"

[25] Of course, if one wants to reserve the term "probability" for numerical assignments, as many theorists prefer to do, then "non-quantifiable probabilities" aren't possible. Instead, one would need to have a special terminology for the concept we are talking about in this paper. Perhaps, we could refer to it as *likelihood*, as suggested by an anonymous referee. Propositions that lack numerical probabilities can still be more or less likely than other propositions. And while only some propositions have numerical probabilities, every proposition has a (qualitative) degree of likelihood, given by its equivalence class with respect to the relation of being equally likely. In this paper, however, the term "probability" is used in a broad sense and the distinction between probability and likelihood is not made.

(example: W and V); propositions that do not lie on the same path are probabilistically incommensurable (example: V and Z); the same proposition can lie on several paths (example: W) and thus be more probable than several mutually incommensurable propositions (such as V and Z); only the propositions on the path OAI have numerical probabilities; a proposition that lacks a numerical probability can lie on a path connecting propositions with numerical probabilities and thus be more probable or less probable than such propositions (example: V, which lies on a path from O to A).

Thus, it seems that the approach I have presented is close in spirit to the Keynesian conception of probability. It takes account of his FA-style analysis of this concept and it builds on this analysis to provide a model that makes room for non-numerical probabilities and for various incommensurability phenomena.[26] But it goes beyond Keynes in distinguishing between different types of incommensurability relations. Above, we have seen that Keynes was anxious to establish that some propositions might be probabilistically incomparable. But he did not seem to consider the possibility of probabilistic parity.

Indeed, Keynes might not have been prepared to accept the possibility that different doxastic states could be permissible. If confronted with this issue, he might have assented to the condition of Uniqueness, according to which there is only one permissible doxastic state. Then it is another matter that this state could not have been fully opinionated; on the opposite, it would have to contain a large number of credence gaps, to account for Keynes' views about probability. For an interpretation of Keynes along these lines, see Weatherson (2002).[27]

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[26] In his (1921), Keynes to a large extent worked with what we might call conditional probabilities. He used expressions of the form a/h, standing for the probability of a proposition a on the hypothesis that h. As long as the condition h is positively probable (i.e., as long as for all S in $\mathbf{K}$ and all C in S, $C(h) > 0$), conditional probabilities do not present any special problem for our model: We represent them by conditionalizing credence functions in all permissible states on positively probable conditions. But if we also want to allow conditional probabilities with conditions whose probability need not be positive (i.e., which receive value zero in at least some credence functions in some permissible doxastic states), the credence functions that are elements of doxastic states would have to be Popper functions instead. That is, they would need to be conditional functions that are defined even for the conditions that are assigned zero credence.

[27] If one not only assumes that there is a unique permissible doxastic state, but also holds that this state is fully opinionated, then there is no longer any room for probabilistic incommensurabilities. Nor is there any room for propositions whose probablities are unmeasurable. Some of the post-Keynesian adherents of the FA-account of probability have chosen this uncompromising path. Perhaps the best example is Jeffreys (1931):

> On a given set of data p we say that a proposition q has in relation to these data one and only one probability. If any person assigns a different probability, he is simply wrong [....] Personal differences in assigning probabilities in everyday life are not due to any ambiguity in the notion of probability itself, but to mental differences between individuals, to differences in the data available to them, and to differences in the amount of care taken to evaluate the probability. (p. 10)

Whether Uniqueness is a plausible condition or not crucially depends on one's view concerning what credences are permissible when (i) different parts of evidence point in different directions, one part supporting *A* rather than *B* and the other supporting *B* rather than *A*, and (ii) there is no single admissible way in which we should weigh the different parts of evidence against each other. If one thinks that in such a case the credence gap with regard to *A* and *B* is the only permissible stance, then Uniqueness is the right condition to assume. But if one instead allows, as I think one should, that in cases like this it might sometimes be permissible to give more credence to *A* and also permissible to give more credence to *B*, then one needs a model in which different doxastic states can be permissible.

Gideon Rosen gives expression to a view that implies this kind of doxastic permissivism:

> It should be obvious that reasonable people can disagree, even when confronted with a single body of evidence. When a jury or a court is divided in a difficult case, the mere fact of disagreement does not mean that someone is being unreasonable. Paleontologists disagree about what killed the dinosaurs. And while it is possible that most of the parties to this dispute are irrational, this need not be the case. To the contrary, it would appear to be a fact of epistemic life that a careful review of the evidence does not guarantee consensus, even among thoughtful and otherwise rational investigators. (Rosen, p. 71)

If, when confronted with a single body of evidence, reasonable people can disagree, then their opposing judgments, *A* and *B*, are equally permissible given the evidence in question. But, if so, then it follows that it is permissible, given the same body of evidence, to give *A* more credence than *B* and likewise permissible to give *B* more credence than *A*. In other words, the possibility of reasonable disagreement implies the falsity of Uniqueness.[28]

Whether Uniqueness should be accepted or not is hotly debated at present. For what it is worth, my own sympathies lie with the critics of Uniqueness.[29]

While Jeffreys here postulates "one and only one probability" rather than one and only one permissible credence, he does seem to run the two together (which would be the right thing to do if only one degree of credence were permissible with regard to each proposition, given the evidence). This is confirmed by his (1939, p. 39), where he talks about "the unique reasonable degree of belief".

[28] There has recently been much discussion about peer disagreement. The central question in this debate is, as van Wietmarschen (2013) puts it: "what should you believe about the disputed proposition if you have good reason to believe that an epistemic peer disagrees with you?" It is clear that the debate about peer disagreement has connections to debates about Uniqueness. I am indebted to Richard Dietz for pointing this out.

[29] For recent criticisms of Uniqueness, see Kelly (2013), Schoenfield (1014), Meacham (forthcoming) and Mayo-Wilson & Wheeler (forthcoming). For defences of uniqueness, see White (2005, 2013), Hedden (forthcoming) and Horowitz (forthcoming).

3. *Vagueness*

I conclude this paper with brief remarks concerning *vagueness* in probability comparisons. On the FA-account of probability, probability judgments are normative assessments of credence. In the intersection model, probability comparisons between propositions are based on the way in which we draw the boundaries of the class of permissible doxastic states. Now, a natural route by which vagueness can enter the picture is through the *vagueness of permissibility*. For some propositions A and B, it might be indeterminate – neither true nor false – whether it is permissible to give A more credence than B. Or, it might be indeterminate whether it is permissible to have a preferential gap with respect to A and B. In terms of our model, this implies that for some doxastic states it might be indeterminate whether they are permissible given the evidence available. The standards of permissibility we rely on need not be precise enough to determine the permissibility status of every doxastic state.[30] This means that the boundaries of class **K** might well be fuzzy.[31] Or, more precisely, if we adhere to supervaluationism as the theory of vagueness (cf. Fine 1975 and Keefe 2000), then the vagueness of permissibility entails that, instead of a single class **K**, we need to consider several such classes. Each of them is an admissible precisification of the class of permissible doxastic states. Probability judgments are true (false) if they are true (false) on all such precisifications; otherwise they are indeterminate – neither true nor false.

In the presence of several admissible precisifications of **K**, it might be indeterminate for some propositions what probability relation obtains between them. For example, on one precisification, two propositions A and B might be commensurable probabilitywise, while on another they might not.

To give a simple example, suppose there are two admissible precisifications, $\mathbf{K}_1$ and $\mathbf{K}_2$, where $\mathbf{K}_2$ is a proper superset of $\mathbf{K}_2$. In $\mathbf{K}_1$, A is given more credence than B in every doxastic state, while in $\mathbf{K}_2$ we add to the states in $\mathbf{K}_1$ some doxastic states in which B is given more credence than A. Under these circumstances, A is more probable than B on precisification $\mathbf{K}_1$

[30] Needless to say, this is also the way vagueness can be injected into the intersection model of value relations. It night be indeterminate which preference orderings are permissible. Cf. Rabinowicz (2009b).

[31] There might also be another route to vagueness: It might be argued that doxastic states aren't sharply delimited. Thus, some permissible doxastic states might be 'fuzzy' sets of credence functions. However, to the extent that we are interested in the probability judgments and not in judgments about particular doxastic states, this possibility boils down to the possibility that the boundaries of class K are fuzzy. Yet another route to vagueness in probability judgments might have to do with their objects. If propositions aren't sharply delimited entities, this vagueness will infect probability judgments that concern the propositions in question. In what follows, however, I disregard this and other potential sources of vagueness in probability assessments and focus on indeterminacies concerning the boundaries of the class of permissible doxastic states.

but not on $\mathbf{K}_2$. Indeed, on $\mathbf{K}_2$, *A* and *B* are on a par probabilitywise. Consequently, since both $\mathbf{K}_1$ and $\mathbf{K}_2$ are admissible, it is indeterminate whether *A* is incommensurable with *B*. On the other hand, suppose that there is some proposition *D*, such that every precisification of the class of permissible doxastic states includes both some states in which *A* is given more credence than *D* and some states in which *D* is given more credence than *A*. (To take a simple case, suppose that there are some credence functions *C* and *C'* such that $C(A) > C(D)$, $C'(A) < C'(D)$ and every admissible precisification contains some states *S* and S' that contain *C* and *C'*, respectively.) Then *A* and *D* are determinately incommensurable. Indeed, they are determinately on a par.

Probabilistic indeterminacy and probabilistic incommensurability are different and mutually independent phenomena. We might have (i) a model without indeterminacies in which we have incommensurable propositions. Or (ii) a model with indeterminacies but without incommensurabilities. In the latter, for some propositions *A* and *B*, it might be indeterminate whether *A* is more probable than *B*, less probable than *B* or equally as probable as *B*, even though it is determinate that one of these possibilities obtains, i.e. it is determinate that *A* and *B* are commensurable. Here are two simple examples that illustrate these possibilities:

Consider three credence functions, C_1, C_2 and C_3. Suppose that $C_1(A) > C_1(B)$, $C_2(A) < C_2(B)$, and $C_3(A) = C_3(B)$. Now, we can distinguish between two models:

Model 1. There is only one admissible precisification of the class of permissible doxastic states: $\mathbf{K} = \{\{C_1\}, \{C_2\}, \{C_3\}\}$. In this model, there is no indeterminacy, but *A* and *B* are incommensurable probability wise; they are on a par.

Model 2. The class of permissible doxastic states admits of three admissible precisifications: $\mathbf{K}_1= \{\{C_1\}\}$, $\mathbf{K}_2= \{\{C_2\}\}$, $\mathbf{K}_3= \{\{C_3\}\}$. It is indeterminate whether *A* is more probable than *B*, less probable than *B* or equally as probable as *B*. It is determinate, however, that one of these possibilities obtains, i.e., it is determinate that *A* and *B* are commensurable. Indeed, in this model, which both satisfies Uniqueness and disallows credence gaps, there are no incommensurabilities.

On our approach, incommensurability and vagueness are represented by set-theoretical constructions: Incommensurability is made possible by allowing **K** to consist of several permissible doxastic states and/or by allowing doxastic states to consist of several credence functions. Vagueness is made possible by allowing several admissible precisifications of class **K**. One might thus be tempted to look upon incommensurability and vagueness as closely related phenomena. Perhaps, as an anonymous referee has suggested, incommensurability is "first-order vagueness" – imprecision or indeterminacy *within* **K** - while what I call vagueness is "second-order vagueness" – imprecision or indeterminacy with regard to **K** *as such*.

I agree that it might be in some ways instructive to use terms such as "imprecision" or "indeterminacy" in this liberal way (I am less sure about "vagueness"), as long as one continues to draw a clear distinction between semantic indeterminacy, i.e. the lack of truth value (which is what I refer to when I speak of indeterminacy and vagueness), and 'ontological' indeterminacy (which is what I call incommensurability), i.e. the non-obtaining of the three standard comparative relations (more probable, less probable, equally probable). If *A* and *B* are incommensurable, then it is positively false – and not merely neither true or false – that *A* is more probable than *B*, it is false that *A* is less probable than *B*, and it is false that *A* and *B* are equally probable.

Let me now take up another worry. One might think that if our model already allows for incommensurability, then adding vagueness to the model, on top of incommensurability, seems unnecessary. Isn't it too much of a good thing (or rather too much of a bad thing, if one dislikes imprecision)? I believe, however, that such an addition might be well-motivated. The reason is that the borderlines of incommensurability areas might be vague.

Let me explain what I have in mind. Suppose there are two propositions, *A* and *B*, that are determinately incommensurable in their probabilities. Then it should be possible to construct a finite sequence of propositions, starting with *B*, in which every proposition is slightly more probable than its immediate predecessor. Suppose that the sequence continues until it reaches propositions that are determinately more probable than *A*. The latter propositions are of course determinately commensurable with *A*.[32] It may well be the case, I take it, that there is no proposition B_i in the sequence such that B_i is determinately incommensurable with *A* while the next proposition, B_{i+1}, is determinately more probable and thus determinately commensurable with *A*. Instead, the transition from determinate incommensurability to

[32] To illustrate, suppose that *A* and *B* are, respectively, propositions It will rain and It won't rain in Keynes's example, in which "the barometer is high, but the clouds are black" (Keynes 1921, p. 31). Now, consider a fair lottery with 1000 tickets, of which exactly one is going to win. Let B_0 = *B* = It won't rain, B_1 = It won't rain or ticket 1 will win, B_2 = It won't rain or one of the tickets 1 and 2 will win, .., B_{1000} = It won't rain or one of the tickets 1, 2, …, 1000 will win. We thus construct the relevant sequence by disjoining the original proposition *B* with propositions unrelated to *B* in such a way as to increase the probability of the disjunction by small steps all the way up to 1. Admittedly, it is a very artificial construction, but it should do for my purposes.

determinate commensurability might be mediated by propositions for which it is indeterminate whether they still are incommensurable with *A* or already more probable than *A*. If this is correct, then we may of course expect a similar area of indeterminacy when we instead construct a finite sequence, starting with *B*, in which every proposition is slightly *less* probable than its immediate predecessor and which sooner or later reaches propositions that are determinately less probable than *A*.[33] We may expect that such a sequence will contain propositions for which it is indeterminate whether they still are incommensurable with *A* or already less probable than *A*.

If the above argument is plausible, then allowing for incommensurabilities creates a need for vagueness.[34] The overall map of different but related concepts that come into play when we try to model probability relations is thus quite complex. But I do hope that at least some light has been shed by this paper on how these different concepts interact with each other.

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[33] To illustrate, consider the sequence $B_0 = B =$ <u>It won't rain</u>, $B_1 =$ <u>It won't rain and ticket 1 won't win</u>, $B_2 =$ <u>It won't rain and none of the tickets 1 and 2 will win</u>, .., $B_{1000} =$ <u>It won't rain and none of the tickets 1, 2, …, 1000 will win</u>.

[34] But not vice versa, though. Allowing for probabilistic vagueness does not as such create a need for incommensurabilities. Introduction of incomensurabilities must be argued for independently.

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